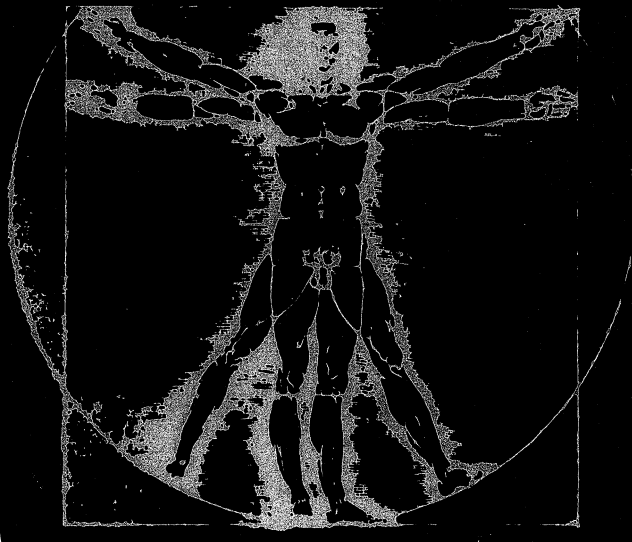
IL GRANDE GRIDO



ETHICAL PROBE ON EINSTEIN'S FOLLOWERS IN THE U.S.A. *–An Insider's View*



Ruggero Maria Santilli

A CONSPIRACY IN THE U. S. ACADEMIC–GOVERNMENTAL COMPLEX ON EINSTEIN'S RELATIVITIES?



In a witty and anectodic style, IL GRANDE GRIDO provides a relentless probe into the scientific ethics and accountability of individual scientists, administrators and officers at several universities, governmental agencies and professional associations.

Some of the questions raised by IL GRANDE GRIDO:

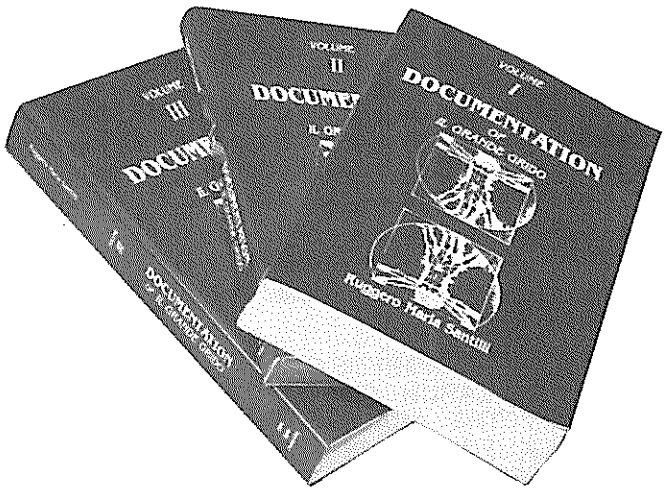
- HOW MANY HUNDREDS OF MILLIONS OF FEDERAL RESEARCH CONTRACTS WOULD BE LOST BY THE ACADEMIC–GOVERNMENTAL COMPLEX FROM THE POSSIBLE INVALIDATION OF EINSTEIN'S RELATIVITIES?
- WHAT ARE THE RISKS FOR THE SECURITY OF THE U.S.A. WHICH WOULD RESULT FROM MANIPULATIONS OF BASIC PHYSICAL KNOWLEDGE PERPETRATED BY LEADING PHYSICISTS AT LEADING INSTITUTIONS?
- WHAT ARE THE RESPONSIBILITIES OF INDIVIDUAL PHYSICISTS, COLLEGE ADMINISTRATORS AND GOVERNMENTAL OFFICERS?

DOCUMENTATION

This book depicts real facts. All names of individuals, institutions, academic societies and governmental agencies are real.

The documentation regarding all factual statements has been collected into three separate volumes comprising copies of: letters written and/or received by the authors; referees' reports from physical journals and Governmental Agencies; official documents; newspaper clips; and any other documentation corroborating statements made in *IL GRANDE GRIDO.*

The DOCUMENTATION discloses only the names of individuals and/or institutions probed in *IL GRANDE GRIDO* for aspects pertaining to scientific ethics. The names of all other individuals and/or institutions have been deleted.



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Ruggero Maria Santilli

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FOREWORD

Dear Fellow Taxpayer,

I am an Italian physicist who, back in 1967, decided to follow the footsteps of Enrico Fermi, and left Italy to place his best energies and capabilities at the service of America.

At the time of this decision, I was unaware of the fact that scientific ethics in the U. S. physics community had declined since Fermi's time. Following my arrival here, I have observed and experienced a further deterioration of scientific ethics. A series of more recent episodes has created my conviction that it is time for the U. S. society to confront and contain the problem of scientific ethics in physics. In fact, the lack of vigilance on ethical issues may well constitute a threat to our free societies.

In this book, I present my case to the best of my recollection and documentation. In Chapter 1, I present the background scientific issues in a way as understandable to the general audience as possible. In Chapter 2, I review my personal experiences with primary U. S. Universities, Federal Laboratories, Journals of the American Physical Society, and Governmental Agencies. Finally, in Chapter 3, I pass to the constructive part, the submission of a number of recommendations aimed at containing ethical problems in physics.

This book has been conceived and written for you, fellow taxpayer, wherever you are. In fact, when (and only when) ethical problems have been brought to the attention of the general public, the U. S. have proven the capability of undertaking all the necessary corrective measures in a way unmatched by other Countries. Lacking sufficient exposure to the general public, ethical problems remain generally ignored, as we all know well.

I am confident that all necessary or otherwise possible, corrective measures will be undertaken also for the problem of ethics in physics as soon as it is sufficiently exposed to the general public. This book brings the reader through a dark tunnel only because at the end I see light.

My task is that of providing you with sufficient information on the problem as well as on its implications for our societies. The decision regarding possible corrective measures is yours. The initiation of the distribution of this book signals the completion of my duty. Its continuation, if any, is now in your hands, fellow taxpayer.

I should also indicate from the outset that the problem

of scientific ethics considered in this book does not refer to stealing money, or the like.

The problem is instead of much more insidious nature and consists of manipulatory practices on truly fundamental physical issues perpetrated by overlapping rings of academic–financial–ethnic interests in the highest levels of the physics community. As such, the problem is potentially much more damaging to society than ordinary crime, as I hope to indicate in detail throughout this presentation.

Needless to say, the problem may well be of global nature and not only localized in the U.S.A. [in fact, in one of the appendices I present comments regarding scientific ethics at the largest european physics laboratory, the C.E.R.N. in Geneva, Switzerland]. Nevertheless, I pay taxes in the U.S.A. and, thus, I shall be primarily concerned with the U.S. profile. The problem of scientific ethics in other Countries is the concern of the taxpayer in those Countries.

I had several motivations for undertaking this rather unpleasant and uneasy task. They grew in time to such a point to render the completion of this book unavoidable, at whatever personal cost.

The first motivation originated from my children. I am now the father of two American children. My silence would have made me an accomplice in unethical practices at the foundation of physical knowledge which, as such, constitutes a threat to my children's future.

The second motivation originated from my fellow scientists scattered throughout the world. Even though their interest toward America has declined considerably in recent decades, as well known, they still dream in considerable numbers of following, like myself, the footsteps of Enrico Fermi. I felt a duty of telling them my story, so that they can have a true account of what it really means attempting to become a member of the contemporary U. S. academic community (and what are the implications for their families), particularly if they have creativity and independence of thought.

In short, I felt obliged to illustrate that, in my personal view and experience, under the deceptive vest of democratic peer review, the current U.S. academic community in physics is a most totalitarian (and internationally powerful) scientific organization which imposes a most questionable form of slavery, that of the human mind; the whole thing accomplished with our money, fellow taxpayer!

The third and perhaps most important motivation originated from my love for this beautiful Country. I would like to differentiate here my distrust of the U.S. academia from my love and respect for America, to which I have dedicated the best years of my life. At any rate, facts speak for themselves, by illustrating

that, while the U.S. physics community has been hostile to me, America has been quite generous indeed.

At a deeper analysis, this book is the best form of appreciation I can provide the U.S.A. Rather than being weakened, the U.S. society can emerge stronger from a moment of critical examination of one of its most vital structures, the free pursuit of novel physical knowledge.

At any rate, I could not have possibly remained in the U.S.A. while silently watching its scientific future being jeopardized by rather unprecedented extremes of scientific–academic–ethnic greed.

Owing to its riches, this Country can well afford paying $ 1,000.00 for a military gasket that is normally worth $ 1.00 on the commercial market. But insufficient vigilance or excessive leniency on the ethics of basic research may well prove to be self–destructing.

A few additional, introductory comments may be of value for the appropriate perspective in the reading of this presentation.

During my European studies, from the elementary school up to the graduate school in theoretical physics, I had to study a number of ancient and contemporary languages. Nevertheless, whether you believe it or not, I never sat in an English class. I learned English by studying papers and books in mathematics and physics.

As of now, I have written a number of papers and monographs in English, but they are all of technical nature, and, as such, with emphasis on mathematical–physical elaboration and with the language reduced to an absolute minimum.

This work, instead, demands a literary knowledge of the English language which I simply do not have. The book is therefore written in "broken English", as I know well. At any rate, I see no need for linguistic perfection to convey the desired message, and for this reason I have absteined from the use of professional English editors.

Also, the language I have selected is as crude as possible. I have also eliminated in the final version of the manuscript all those calls to history, literature and art that render pleasant the reading of a book. The reasons are obvious. This books deals with seemingly dishonest episodes perpetrated by academicians. The matching of these episodes with historical, literary or artistics calls would have been offensive to the latters.

All names of individuals and institutions appearing in this book are real. The fact described are also real to the best of my recollection and documentation. Only the names of the innocent and of the victims of manipulatory academic practices have been withheld and are indicated with capital letters (such as A.A.A., B.B.B., etc.).

All statements of Chapter 2 are documented to my best.

Such documentation, being rather large, has been collected in three separate volumes.

If some of my statements are incorrect or erroneous, I beg the interested reader to provide me with the contrary evidence. I shall than take all necessary corrective measures, beginning with all needed apologies.

Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138, U.S.A.

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CHAPTER 1

THE SCIENTIFIC CASE

1.1: THE LIMITATIONS OF EINSTEIN'S IDEAS IN FACE OF THE COMPLEXITY OF THE UNIVERSE.

The existing scientific literature contains a considerable number of theoretical, experimental and mathematical elements according to which:

1) **Einstein's special relativity is exactly valid for particles which can be effectively approximated as being point–like while moving in empty space conceived as a homogeneous and isotropic medium.**

This is the arena of the original conception of the special relativity, as clearly expressed by Einstein himself in his limpid writings.

Typical examples of exact validity of the special relativity are given by the peripheral electrons of the atomic structure, or by electrons and protons moving in particle accelerators;

2) **Einstein's special relativity is only approximately valid (that is, strictly speaking it is violated) for extended particles/wave–packets under the short range interactions responsible for the nuclear structure, called strong interactions.**

Evidently, these physical conditions are broader than those of the original conception. Rather than being diminished by the advancement of physical knowledge, the stature of Albert Einstein is therefore magnified by the physical intuition and scientific honesty that led him to state as clearly as possible the physical arena of applicability of his ideas.

Thus, according to the information under consideration, the special relativity is exact for the motion of the center–of–mass of a proton in a particle accelerator, but the same relativity is expected to be violated in the interior of the proton itself, or when the same proton exits the particle accelerator, and enters

within the intense, short range, force fields in the vicinity of a nucleus.

3) **Einstein's general theory of gravitation is intrinsically erroneous and incompatible with nature.**

Thus, while the special relativity may still be considered as approximately valid in the interior of a hadron, the information under consideration excludes even the approximate character of the general relativity because of a number of inconsistencies we shall review in Section 1.5.

The historical roots of the limitations.

Let me say from the outset that the above elements are not of my own invention. In fact, they have been known in academic circles before I initiated any research activity.

As a matter of fact, most of the scientific scene characterized by points 1), 2), and 3) above reached me when I was a high school student in a small, but fascinatingly beautiful town in the Appennines, renouned for its schools and called the "Athens of the Sannium" (the town is Agnone in the Province of Isernia, Italy).

The information of the unsettled character of Einstein's ideas reached my high school mind with an impact that I still remember, because of the credibility of its authors. For instance, I still remember vividly when in the 50's I read the passage in the "Lecture Notes in Nuclear Physics" by Enrico Fermi [1], who stated, when referring to the nuclear forces and their range (which is of the order of 10^{-13}cm = 1 Fermi),

". . . .there are doubts as to whether the usual concepts of geometry hold for such region of space."

My high school knowledge of geometry was sufficient to see that doubts on conventional geometries necessarily imply doubts on Einstein's special relativity, owing to the deep interplay between geometry and dynamics identifiable already at the level of high school courses.

I subsequently discovered that the literature on the limitations of Einstein's relativities was rather vast. In fact, the limitations could be often traced back to names in the history of physics, and carry names such as the legacies of Langrange, Hamilton, Liouville, Jordan, Pauli, Fermi, Cartan, and others, as we shall see.

My lifelong programs of study and research.

The information was sufficient to create an uncontainable interest in this truly fundamental problem of contemporary know-

ledge. I therefore decided to become a physicist and to devote my life to the study of the issue. For this purpose, I resolved myself, first, to reach in Italy the most advanced possible technical preparation in pure and applied mathematics and in theoretical physics, and then move to the U.S.A. for the actuation of my research program.

I did complete the first part of my program, by obtaining in 1966 the (Italian equivalent of the) Ph. D. in theoretical physics at the University of Turin. I did move to the U.S.A. soon thereafter. But in over sixteen years of attempts, I have been able to realize my research program only minimally, despite efforts to the limit of my capabilities.

The hostility I have encountered in the U.S. physics community.

This book is, in essence, a report on the rather extreme hostility I have encountered in U.S. academic circles in the conduction, organization and promotion of quantitative, theoretical, mathematical, and experimental studies on the apparent insufficiencies of Einstein's ideas in face of an ever growing scientific knowledge.

The hostility originated within vested, academic–financial–ethnic interests who apparently oppose the conduction of the studies for the sole pursuit of personal gains, in disrespect for the interests of America, as well as of the society at large.

In this chapter, I shall summarize the state of the art of Problem 1), 2) and 3) above. My personal experiences will be reported in Chapter 2.

The nontechnical character of this presentation.

I must stress that, under no circumstance this presentation can be considered as technical. It is a mere indication of the essential ideas which, as such, should be understandable to all.

I shall however indicate some of the technical literature to permit the interested, but yet uninformed scientist to acquire the necessary knowledge for ethically sound judgments. The quotation of relevant literature is also necessary to minimize the not unfrequent venturing of judgments by mumbo–jambo pseudo–scientists without technical knowledge of the background issues. In this way, physicists expressing their opinions can be subjected to a judgment of their technical knowledge and qualifications for this rather specialized field.

The technical literature directly or indirectly related to the problem considered is quite vast, and estimated to exceed the mark of 10,000 pages of printed research. My list of technical references cannot but be partial, and the interested colleague must do what all others in the field have done: spend several

years of library search and study of the most advanced possible, relevant literature.

The unsettled character of available studies.

Despite their size, the available studies are inconclusive at this time. That is, we do not have conclusive evidence to claim that Einstein's special relativity is violated under strong interactions, and that the general relativity is incompatible with nature. We merely have a number of serious and authoritative reasons of doubts.

It should be stressed that the opposite view is also in the same situation. That is, we do not have at this time conclusive evidence that the special and general relativities are exactly valid. We merely have indications of validity.

In short, the scientific case underlying this book is, without any doubt, the most fundamental, basically unresolved problem of contemporary physics. The hostility I have encountered in academic circles appeared to be intended to suppress or otherwise jeopardize quantitative theoretical, mathematical, and experimental studies. These hostilities were perpetrated by renowned scholars, for the apparent purpose of preventing the achievement of progress in the field.

It is hoped that coordinated research on the limitations of Einstein's ideas will indeed be properly funded, and conducted as soon as the information on the currently deprecable state of research in the field has reached the general public.

An illustration of the direct implications for you, fellow taxpayer, of the problem of validity or invalidity of Einstein's ideas: the controlled fusion.

The historical dispute between Galilei and the Catholic Church whether or not our Earth is moving,had no practical implications for the people of that time. In fact, it took centuries of developments of the seeds planted by Galilei to reach technological applications.

The situation nowadays is fundamentally different than that at Galilei's times. In fact, the problem of the validity or invalidity of Einstein's ideas for strong interactions has direct implications for all our lives, as well as the lives of our children.

Einstein's ideas are the true, ultimate foundations of contemporary physics. Studies on their limitations, and possible generalizations may therefore have such scientific, economic and military implications as to dwarf most of the research currently preferred by leading academicians, and therefore funded by governmental agencies.

As a preliminary illustration of the implications of Einstein's ideas, consider the current efforts to achieve the con-

trolled fusion, that is, the laboratory production of bound states of protons and neutrons under controlled conditions with a positive energy output.

It is evident that the characteristics of protons and neutrons play a fundamental role in a problem of this nature. For instance, one of the aspects currently studied is magnetic confinement of the plasma of particles. In turn, such confinement is evidently dependent on the values of the intrinsic magnetic moments of the particles.

Now, Einstein's special relativity characterizes the proton and the neutron as massive points. But points, being dimensionless, cannot be deformed. This implies the constant character of the intrinsic characteristics for the particles. It follows that, according to Einstein's special relativity, the values of the intrinsic magnetic moments of the protons and neutrons under the conditions of the controlled fusion are the same as those under other physical conditions (say, of electromagnetic type).

But, according to incontrovertible experimental evidence, the proton and the neutron have a charge distribution which is extended in space and whose dimension is of the order of one Fermi. The assumption of the extended character of the particles evidently implies the possibility of deformations under sufficiently intense external fields and/or collisions. In turn, deformations of the charge distribution are known (from classical electrodynamics) to imply an alteration of the value of the magnetic moments. Quantitative studies have indicated (see Section 1.6) that about 1% deformation of shape can imply 50% and more alteration of the value of the magnetic moments.

But the conditions of the controlled fusion are similar to those considered here. We therefore see the possibility that the intrinsic magnetic moments of protons and neutrons (as well as other characteristics) may change when the particles perform the transition from long range electromagnetic interactions (as experimentally detected until now) to the conditions of the controlled fusion. In turn, such alterations would have far reaching implications for the achievement of magnetic confinement and for other aspects of the controlled fusion, beginning with the engineering design of the magnetic bottle, let alone theoretical considerations.

The implications of Einstein's special relativity for the controlled fusion are now identifiable. If the theory is assumed to be strictly valid under strong interactions, as currently believed in leading academic circles, the protons and neutrons preserve all their intrinsic characteristics under the fusion conditions. If these characteristics are instead altered, a suitable generalization of the special relativity is unavoidable, as we shall see.

To put it bluntly, possible deviations from the special relativity under strong interactions may have a crucial role for the

achievement of controlled fusion. At the extreme, a number of scholars (including myself) believe that the insistence on the strict validity of the special relativity under strong interactions may well prevent the achievement of the controlled fusion.

Some preliminary elements on academic interests suffocating at birth certain undesired experimental resolutions.

I should stress here that the hypothesis of the possible alteration of magnetic moments under nuclear conditions is not mine. In fact, it was conceived in the early stages of the theory as one possibility to interpret the total nuclear magnetic moments (which are still far from being understood despite over half a century of research).

In fact, in book [2] in nuclear physics by Blatt and Weisskopf, one can read on p. 31: "It is possible that the intrinsic magnetism of a nucleon [i.e., a proton or a neutron] is different when it is in close proximity to another nucleon." Similar statements can also be found in other well written early treateses in nuclear physics, such as that by Segrè [3].

Subsequently, studies of the hypothesis were reduced up to the current status of virtual complete silence in the technical literature, despite its manifest plausibility and its equally evident, rather large implications of scientific as well as societal character.

The reasons for such an unusual occurrence are known in academic corridors, but unspoken. They are due to the fact that, alterations of magnetic moments generally imply deviations from Einstein's special relativity because they are due to deformations of the charge distribution. In turn, such deformations generally imply the breaking of a central component of Einstein's special relativity, the symmetry under rotations.

The understandability of fundamental physical issues, with consequential capability by the taxpayer to identify manipulatory academic practices.

In short, one does not need a Ph. D. in theoretical physics to understand the essential physical ideas, and therefore appraise possible underground academic manipulations. In fact, everybody can see that a spherical charge distribution deformed by collisions and—or external fields is no longer rotationally invariant. This deformation is the fundamental physical point here. The alteration of the magnetic moments, on one side, and the violation of Einstein's special relativity, on the other side, are mere technical consequences.

In my view, the reason why no significant research on the hypothesis has been conducted, despite its manifest plausibility and evident relevance, is that its primary implication (the possible invalidation of Einstein's special relativity under strong inter-

actions) is damaging to the vested, academic–financial–ethnic interests currently controlling the U.S. physics. In fact, after several years of efforts, I have encountered nothing but hostility and interferences in the study of the hypothesis, by exhausting all possible avenues for an orderly conduction of the needed research. At any rate, the lack of cooperation by Victor F. Weisskopf and his associates at the Massachusetts Institute of Technology is established beyond reasonable doubt, as reported in Section 2.2.

To this writing, the exact validity of Einstein's special relativity under strong interactions continues to be imposed in the typical way of all totalitarian regimes, via shear power of authority and the suppression, dismissal or disqualification of dissident views, in fundamental disrespect of the most elemental human and scientific values.

Silence as complicity in scientific crimes.

I hope, fellow taxpayer, you begin to see the tip of the iceberg that forced me to bring the situation directly to your attention.

I am sincerely convinced that the continuation of the current academic status on Einstein's ideas has such scientific, economic and military implications for our free societies to qualify silence as complicity in manipulating fundamental human knowledge, that is, complicity on scientific misconduits.

It is time to identify publicly the responsible academicians, administrators and governmental officers and expose them to the societal judgment.

1.2: THEORETICAL, MATHEMATICAL, AND EXPERIMENTAL MEANS TO ASSESS EINSTEIN'S IDEAS

Predictably, no scientifically meaningful assessment of Einstein's ideas can be conducted without a comprehensive analysis encompassing theoretical, mathematical and experimental aspects. This is due to the fundamental role of the ideas in all these aspects.

The fundamental character of Galilei's relativity for an appraisal of Einstein's ideas.

As soon as the consideration of a research program of this nature is initiated, one sees that the analysis cannot be limited to Einstein's ideas per se, but must initiate at the level of their own foundations, Galilei's relativity, that is, the relativity for point–like particles moving in vacuum at speeds that are small when compared to that of light.

In fact, Einstein's special relativity essentially generalizes Galilei's relativity to speed of the order of that of light. Einstein's theory of gravitation considers a further generalization, this time of geometric nature, via the transition from a flat to a curved space, but always in such a way to seek compatibility with the Galilean relativity.

As a result of this long historical process, the Galilean, the special and the general relativities have emerged to be deeply inter-related and mutually compatible. It then follows that the identification of insufficiencies of Galilei's relativity necessarily implies, for consistency, the existence of corresponding insufficiencies at the level of the special and of the general theory. Viceversa, insufficiencies independently identified at the levels of the special and/or of the general relativity must admit, also for consistency, corresponding, physically meaningful insufficiencies at the level of the Galilean relativity.

The need for classical and quantum mechanical studies of the problem.

At a deeper analysis, one can see that the entire process of critical examination of the Galilean–special–general relativities must be repeated twice, the first time for the classical description of our macroscopic environment, and the second time for the quantum mechanical counterpart at the level of particle physics.

The important point is that the scientific process can be initiated at the level of the physical reality of our environment. It is this ultimate origin that renders the expected limitations of Einstein's ideas, understandable by the general audience, without any need of graduate studies in theoretical physics.

It should be stressed that, even though the classical–macroscopic framework remains fundamental on conceptual [as well as technical] grounds, the quantum mechanical analysis is particularly important for the experimental resolution of the issues. In fact, most of the experiments needed to test Einstein's ideas call for beams of particles and other means that are typical of quantum mechanics.

As a first indication of the vastity, complexity and diversification of the problem considered, we can therefore say that scientifically meaningful assessments of the Galilean–special–general relativities can be done only upon completing the analysis at both levels, the classical and the quantum mechanical one.

The need for a vast program of research in pure mathematics.

As soon as a research program of this nature is initiated one can see a host of rather fundamental implications at the level of pure mathematics.

As an indication, Galilei's relativity is a manifestation of a certain type of algebras [the Lie algebra] and of a certain type of geometries [the symplectic geometry]. No advance on the physical issues is possible without the identification of the needed generalization of these mathematical tools.

To put it differently, the identification of insufficiencies of Galilei's relativity essentially means the identification of physical systems and conditions broader than those permitted by Galilei's relativity. But then, no physically meaningful elaboration of these broader systems can be conducted without a corresponding generalization of the underlying mathematics.

The dual analysis of the Galilean—special—general relativities at the classical and quantum levels, must therefore be complemented by a rather vast [and truly intriguing] program of research at the pure mathematical level. Only in this way, the physicist is provided with the rigorous mathematical tools needed for quantitative treatments.

The manifest need for a comprehensive experimental program.

Needless to say, the above theoretical and mathematical studies must be completed by a comprehensive experimental program. In fact, the only way final conclusions are reached in physics is the experimental way.

In particular, the experimental program cannot be limited to the formulation of suitable experiments that are feasible in currently available laboratories, and implies much more profound issues.

At this point, the fellow taxpayer is encouraged to meditate a moment on the fact that contemporary physical experiments no longer have the dials for visual measurements used up to the early part of this century. Today, a particle experiment is run, in its physical part, say, in an underground tunnel in Illinois; the information is fed into a computer, say, in Long Island; and the elaboration of the data is conducted, say, by a team in Berkeley, Cambridge and Paris.

Thus, the experimental program needed to achieve the future resolution of the validity or invalidity of Einstein's ideas is per se, highly complex and diversified, as well as deeply dependent on the preceding theoretical and mathematical research.

First, there is the need to formulate direct experiments on

the exact or approximate validity of the special relativity under strong interactions [Section 1.1]. By recalling that virtually all contemporary measures in particle physics are done via external electromagnetic interactions, one can see the need of a new generation of experiments, those capable of achieving direct measures under external strong interactions.

But this is not all. The experimental data are today elaborated via the use of theoretical tools that, in general, are dependent on Einstein's ideas in a truly essential way, as it is typically the case for contemporary high energy scattering experiments. It is then evident that the "experimental results" cannot be claimed as providing final evidence on the basic assumptions. In fact, if these assumptions are changed or modified, the numbers expressing the "experimental results" change, as already shown in the technical literature [See, later on, Section 1.7]. Jointly with the formulation of new, direct experiments, there is therefore the need to re–examine the very ways in which experiments are conducted these days and the "experimental results" claimed.

In conclusion, the research on the assessment of Einstein's ideas soon becomes so technically involved on all fronts, to be not only beyond this presentation for the general public, but also beyond professional physicists and mathematicians without a specific expertise in the field.

The need for a strict definition of experts in the problem of the validity or invalidity of Einstein's ideas.

In closing this section, permit me to warn the fellow taxpayer against false experts, no matter how renowned their academic affiliations are, whenever facing judgments on the scientific topic of this book.

Recall that "experts" in a given physical or mathematical field are individuals who have published in refereed journals at least a few papers, specifically, in the field considered. Thus, to qualify as "experts" on the possible insufficiencies of Einstein's ideas, physicists and mathematicians must have published at least some papers in the field considered.

If a guy has published even a large number of papers on Einstein's ideas, but without a mention of their expected insufficiencies and limitations, that guy does not qualify as "expert" in the topic of this chapter.

Besides, if a guy has published several papers on the validity of Einstein's ideas, he/she has manifest, vested, interests in their validity. As such, that guy is the very least qualified for expressing objective judgments on the limitations of the ideas.

The need to insist in requiring proofs of qualifications to

all physicists expressing judgment in the field.

At the risk of being pedantic and repetitive on this important point, I must urge the fellow taxpayer most warmly to ask the documentation of qualification of expertise to anybody expressing judgment on the topics of this chapter that is, to ask not only the references to published articles, but most importantly, the indication of the specific passages where the expected limitations of Einstein's ideas are explicitly presented and analyzed. Lacking these latter essential elements of qualification, judgments may well be a powdery mask for inepts, no matter how high the scholar is on the academic ladder.

It is hoped that the fellow taxpayer can acquire in this way the necessary elements to distinguish between ethically and scientifically sound scientists, who generally express cautious views, and dishonest academic barons, who usually venture judgments because of academic—financial—ethnic motivations, without any documented expertise in the field, and in total disrespect of the pursuit of novel scientific knowledge.

1.3: THE AGING OF GALILEI'S RELATIVITY IN CLASSICAL MECHANICS

As a result of a scientific process initiated with Galilei's *Dialogus de Systemate Mundi* of 1638 [4] and then continued by Newton [5] and other founders of contemporary science, we have reached the rather sophisticated, current formulation of Galilei's relativity in classical, Newtonian, mechanics (see, for instance, ref.s [6,7]). Nevertheless, the ultimate physical foundations remain those of centuries ago, the description of the dynamical evolution of massive points.

An arena of unequivocal applicability of Galilei's relativity in classical mechanics.

In fact, Galilei's relativity describes systems of particles which

1) can be effectively approximated as being point—like (that is, without space dimension);

2) move in vacuum (empty space) assumed to be homogeneous and isotropic; and

3) are such that relativistic, gravitational, and quantum mechanical effects are ignorable (that is, the speeds are much smaller than that of light; the space has null curvature; and the masses of the objects are such to render ignorable effects due to their individual particle constituents).

An illustration of the physical arena of applicability of Galilei's relativity is given by our solar system in Newtonian approximation, in which the sun, our earth and all planets and satellites are approximated as massive points (see Figure 1.3.1).

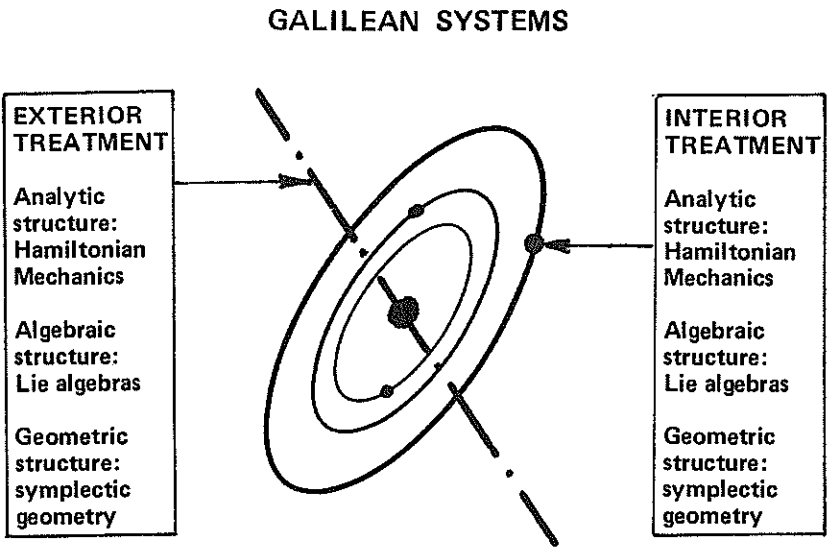


Figure 1.3.1. A schematic view of a system characterized by Galilei's relativity, the solar system in Newtonian approximation which verifies conditions 1), 2) and 3) of the text. The relativity describes not only the center–of–mass of the solar system in its evolution within our galaxy, but also the dynamical evolution of each constituent. The description is achieved via the form invariance of the equations of motion under the so–called Galilean transformations in Euclidean three–dimensional space and time. They are the largest possible set of linear transformations interconnecting inertial reference systems, that is, observers not experiencing accelerations or any external force. The Galilean invariance of the equations of motion leads to ten conservation laws, those of the energy (one), of the total linear momentum (three), of the total angular momentum (three) and the uniform motion of the center–of–mass (three). In this way, physical conservation laws are reduced to primitive, abstract, mathematical laws of invariance under the (Lie) group of Galilean transformations, by achieving a symbiotic reduction of three underlying methodological tools: Hamiltonian mechanics, Lie's theory, and the symplectic geometry.

An arena of inapplicability of Galilei's relativity.

The physical arena characterized by conditions 1), 2) and 3) above also identifies the limitations of the relativity. In fact, Galilei's relativity is unable to provide meaningful treatments of systems of particles which

1') cannot be effectively approximated as being point–like;

2') move in a physical medium (gas, liquids, etc.); and

3') are such that relativistic, gravitational and quantum mechanical effects are ignorable as in 3).

A typical example is given by a satellite during re–entry. As well known, when the satellite orbits around earth in empty space, its actual size and shape do not affect the dynamical evolution. As a result, the satellite can be effectively approximated as a massive point concentrated in the center–of–mass. Galilei's relativity then strictly applies.

However, when the same satellite penetrates the Earth's atmosphere, its actual size and shape affect the dynamical evolution directly. Under these conditions, the satellite cannot any longer be approximated as a massive point and Galilei's relativity becomes inapplicable. In fact, insistence on its applicability would lead to "perpetual motion" types of academic abstractions (such as the orbiting of the satellite within our earth's atmosphere with a conserved angular momentum and consequential lack of decaying of the orbit).

The mathematical roots of the inapplicability.

At a deeper analysis, the insufficiencies originate at the mathematical foundations of the relativity, that is, the analytic, algebraic and geometric methods in their so–called canonical realization. In fact, condition 1) essentially implies a local–differential geometry, that is, a geometry characterizing ordinary differential equations, which are the equations of motion of the centers–of–masses. On the contrary, condition 1') calls for a suitable nonlocal/integral geometry, that is, a geometry yet to be constructed by pure mathematicians (beginning from its topology), which characterizes equations of motion involving not only ordinary local terms for the centers–of–masses, but also integral terms computed on the surface–shape and/or volume of the objects.

A fully similar situation occurs in the transition from condition 2) to 2'). In fact, empty space can be safely assumed

(for all Newtonian approximations) as being homogeneous and isotropic, while the Newtonian time, with its immutable character is evidently isotropic (again, at the Newtonian approximation). It is known that these conditions imply Galilei's relativity. In fact, the homogeneity and isotropy of space imply the exact character of the central part of the Galilei as well as of all relativities, the symmetry under rotations and translations in space. The isotropy of time implies the symmetry under translations in time. Additional technical steps imply the symmetry under the remaining component of the Galilean transformations the so—called velocity transformations (for technical details, one can consult, for instance, ref.s [6,7]). In turn, these symmetries imply the ten celebrated Galilean conservation laws (Figure 1.3.1).

In the transition to condition 2'), motion in physical media, the situation becomes profoundly different. In fact, as everybody knows, physical media such as our atmosphere are not homogeneous or isotropic. This implies the manifest breaking of the symmetry under rotations which, in turn, is a necessary condition for a representation of the decay of the satellite's orbit, that is, for the nonconservation of the angular momentum. Technical arguments then imply the breaking of the entire relativity [10].

In conclusion, conditions 1') and 2') complement each other into the same results, the inapplicability of Galilei's relativity for the broader physical conditions considered, with the consequential need for a suitable generalization.

The process of closing a nonconservative system into an isolated system inclusive of its environment.

The analysis cannot be halted at the level of the satellite. In fact, we must complement the nonconservative satellite with its environment, which has absorbed in various forms its loss of energy, in such a way to reach a closed system, that is, a system whose total energy is conserved. The issue is whether during this process we recover Galilei's relativity, in which case its loss at the constituent level would be of lesser significance.

Inspection of nature soon reveals that in the process of closing a nonconservative system into a broader conservative form inclusive of its environment, the ten total conservation laws for the center—of—mass are recovered, but Galilei's relativity remains inapplicable.

The understanding of this occurrence can be reached by comparing a Galilean system, such as our solar system, with a non—Galilean one, such as our earth, we considered as isolated from the rest of the universe to achieve closure.

In the case of the solar system, the validity of Galilei's relativity originates at the level of its planetary constituent

(Figure 1.3.1). The validity of the relativity for the system as a whole is then consequential.

In the case of our Earth, Galilei's relativity is inapplicable to its constituents, such as a satellite during re—entry, and such inapplicability persists in the transition to the earth as a whole, trivially, because the inapplicability is unaffected by our shifting the observation from the satellite to the center—of—mass motion of the entire earth.

Dynamical origin of the breaking of Galilei's relativity: the contact/nonpotential/nonlocal forces.

Nature therefore indicates, quite forcefully, that the validity of total conservation laws of an isolated system, by no means, necessarily implies the exact validity of Galilei's relativity (as erroneous stated or implied in a number of contemporary books of theoretical physics), because the same laws are admitted also by systems which are intrinsically non—Galilean.

We reach in this nontechnical way the ultimate dynamical foundations of the problem, the nature of the acting forces (or interactions). It is generally assumed that total conservation laws occur because the internal forces are of the so—called conservative type, that is, of action—at—a—distance type derivable from a potential energy. A typical example of a conservative force is the gravitational force responsible of the solar system (in Newtonian approximation).

Non—Galilean systems such as our earth admit instead internal forces that are conceptually, physically and mathematically more general than those of the solar system. They are called of contact type to express the actual, physical, contact among extended objects (these forces are evidently absent for point—like, Galilean particles, trivially, because they have no dimension in space and, thus, they cannot have contact effects). Second, the forces are called of nonpotential type. In fact, the notion of potential energy has no physical basis for them, because of the lack of distance which is essential to define it. Finally, the forces are called of nonlocal type, to express the fact that they do not occur at a point, but rather at a surface or volume, exactly as it is the case for the satellite during re—entry. As a result, the forces are called of contact/nonpotential/nonlocal type [10, 12] or of follower type, particularly in engineering [13]. At a deeper analysis, the forces are also of non—Hamiltonian type, in the sense that they violate the conditions for the applicability of the entire mechanics at the foundation of Galilei's relativity, Hamiltonian mechanics [9].

Once the nature of the forces acting on the satellite during re—entry is understood, the inability to recover Galilei's relativity in the closure of the system into a conservative

form is consequential. In fact, our shifting of the observation from the open–nonconservative satellite to the closed system constituted by the entire earth, leaves the nature of the forces unaffected: the forces are of non–Galilean type prior to closure and remain of non–Galilean type after closure of the system.

The identification of the nature of the acting forces also permits the understanding that the inapplicability of Galilei's relativity originates at the mathematical foundation of the theory. In fact, not only the equations of motion are noninvariant under Galilei's transformations, but the underlying mathematical structures are inapplicable. I am referring here to the inapplicability not only of Hamiltonian mechanics, but more specifically of the Lie algebras and of the symplectic geometry (Figure 1.3.1).

The fundamental notion of closed non–Hamiltonian systems as forcefully established by nature.

We reach in this way a notion which is at the foundation of the studies presented in this chapter, from the Newtonian, to the quantum mechanical ones. I am referring to "closed/non–Hamiltonian systems", that is, systems which, when seen from the outside, verify all conventional conservation laws of total quantities, but their structural equations are of non–Galilean type.

The systems were identified, apparently for the first time, in memoir [14] and then studied by a number of authors (see, e.g., ref. [15]). For a review, the reader may consult monograph [10].

The approximation of the integral forces via power series in the velocities permits the regaining of the locality of the theory, that is, its definition at a set of isolated points. But the Galilean noninvariance as well as the general non–Hamiltonian character persist , all in a way compatible with conventional total conservation laws. This local approximation is also useful to illustrate the mathematical consistency of the theory via readily solvable equations (see Figure 1.3.2 for more details).

Generalization of Galilei's relativity for closed non–Hamiltonian systems.

As a result of a considerable number of contributions in mechanics, algebras and geometries beginning from the past century, a generalization of Galilei's relativity for closed non–Hamiltonian systems has been submitted in ref. [8], and worked out in monographs [9, 10, 11, 12].

The generalized relativity consists of two formulations. The first (tentatively called "Galilei–isotopic relativity"for cer-

NONGALILEAN SYSTEMS

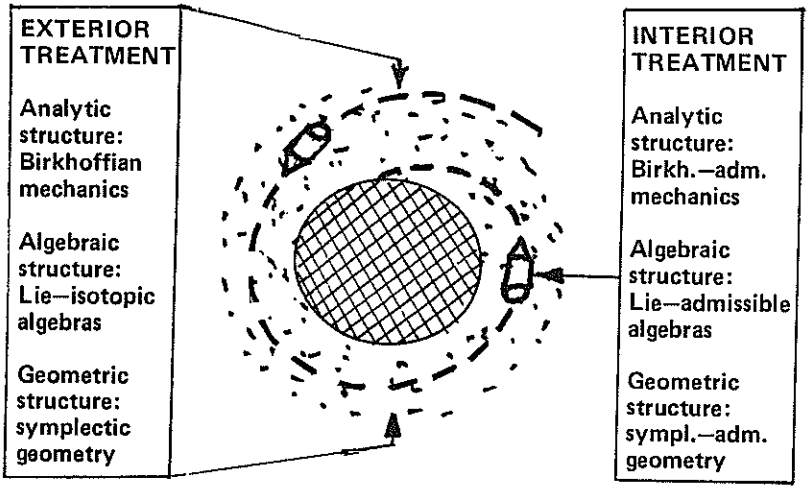


Figure 1.3.2. A schematic view of systems, called closed/non–Hamiltonian, which are outside the technical capability of Galilei's relativity. They are given by systems such as our Earth which, when considered as isolated, verify all total conservation laws of the conventional Galilei's relativity (Figure 1.3.1), but the internal forces violate the conditions for the applicability of the mathematical foundations of the relativity (analytic mechanics, Lie algebras and symplectic geometry in canonical realizations). While Galilei's relativity can treat only systems that are of local and potential nature, the internal forces of non–Galilean systems such as our Earth are of nonlocal/integral and of non–potential/noncanonical/non–Hamiltonian type due to motion of extended objects (such as satellites) moving within material media (such as our atmosphere). The consistency of our mathematical description of closed non–Hamiltonian systems is readily established [10, p. 235]. In fact, the conventional, total conservation laws imply only seven conditions on the internal forces, thus permitting multiple infinities of consistent, non–Hamiltonian equations of motion. Two generalizations of Galilei's relativities have been submitted in ref.s [8, 9, 10, 11, 12] for closed/non–Hamiltonian systems under the approximation of nonlocal internal forces via power series in the velocities, in which case locality is regained, but the nonpotential/non–Hamiltonian character persists. The first generalized relativity (called Galilei–isotopic) is conceived for the exterior treatment [9, 10], while the second (called Galilei–admissible) is conceived for the complementary interior treatment of open–nonconservative constituents [8, 11, 12]. Both generalized relativities are based on the central idea of all relativities, the identification of symmetries for the invariant descriptions of the equations of motion. Nevertheless, the objectives are different for the exterior and the interior case. In the former, the symmetry is used to characterize total conservation laws under non–Hamiltonian internal forces, while in the latter, the symmetry is used to characterize time–

rate—of—variations of physical quantities, the systems being nonconservative by conception. Rather profound conceptual differences also exist between the conventional and the generalized relativities. In the conventional relativity, one assumes the underlying symmetry, Galilei's symmetry, and restricts the systems to verify such symmetry. This attitude generally results in the exclusion of systems of the physical reality, inasmuch as only very few Newtonian systems verify Galilei's relativity. In the generalized Galilei—isotopic relativity, the attitude is reversed, inasmuch as one first assumes the equations of motion in their most general possible form, and then seeks its symmetry according to a method called of Lie—isotopy which is based on the generalization of the unit of Galilei's symmetry, while leaving all other aspects unchanged (see ref. [10] for the general lines and the subsequent ref.s [18, 19] for the detailed techniques). While Galilei's transformations are unique, there exist multiple infinities of Galilei—isotopic transformations because of the multiple infinities of contact/non—Hamiltonian forces (which are represented by the multiple infinities of possible generalized units). Also, while Galilei's transformations are linear, the Galilei—isotopic ones are generally nonlinear (although expressible in a formally linear, isotopic, form which suggested the name of Galilei—isotopic relativity). Finally, while Galilei's transformations connect inertial frames, the Galilei—isotopic transformations connect noninertial frames (recall that inertial frames are a conceptual abstraction and do not exist in the physical world). Despite all these and additional differences, Galilei's and Galilei—isotopic relativities coincide at the level of abstract, coordinate—free, algebraic—geometric formulations, by therefore resulting to be characterized by different realizations of the same abstract mathematical structure. This latter property is truly fundamental for the studies presented in this chapter. In fact, the same situation will be found at the relativistic and quantum mechanical levels.

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tain technical reasons) is conceived for the exterior treatment, in which case the emphasis is on the achievement of conventional, total, conservation laws under non—Galilean internal forces [9, 10].

The second formulation (tentatively called Galilei—admissible relativity) is conceived for the complementary interior treatment of each constituent, such as a satellite during re—entry. In the latter case, the emphasis is in the maximal possible time—rate—of—variations of physical quantities under the most general possible external forces [11, 12].

The underlying generalizations of Hamiltonian mechanics.

The generalizations were permitted by the previous construction of two complementary generalizations of Hamiltonian mechanics for closed and open systems, called Birkhoffian and Birkhoffian—admissible mechanics for certain historical reasons related to ref. [16]. In turn, the two mechanics were permitted by two, progressive generalizations of Lie's theory, the first of the Lie—isotopic type and the second of the more general Lie—

admissible type. The underlying geometry in the former case resulted to be of conventional symplectic type [17], although realized in its most general possible form, while that of the latter case (called symplectic-admissible) is under investigation.

Both generalized mechanics verify the so—called theorems of direct universality, that is, the capability of representing all Newtonian systems considered (universality) in the frame of the experimentalist (direct universality). By comparison, Hamiltonian mechanics is capable of representing in the frame of the observer only a rather small class of Newtonian system.

Also, both the Birkhoffian [10] and the Birkhoffian—admissible mechanics [12] preserve their structure under the most general possible transformations. By comparison, Hamiltonian mechanics preserves its structure only under a special class of transformations (called canonical). In particular, the Birkhoffian—admissible mechanics is a covering of the Birkhoffian mechanics which, in turn, is a covering of the Hamiltonian one.

Status of the studies.

Despite these advances, I must stress that, by no means, the studies are final. In fact, despite the number of independent contributions in several theoretical and mathematical aspects, the studies are essentially at the beginning. Nevertheless, we can claim today:

a) the unequivocal existence in our classical, macroscopic reality of closed—isolated systems whose internal dynamics is beyond Galilei's relativity (such as our Earth);

b) the consistency of our nonlocal mathematical representations as well as of their local approximation via power series in the velocities; and

c) the expectation of the consequential existence of suitable generalizations of Galilei's relativity, for which the generalized relativities submitted in ref.s [8, 9, 10, 11, 12] may be useful working grounds.

Independence of the proposed generalizations of Galilei's relativity from those worked out by Einstein.

Note that the proposed generalizations of Galilei's relativity are basically independent from those worked out by Einstein. In fact, the former generalizations are characterized by structurally broader forces, while the latter generalizations are

characterized by other physical rules, such as relativistic speeds or curvature. This independence has been implied in the preceding analysis by keeping conditions 3) unaltered.

The independence of the generalizations of Galilei's relativity under consideration from the Einsteinian ones is evidently of utmost importance. In fact, it opens up a new, virtually endless, scientific horizon for potentially fundamental, novel advancements (Figure 1.3.3). At the same time, the independence is at the foundation of the limitations of Einstein's ideas, as we shall see.

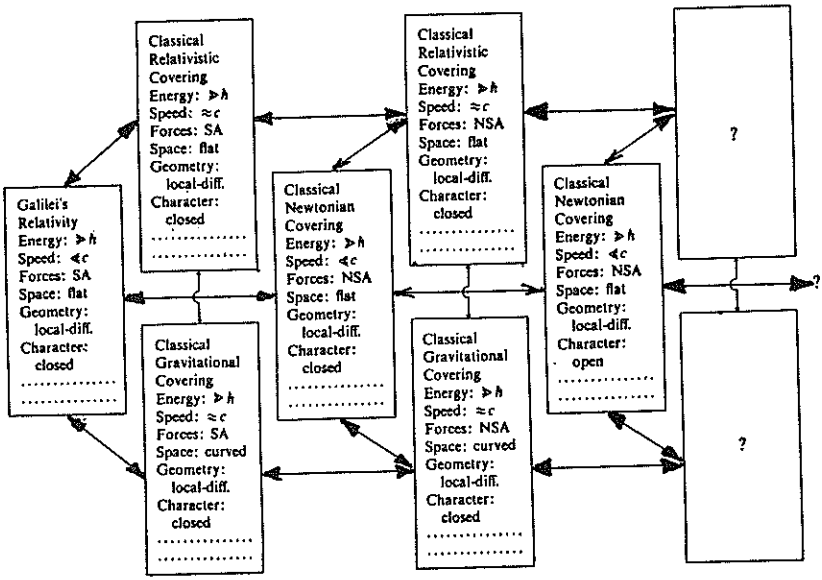


Figure 1.3.3. A reproduction of the figure of page 250 of ref. [10] illustrating the absence in physics of terminal theories. The first column depicts the conventional Galilei's relativity in Newtonian mechanics where: ħ is Planck's constant; c is the speed of light; and SA stands for selfadjointness, that is, the verification of the conditions for the forces to be of potential type [8]. The second column depicts the generalizations of Galilei's relativity proposed by Einstein's. As well known, the generalizations were intended to admit relativistic effects due to speed and gravitational effects due to curvature of space, but not more general forces. The relativity of the third column is the first conceived for the treatment of systems which are still purely Newtonian, yet of non–Galilean and non–Einsteinian type because of structurally more general forces of nonselfadjoint (NSA) type, that is, of contact/nonpotential type, as incontrovertibly established in the physical reality. In turn, the mere plausibility of the generalization of the third column implies the expectation of relativistic and gravitational generalizations of Einstein's relativities depicted in the fourth column, with additional chains of generalizations in sight. We can therefore conclude by saying that the lack of terminal character of Einstein's ideas can be identified in a rather forceful way via the mere inspection of the Newtonian reality of our environment.

Some, rather frequent, dishonest comments intended to suppress the need for suitable non—Einsteinian generalizations of Galilei's relativity.

I would like to close this section by providing the fellow taxpayer with some elements of judgment to identify dishonest academic postures in regard to the research reviewed in this section.

The fact that Galilei's relativity is violated in the classical physical reality of our environment is an absolutely incontrovertible fact. The relativity necessarily implies the conservation of the energy and other physical quantities. The insistence on its validity would imply the existence of the perpetual motion in our environment. Again, the proposed generalized relativities are conjectural, tentative and yet incomplete. But the insufficiency of Galilei's relativity for the description of our Newtonian environment is absolutely out of the question.

Whenever confronted with this reality, and with efforts in attempting generalizations, dishonest academicians generally venture rather incredible (at times hysterical) mumbo jumbo talks.

The most plausible reason why these academicians dismiss the violation of Galilei's relativity in our environment is due to the fact that such violation implies a corresponding violation of Einstein's special relativity (Section 1.4), as well as some irreconcilable inconsistencies of Einstein's general relativity (Section 1.5).

These occurrences must be expected from the deep inter—connections and mutual compatibilities among the Galilean, the special, and the general relativities (Section 1.2).

The purpose of this book is to stimulate the taxpayer to initiate actions aimed at an improvement of ethics in physics, and of accountability in the use of public funds.

Along these lines, it is important that the taxpayer is informed in more details of the arguments by which academic barons attempt to suppress the invalidity of Galilei's relativity in Newtonian mechanics.

Approach an academician with documented record of vested interests in Einstein's ideas. Present the equations of motion of any system of our environment, such as the damped pendulum, the damped giroscope, etc. All these equations violate Galilei's relativity in a manifest way (see ref. [10], pp. 344—348 for a treatment and classification). Ask them to reconcile this reality with the validity of Galilei's relativity.

One answer I have heard countless times is that the violation is "apparent" (sic), in the sense that if the equations are subjected to an appropriate transformation, the validity of Galilei's relativity is regained.

Fellow taxpayer, do not be blinded by this type of academic talk with a mask of technical vest. You are the observer watching the decay of the pendulum (that is, the NON–conservation of its energy) or the decay of the gyroscope (that is, the NON–conservation of its angular momentum). Any relativity, to be applicable, must hold in the frame of the observer, that is, in your frame, and not in another hypothetical frame. At any rate, explicit calculations are possible (and I have done them, see ref. [10], p. 246) to prove that, in general, the transformed frame in which Galilei's symmetry might be recovered is generally nonrealizable with experiments because it would imply accelerating all your laboratory equipments into a logaritmic orbit spiraling throughout the Milky Way!

In short, when academic barons suggest you to change reference frame to regain Galilei's relativity, chances are that the guys are asking you to sail with your equipment throughout our galaxy so that they can protect vested academic–financial–ethnic interests.

Another mumbo–jumbo comment I have heard countless times, is that the forces causing the breaking of Galilei's relativity are themselves "apparent" (sic!), because, the argument goes, when the systems considered (damped pendulum, damped gyroscope, satellite during re–entry, etc.) are reduced to their elementary particle constituents, the potentiality of the force is regained in full, and so is the strict validity of Galilei's relativity.

This second argument is much more dishonest than the former, in my view, for numerous reasons.

First, you have presented the academician ONE single equation describing very well the decaying of the angular momentum of the gyroscope, etc. With the argument above, the academic baron is essentially telling you that this is wrong. What you should do instead is to replace your single equation with multi–gillions of many different equations for all the elementary constituents of your system. You do not need a Ph. D. in theoretical physics to see that, while you could compute numbers with the original, single, Galilei–non–invariant equation, you have lost all computational capability whenever you (try to) replace it with a very large number of different equations.

In short, chances are that the academic baron is asking you to renounce all your computational capability in engineering, so that he/she can serve vested academic–financial–ethnic interests. And, do not forget, fellow taxpayer, that any physicist proposing this is fully aware of the implications. It is all done in full consciousness!

But this is only the beginning of the story. The argument of reducing Newtonian systems to elementary constituents in the hope of regaining conventional relativities is plagued by so many technical inconsistencies to truly render it

dishonest, particularly when ventured verbally, without the backing of published papers.

Regrettably, this general presentation is not conducive to technical treatments. Nevertheless, permit me to recall that the forces experienced by the damped oscillator, by the damped gyroscope, by the decaying satellite, etc., are of generally non–Hamiltonian and non–canonical type. This implies that the time evolutions of the systems are generally of noncanonical type. Now the description of the elementary constituents demands quantum mechanics (Section 1.6) and, for any conventional relativity to hold, the time evolutions must be of the so–called unitary type.

The technical inconsistency under consideration here is that a classical noncanonical time evolution cannot be reduced to a collection of unitary ones, no matter how many you have of them. In fact, at the classical limit of the quantum description, unitary laws will always recover potential forces, and you will never be able to recover the true, actual, real NON–potential/NON–Hamiltonian force of your system.

These things are taught in undergraduate studies of physics and, as such, are well known, and otherwise must be assumed as known by anybody venturing judgments on the "apparent" character of the invalidation of Galilei's relativity in our environment. It is their widespread knowledge that renders unavoidable the raising of ethical issues.

Recommendation to the taxpayer of asking for suitable qualifications by scholars dismissing the limitations of Einstein's relativity.

My first suggestion to the fellow taxpayer is the following. Whenever academicians dismiss or otherwise minimize the invalidation of Galilei's relativity in our reality, the fellow taxpayer should ask for their curriculum and see the papers and books published by the guys. If these papers are heavily dependent on Einstein's ideas, the most probable reasons for the attitude is the protection of vested, academic–financial–ethnic interests, in disrespect of the pursuit of new scientific knowledge.

My second suggestion is the following. Whenever an academic baron tells you that your classical, non–potential, non–Galilean systems can be reduced to a large collection of potential Galilean, elementary, constituents, ask reference to proofs of consistency of the reduction printed in refereed articles. If the baron does not provide such evidence, his dishonesty is established beyond a reasonable doubt. The guy is quite likely acting to protect vested interests.

My list of inconsistencies in the (sometimes frantic) attempts by corrupt academicians to retain old ideas at any cost,

could go on and on, but I do not want to bore you.

The story of governmental pressures on NASA regarding the prediction of the location of impact of Skylab during its re–entry.

The following story may be quite instructive. I have heard it in academic corridors, and I do not know whether it is true or false.

The story is related to the re–entry of Skylab on Earth of a few years ago. Recall that, during the last days prior to impact, NASA did not know where the station would fall. NASA merely knew a strip several hundred miles wide around the entire Earth in which the impact would occur. But the Kremlin was well within such a strip and, thus, it was within the area of impact.

Owing to this occurrence, high governmental officers exercised pressures on NASA scientists to have them sharpen their prediction and calculate more precisely where Skylab would indeed fall.

Whether this is true or not, the press coverage of the episode documented quite well that NASA was indeed under severe pressures to predict the point of impact, and that every possibility was indeed attempted. Now, NASA had at its disposal the best possible scientists, the most powerful computers and the most elaborate sensors on board Skylab that kept sending down, up to the last hours, all sort of data on pressure, temperature, density, etc.

As well known, despite these massive means, NASA was unable to predict the point of impact during the last days of re–entry of Skylab.

As a result, the story goes, pressures on NASA scientists grew and grew by the hour to predict the location of impact. At one point, a high governmental officer urged a NASA scientist at the Johnson Space Center in Texas to call in academician experts in relativities, at which, so the story goes, the NASA scientist promptly replied:

"If a professor comes here with his relativities, he will be chased out of NASA's premises."

The appraisal of our current knowledge provided by the re–entry of Skylab.

The story, whether true or only imagined, is very instructive. Relativities provide the ultimate characterization of dynamics. The governmental officer was therefore well informed of physics, and the recommendation to call in experts in relativities was therefore fully sound. But the reply by the NASA

scientist was equally sound.

While orbiting in empty space, Skylab was a true Galilean system. In fact, its shape and dimension did not affect its dynamical evolution. Under these conditions, Galilei's vision was correct: Skylab could be well approximated as a massive point. The applicability of Galilei's relativity was consequential. This implied the capability of predicting with extreme accuracy the location of Skylab in empty space and in time.

But, once within Earth's atmosphere, Skylab was no longer a Galilean system because the actual size, shape and structure of the station affected directly the re—entry trajectory. This means that Skylab was experiencing contact forces of nonlocal/integral type inasmuch as they were generated at its entire surface. A system of this type is fundamentally outside the technical capability of Galilei's relativity, as well as of Einstein's special relativity, and Einstein's general relativity, as we shall see. In fact, the strict applicability of these relativities would have implied the conservation of the angular momentum, that is, according to the professor, Skylab would have continued to orbit indefinitely within our atmosphere!

This is the reason why the NASA officer would have chased the professor out of NASA's premises. All his/her voluminous books on Einstein's relativities, not only would have been useless, but would have implied ridiculous consequences.

In short, we have reached, today, an extremely advanced knowledge on systems verifying conditions 1), 2) and 3) at the beginning of this section (point—particles moving in empty space). NASA's exploration of the solar system proves that such knowledge permits predictions of extremely high accuracy. Nevertheless, we have virtually no knowledge on the more general systems 1'), 2') and 3'), i.e., for extended objects moving within a resistive medium.

Preliminary elements on the opposition by S. Coleman, S. Glashow, S. Weinberg and other senior scientists of Harvard University against non—Einsteinian generalizations of Galilei's relativity.

In 1977, I was visiting the Department of Physics at Harvard University for the purpose of studying precisely non—Galilean systems. My task was to attempt the generalization of the analytic, algebraic and geometric methods of the Galilean systems into forms suitable for the non—Galilean ones.

The studies began under the best possible auspices. In fact, I had a (signed) contract with one of the world's leading editorial houses in physics, Springer—Verlag of Heidelberg, West Germany, to write a series of monographs in the field (that were later published in ref.s [9] and [10]. Furthermore, I was the recipient of a research contract with the U. S. Depart-

ment of Energy, contract number ER–78–S–02–4720.A000, for the conduction of these studies.

Sidney Coleman, Shelly Glashow, Steven Weinberg, and other senior physicists at Harvard opposed my studies to such a point of preventing my drawing a salary from my own grant for almost one academic year.

This prohibition to draw my salary from my grant was perpetrated with full awareness of the fact that it would have created hardship on my children and on my family. In fact, I had communicated to them (in writing) that I had no other income, and that I had two children in tender age and my wife (then a graduate student in social work) to feed and shelter.

After almost one academic year of delaying my salary authorization, when the case was just about to explode in law suits, I finally renceived authorization to draw my salary from my own grant as a member of the Department of Mathematics of Harvard University.

But, Sidney Coleman, Shelly Glashow and Steven Weinberg and possibly others had declared to the Department of Mathematics that my studies "had no physical value". This created predictable problems in the mathematics department which lead to the subsequent, apparently intended, impossibility of continuing my research at Harvard.

Even after my leaving Harvard, their claim of "no physical value" of my studies persisted, affected a number of other scientists, and finally rendered unavoidable the writing of IL GRANDE GRIDO.*

The details of the story are presented in Section 2.1, while the documentation is available from the publisher of this book. In this way, the taxpayer will be provided with all the necessary elements to decide on his/her own whether S. Coleman, S. Glashow, S. Weinberg and other officers of Harvard University acted in good faith, or their actions were intended to protect vested, academic–financial–ethnic interests in disrespect of their scientific accountability in the use of public funds.

*S. Glashow and S. Weinberg obtained the Nobel Prize in physics in 1979 on theories, the so–called unified gauge theories, that are crucially dependent on Einstein's special relativity; subsequently, S. Weinberg left Harvard for The University of Texas at Austin, while S. Coleman and S. Glashow are still members of Harvard University to this writing.

1.4: THE AGING OF EINSTEIN'S SPECIAL RELATIVITY IN CLASSICAL MECHANICS.

The lack in physics of terminal theories.

Physics is a science that will never admit terminal theories. No matter how valid Einstein's ideas are for contemporary physics, generalized theories will one day be constructed for physical conditions broader than those currently known. It is only a matter of time. It is evident that, the sooner these generalizations are constructed, the better for the advancement of human knowledge. It is also evident that such generalizations will not be constructed overnight. As history of physics teaches, the generalizations will be the result of a long scientific process of trials and errors, presentation of plausible ideas, and their critical examination by independent researchers. Therefore, the sooner the scientific process is initiated, the better.

These facts are well known. They are demanded by scientific ethics as well as the need for scientific accountability vis–a–vis the taxpayer supporting the research. In fact, today we could be using the special relativity under physical conditions for which it is fundamentally insufficient, with consequential waste of public money.

The reality of the situation in U. S. physics departments and research institutions could not be more removed from the above ethical guidelines.

Vested, academic–financial–ethnic interests on Einstein's ideas.

To understand the ethical status in the field, one must recall that Albert Einstein has been the biggest dispenser of academic chairs in the history of physics, of course, not personally, but via his ideas. This has created immense ethnic, financial and academic interests that will be manifestly damaged by any generalized theory. Even the consideration of the limitations of the special relativity, let alone open studies on its generalization, are damaging to vested interests.

This book is intended to be a documentation of the rather extreme, at times hysterical oppositions, obstructions, interferences, manipulations, and shear dishonest actions I have personally experienced in the U. S. physics community while attempting to conduct a critical examination of the limitations of Einstein's ideas and their possible generalizations.

The understanding is that I am not alone. In fact, the methods of suppression at birth of undesired advances in phy-

sical knowledge appear to be practiced across all segments of the physics community, from the assignment of jobs, to the publication of papers, and to funding of research programs.

By publishing this book, I also hope that other colleagues with similar experiences will come out and expose specific names to the societal judgment. In fact, dishonesty feeds on silence, which, as such, is complicity.

This book is solely dedicated to the presentation of my own experience. I shall be silent on the experience by others known to me. In fact, it is up to them to speak out and identify seemingly dishonest academic barons operating under public financial support.

But, let us proceed in an orderly fashion. To judge whether or not dishonesty does indeed exist in U. S. basic research, it is essential to know first the scientific profile. Only then, individual actions and reactions can be properly appraised.

Outline of the status of our knowledge on the special relativity.

To put it in a nutshell, the current state of the art of our knowledge in regard to the special relativity in classical mechanics is the following:

A) A physical arena of unequivocal validity of the relativity is solidly established;

B) Broader physical conditions of insufficiency of the special (as well as general) relativity have been identified;

C) Studies on the generalizations of the special relativity for the broader physical conditions considered have already been initiated by a number of independent scholars, with particular reference to the generalization of the underlying mathematical tools (mechanics, algebras and geometries). The understanding is that the currently available generalizations are tentative. Nevertheless, they constitute valid working grounds for interested young minds of all ages.

In short, we are at the beginning of the scientific process indicated earlier. The fellow taxpayer should keep in mind that the studies under consideration in this section, being classical, constitute only half of the needed studies, the remaining half being the quantum mechanical ones (see Section 1.6). In turn, the ultimate experimental resolution of the problem is expected to occur precisely within a quantum mechanical setting.

Nevertheless, the classical studies remain essential for any meaningful scientific program and, for this reason, they are simply unavoidable. At any rate, classical studies constitute an excellent introduction to the much more advanced and abstract issues in particle physics.

An arena of unequivocal validity of the special relativity in classical mechanics.

The historical contributions by Lorentz [22], Poincaré [23], Einstein [24], and others that were termed "Einstein's special relativity" identified quite clearly the physical conditions of conception and validity of the relativity. These conditions were reproduced in the early treateses in the topic, such as that by Bergmann [25] (see the title of Chapter VI). Regrettably, the same conditions were subsequently suppressed in more recent treateses, such as those by Weinberg [26], Misner, Thorne and Wheeler [27], or the more recent book by Pais [28]. In this way, the special relativity has acquired the character of universal applicability that is tacitly implied in contemporary presentations.

When interested in the limitations of the special relativity, young minds of any age are therefore urged to consult the original contributions of the builders of the theory, rather than the contributions of their followers.

Stated in a way as simple as possible, the special relativity is incontrovertibly valid for systems of particles verifying the following conditions:

I) The particles can be well approximated as being point–like;

II) The particles move in empty space assumed as homogeneous and isotropic; and

III) Gravitational and quantum mechanical effects are ignorable.

Thus, Conditions I) and II) remain exactly the same as Conditions 1) and 2) for the validity of Galilei's relativity (Section 1.3), while only Condition 3) is broadened into III) to permit speeds of the order of that of light. These occurrences should be expected. In fact, the preservation of Conditions 1) and 2) in the relativistic generalization of Galilei's relativity constitute the premises for the compatibility of the two relativities.

Conditions I) and II) are not merely conceptual, because they have deep technical implications. The point–like character of the particles permits the use of local geometries, algebras and topologies. The homogeneity and isotropy of empty

space implies the validity of a central component of the special relativity, the rotational symmetry. The special relativity itself is finally reached from Conditions I) and II) via the imposition of the constancy of the speed of light in vacuum for all inertial observers.

The hystorical, fundamental role of Lorentz and Poincaré in the construction of the special relativity.

The special relativity is fundamentally dependent on transformations discovered by Lorentz [22] for the case without translations, and by Poincaré [23] for the more general case inclusive of translations. These transformations are today called Lorentz and Poincaré transformations.

There is no doubt that the mind who mastered the reduction of available knowledge into one, single, physical theory, the special relativity, was that of Einstein. Nevertheless, the appropriateness of the terms "Einstein's special relativity" have been repeatedly questioned throughout the years because of the fundamental value of the contributions by Lorentz and Poincaré.

For these reasons, a terminology more ethically appropriate I shall adopt hereon for scientific profiles is that of "Einstein—Lorentz—Poincaré relativity" (ELP—relativity) or "special relativity" for short. The terms "Einstein's special relativity" will be used in political parlance.

Once an arena of unequivocal applicability of the special relativity is known, the identification of broader physical conditions suggesting possible generalizations is consequential.

In the following, I shall consider first physical conditions broader than II). Conditions broader than I) will be considered subsequently.

The plausibility of small anisotropies of space.

Consider Condition II). Inspection of our macroscopic environment clearly supports the hypothesis of the homogeneity of empty space. However, the hypothesis of joint isotropy is not equally tenable. This is due to the fact that empty space is far from being "empty". It is in actuality a rather complex medium transmitting all electromagnetic interactions, as well as permitting the existence of elementary particles as some form of dynamical oscillation. As a result, a number of possibilities exist whereby homogeneity can be assumed as exact (for all practical purposes of our current knowledge), but isotropy is only approximate.

As an illustration, it is possible that the violent process of creation of the universal via the primordial explosion (called "big bang") may well have created an anisotropy along the dir-

ection of explosion, and that anisotropy is sufficiently small to have escaped current experimental observations until now.

Numerous additional arguments of plausibility of a sufficiently small anisotropy of space exist in the literature, but they are ignored here for brevity.

The generalization of the special relativity for homogeneous but anisotropic spaces by Bogoslovsky from the U.S.S.R.

If space is homogeneous but anisotropic, even in a very small amount, the Einstein—Lorentz—Poincaré relativity is invalid on strict scientific grounds. A suitable generalization would then be needed for systems of point—like particles moving in a homogeneous but anisotropic space.

Such a generalized relativity has already been constructed in 1977 by Bogoslovsky [29] in all essential elements. Very regrettably, these intriguing studies have been ignored in the virtual totality of the contemporary physical literature.*

The generalization of ref.s [29] is technically based on the replacement of the space underlying the special relativity, the Minkowski space, with the more general Finsler spaces (see, for instance, ref. [30]) which are precisely capable of representing homogeneous but anisotropic media. The generalized relativity then follows by imposing the constancy of the velocity of propagation of light. This leads to a generalization of the fundamental transformations by Lorentz and Poincaré.

The notion of covering theory.

Recall that a physical theory is a "covering" of another one when: (a) the former theory applies for physical conditions broader than those of the latter; (b) the former theory is based on mathematical tools structurally broader than those of the latter; and (c) the former theory contains the old one as a particular case.

The generalized relativity of ref.s [29] is a covering of the special relativity. In fact, it applies for broader physical conditions (anisotropic space); it is based on broader mathe-

*Owing to this silence, particularly in recent books and technical reviews, it is virtually impossible to identify other contributions in the problem, unless one spends years of library search. I would therefore gratefully appreciate the indication of any contribution, specifically devoted to the generalization of the special relativity, that preceded or followed the studies of ref.s [29]. I am referring to generalizations for point—like particles moving in a homogeneous but anisotropic medium, in which gravitational and quantum effects are ignorable. Attempts trying to render the special relativity compatible with a possible anisotropy of space are of no scientific relevance when compared to suitable generalizations.

matical tools (Finsler spaces); and it recovers the special relativity identically whenever the anisotropy is put equal to zero.

In the traditional style of physical advances, the relativity of ref.s [29] is dependent on the preceding work by Lorentz [22], Poincaré [23] and Einstein [24]. For this reason, I shall call the generalized relativity under consideration here the "Bogoslovsky—Einstein—Lorentz—Poincaré relativity", or B E L P —relativity for short.

Scientific implications of Bogoslovsky's studies.

The practical implications of the B E L P —relativity are quite intriguing indeed. in fact, the generalized relativity is consistent with most of the predictions of the special relativity. The primary deviations occur for speeds approaching that of light in vacuum. In fact, the predictions of the gerneralized and of the special relativities regarding the speed dependence of mass, time, length, etc. coincide up to sufficiently high values of speeds and then diverge.

The only possible scientific conclusion at this time is that the B E L.P —relativity is mathematically consistent, plausible, and not disproved by available experimental evidence up to the very high speeds achieved in particle accelerators.

If the ELP—relativity is exactly valid, the mass of the accelerated particles will tend to infinity with the approaching of the speed of light, as well known. If, on the contrary, infinities do not exist in the universe, and the B.E.L.P—relativity is correct, we should expect deviations from the predictions of the ELP—relativity beginning with a certain, hiterto unknown, value of speed (see Figure 1.4.1 for more details).

The invalidation of the special relativity implied by the inapplicability of Galilei's relativity in Newtonian mechanics.

The possible anisotropy of space and the Bogoslovsky—Einstein—Lorentz—Poincaré relativity are only the tip of the iceberg. In fact, we have learned in Section 1.3 that particles can be conceived as moving in empty space only under rather special circumstances. A more general physical situation is that of extended objects moving in material media. In this case, the inhomogeneity and anisotropy of the medium is incontrovertibly established by experimental facts. The inapplicability of the Einstein—Lorentz—Poincaré relativity then follows from that of Galilei's relativity.

Invalidation arguments based on the instantaneous character of contact interactions among extended objects.

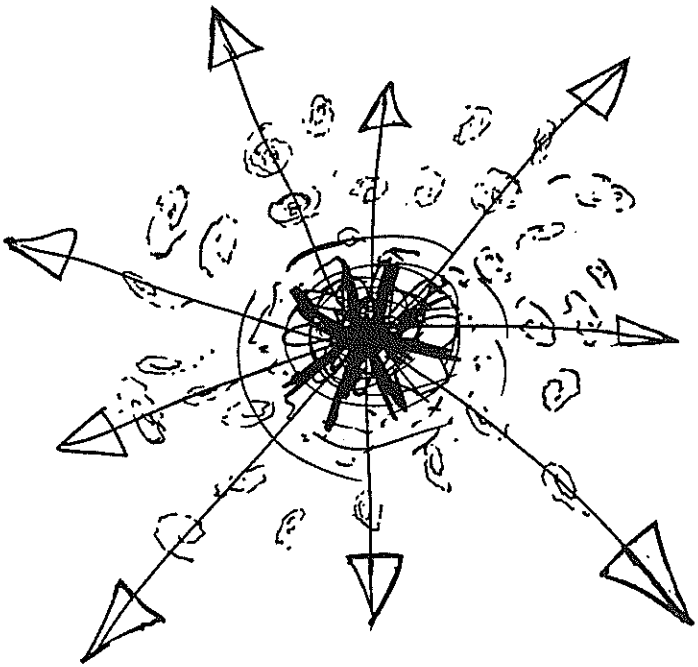


Figure 1.4.1. A schematic view of the primordial explosion (the "big bang") that lead to the creation of the universe as seen by us. It is possible that such explosion created an anisotropy of the space characterized by the direction of propagation of the galaxies, and that such anisotropy is sufficiently small to have escaped detection until now. A number of additional arguments in other branches of physics (e.g., thermodynamics, or particle physics) also lead to a conceivable anisotropy of space. In turn, such anisotropy, if confirmed, would lead to the invalidation of the Einstein–Lorentz–Poincaré relativity at the speed of light, while the same relativity remains valid for speeds sufficiently smaller than that of light. A generalization of the special relativity for anisotropic space has been worked out by the U.S.S.R. physicist Bogoslovsky [29]. Intriguingly, the generalized and special relativities have exactly the same predictions for a range of speeds varying from zero up to relativistic speeds. The predictions of the two theories then diverge with the approaching of the speed of light. Lack of studies on the issue, particularly in the U.S.A., prevent any resolution of the validity or invalidity of the studies of ref.s [29]. The only scientific conclusion we can reach at this moment is that the special relativity is valid up to the very high speeds attained in particle accelerators. No scientific conclusion is possible at this time for speeds very near that of light.

The same conclusion can be reached in a virtually endless variety of ways. For instance, it is known that the notion of simultaneity is outside the context of the Einstein–Lorentz–Poincaré relativity. But then, this evidently implies the inability of the relativity to incorporate the contact/nonpotential/nonlocal interactions of our real world (Section 1.3). In fact, these interactions demand the actual contact of the objects. They are therefore instantaneous by nature and, thus, outside the special relativity. As a result, and as easily predictable, the forces characterizing the inapplicability of Galilei's relativity, characterize also the inapplicability of the special relativity.

Invalidation arguments based on the deformable, rotationally–noninvariant character of extended objects.

A further equivalent way of reaching the same conclusion is the following. Another limitation of the special relativity fully identified in the original treatments, but avoided in more recent ones, is the inability to represent deformable objects. In fact, the special relativity is applicable only to absolutely rigid bodies, while no relativistic formulation of the entire branch of engineering known as the theory of elasticity has ever been achieved.

It is evident that perfectly rigid objects are a mere academic abstraction. In the real world, all material objects are elastic. Evidently, the amount of deformation may vary from one object to another and from one physical condition to another. But the existence of the deformation itself is absolutely out of question.

This deformation implies the incontrovertible invalidation of the Einstein–Lorentz–Poincaré relativity. The occurrence can be proved in a variety of ways with a minimum of high school mathematics.

Consider a particle moving in empty space at sufficiently high (relativistic) speeds. Suppose that the particle is perfectly spherical and with unit radius (say, one cm). In three–dimensional Euclidean space, the sphere is represented by $R' R = xx + yy + zz = 1$, where R is a column with values (x, y, z); R' is its transpose (a row with the same values); x, y, z are the (Cartesian) coordinates of a generic point of the sphere with center at the origin of the reference system; and 1 represents the unit radius.

Under the conditions considered, the special relativity is strictly verified. In fact, the sphere is a particular case of the Minkowski invariant $X'mX = xx + yy + zz - tc^2t$ where X is a column with elements (x,y,z,ct): X' represents the transpose of X; m is the Minkowski metric (a four–by–four diagonal matrix with elements $m = diag(+1, +1, +1, -1)$ and

zero elsewhere); t represents time; and c represents the speed of light in vacuum.

The Poincaré transformations are the most general possible, linear transformations $Y = AX$ preserving the Minkowski invariant, $Y'mY = X'mX$, while the Lorentz transformations are the most general possible ones without translations. Note in particular, the rotational symmetry originating from the perfectly spherical shape of the particle.

Suppose now that, at a certain value of time, the particle experiences a deformation of its shape due to sufficiently intense external forces or collisions. Assume the simplest possible deformations, those into the ellipsoids $R'gR = xa_1 x + ya_2 y + za_3 z = 1$ where g is a three—by—three diagonal matrix with elements $g = \text{diag}(a_1, a_2, a_3)$ given by positive definite quantities representing the three characteristic axis of the ellipsoid.

The invalidation of the special relativity under the broader physical conditions considered is then incontrovertible for a number of independent, but concurring reasons, such as the breaking of the rotational symmetry, the loss of the Minkowski invariant, etc.

At any rate, the proof can be conducted by any high school student. When the spherical particle is deformed into an ellipsoid, the Minkowski invariant must be replaced by the more general one $X'GX = xa_1 x + ya_2 y + za_3 z - tc^2 t$ where G is now a four—by—four diagonal matrix with elements $G = \text{diag}(a_1, a_2, a_3, -1)$. Then, the Lorentz transformations produces two effects on the generalized invariant X'GX. First, they alter the shape of the ellipsoid, and, second, they alter the value of the speed of light in vacuum. In this way, the insistence in the preservation of the special relativity for deformed spheres implies the violation of two of its basic postulates, that of form—invariance, and that of constancy of the speed of light.

Needless to say, the considerations above have been specifically selected for the nontechnical level of this book. The technical treatment can be presented in rather sophisticated theoretical language (via the embedding of the deformed sphere in Euclidean space of the so—called SO(3) symmetry, into the covering complex space of the so—called SU(2) symmetry, and then extending the results to the covering of the Lorentz group, the so—called SL(2.C) group).

The taxpayer, however, should dismiss these technical aspects. They may have a value in satisfying academic wishes and preferences, but the physical roots of the invalidation of the special relativity remain the same as those in the rudimentary considerations presented above.

Invalidation arguments based on the locally varying character of the speed of light.

A further way of reaching the same conclusion is by examining the basic postulates of the Einstein—Lorentz—Poicaré relativity and comparing them with nature. As recalled earlier, the relativity is based on the constancy of the speed of light. But, as everybody knows, the speed of light is not constant in the real world. Not at all. In fact, such speed has a complicated functional dependence on a number of physical characteristics, beginning with the frequency f of light itself, and continuing with characteristics of the medium in which the propagation occurs, such as: local coordinates r ; time t ; density d ; etc. We must therefore assume that the speed of light is a function of the type $c = c(f, r, t, d, \ldots)$. .

The question is then: does the special relativity apply to the speed of light as it actually occurs in nature, that is, with a complex functional dependence on local physical characteristics? The answer is NO! In fact, the Lorentz transformations are generally unable to preserve the value of such a locally varying speed, contrary to the very fundamental postulate of the relativity itself.

It is evident that there is no contradiction here with the celebrated Michelson—Morley experiment. In fact, this experiment was intended to treat the speed of light in vacuum [25]. We are referring here to a different physical arena, such as light traveling in a region of space occupied by a variety of adjoining, transparent substances, such as air, ice, glass, oil, water, etc.

Complementarity of all invalidation arguments.

Equally evident is the complementarity of the deformation of physical objects with the dependence of the speed of light on local physical conditions. In fact, they both refer to the need to generalize the basic Minkowski invariant via structures at least of the type $X'GX = R'gR - tC^2t = xa_1\, x + ya_2\, y + za_3\, z - tC^2t$ where we assume hereon that X is the column with elements (x, y, z, t); G is the diagonal matrix with elements a_1, a_2, a_3, and $-C^2$, all depending on local physical characteristics, that is, $G = G(X, \dot{X}, d, \ldots)$.

The space part $R'gR$ of the generalized separation then permits the description of extended, deformable particles, as well as motion within inhomogeneous and anisotropic media, while the time part tC^2t represents the locally varying speed of light.

The four—dimensional space with points $X = (x, y, z, t)$ equipped with the invariant $X'GX$ can be conceived as an isotope of the Minkowski space and, for this reason, it is called the

Minkowski–isotopic space [32]. This isotopy is useful for constructing the new space–time symmetries (see below).

A non–Einsteinian generalization of the special relativity for extended, deformable particles moving within inhomogeneous and anisotropic media.

A generalization of the Einstein–Lorentz–Poincaré relativity for the more general physical conditions under consideration here, has been submitted in ref.s [32, 33] following preparatory works in ref. [31] as well as previously quoted references by the same author. We are referring to a generalized relativity for systems of particles which:

I′) cannot be effectively approximated as being point–like, thus demanding a suitable representation of their extended and therefore deformable character;

II′) move in physical, generally inhomogeneous and anisotropic material media; and

III′) gravitational and quantum effects are ignorable as in III).

The generalized relativity is then reached by imposing the local invariance of the locally varying maximal speed of propagation of causal signals. More explicitly, such speed is assumed as varying from one space–time point to another, as indicated earlier. Thus, the invariance is referred to the value of the maximal speed at each space–time point (local invariance).

Also, the speed of light is replaced in the generalized relativity of ref. [32] with the "maximal speed of propagation of causal signals", that is, of signals verifying the principle that effects do not precede the cause in our time arrow. This is recommended when the generalized relativity is applied to the interior of hadronic matter (such as a nucleus). In fact, light cannot propagate within these media (whose density is among the highest known in the universe). Light is then replaced by any causal signal, such as the collision of a particle on one point of the surface of a nucleus, and the subsequent, consequential process of emission of other particles in another point of the surface of the same nucleus.

The central part of the non–Einsteinian generalization of the special relativity: the explicit construction of the generalized Lorentz and Poincaré transformations.

The most important part of the generalized relativity of ref. [32] is constituted by the techniques permitting the expli-

cit construction of the generalized Lorentz and Poincaré transformations that apply for conditions I'), II') and III'). These techniques are based on the so–called Lie–isotopic generalization of Lie symmetries which were proposed, apparently for the first time, in memoir [8], subsequently outlined in monograph [10], and more recently re–elaborated in papers [18, 19]. The main ideas are simple and deserving an outline.

The fundamental transformations of the special relativity, the Lorentz or Poincaré transformations, are representations of corresponding Lie groups, called Lorentz and Poincaré groups (see, for instance, ref. [6]). The special relativity is based on the postulate of invariance of nature under these groups of transformations.

Now, all (continuous) Lie groups in their current formulation are constructed from an element called the unit element. For the case of the Lorentz and Poincaré transformations in Minkowski space–time, this unit is the four dimensional unit matrix I having all +1 in the main diagonal and zero elsewhere, $I = \text{diag}(+1, +1, +1, +1)$.

The Lie–isotopic generalization of Lie symmetries permits the generalization of the Lorentz and Poincaré groups for all physical conditions I'), II') and III') via the replacement of their unit I into the generalized unit $\hat{1}$ given by the inverse of the metric G of the separation considered earlier, X'GX, while all other aspects of the original groups remain essentially unchanged.

A number of theorems then ensure that the generalized transformations emerging from this procedure (called in ref. [31] Lorentz–isotopic and Poincaré–isotopic transformations) leave invariant the new separation X'GX. Theorems aside, it is known that Lie groups leave invariant the unit in a trivial way. Exactly the same property holds when the theory is expressed in terms of the more general unit $\hat{1} = G^{-1}$. The invariance of all possible metrics G is then a trivial consequence.

In particular, the generalized transformations can be explicitly computed for each given physical condition via the sole knowledge of the metric G.

Direct universality of the generalized Lorentz and Poincaré transformations.

It has been proved [33] that the Lorentz–isotopic and Poincaré–isotopic transformations provide the form–invariance of the generalized separations X'GX for all possible metrics G (universality) in the space–time coordinates X of the experimenter (direct universality).

It should be also indicated that the sole restrictions on the metrics G are those of being, real–valued, symmetric, nonsingular and of verifying certain continuity conditions. The important

point is that the underlying geometry, and, most importantly, the functional dependence of G on local quantities are completely unrestricted by the Lie–isotopic theory.

Thus, while the special relativity is based on one, unique, type of transformations (the Poincaré transformations for the most general possible case inclusive of translations), the generalized relativity of ref. [32] applies for each of the multiple infinity of physical conditions I'), II') and III'), that is, of possible, different metrics G.

The local isomorphism between the Poincaré–isotopic group and the conventional group.

The Minkowski metric $m = diag(+1, +1, +1, -1)$ and the generalized metric here considered, $G = diag(a_1, a_2, a_3, -C^2)$ are equivalent from an abstract topological viewpoint, in the sense that, in both cases, the first three diagonal elements are positive definite, while the fourth elements are negative definite.

This equivalence has far reaching implications. In fact, it implies that the Poincaré–isotopic group is locally isomorphic to the conventional Poincaré group. This property is proved for the Lorentz–subcase in ref. [32] and the full proof is worked out in detail in ref. [33].

A necessary condition for the achievement of such isomorphism is that the generalized transformations are expressed via the Lie–isotopic theory (that is, via associative products of the type $A*B = AGB$, G = fixed, with Lie–isotopic product $A*B - B*A$, while the conventional Poincaré group is expressed via conventional associatives products AB with conventional, attached, Lie product $AB - BA$. (See Section 1.8 for more details).

We recover in this way a fundamental aspect of the Galilean studies of Section 1.3.

Recall that, for the Newtonian case, the generalized mechanics (Birkhoffian mechanics) coincides with the conventional mechanics (Hamiltonian mechanics) at the level of abstract, coordinate–free geometric formulations. The two mechanics emerged as being different realizations of the same geometric axioms (those of the symplectic geometry [17]). Hamiltonian mechanics is the simplest possible realization (called canonical), while the Birkhoffian mechanics was constructed as the most general possible realization of the same axioms [10].

In the transition to the applicable Newtonian relativities, the situation was predictably equivalent. In fact, the Galilei–isotopic and Galilean relativities admit one, single, abstract, geometric–algebraic formulation (technically realized by imposing that the Galilei–isotopic group is locally isomorphic to the conventional Galilei group [18, 19]).

The situation at the level of the generalization of the spe-

cial relativity under consideration here is equivalent, as it must be for unity of physical thought as well as self–consistency and mutual compatibility of the different layers of analysis.

In fact, we have generalized the Minkowski invariant from the form $X'mX$ applicable for point–like particles moving in empty space, into the form $X'GX$, $G = G(X, \dot{X}, \ldots)$, for extended–deformable particles moving within inhomogeneous and anisotropic material media. The generalization implies a corresponding one for the transformations, because the Poincaré transformations that leave invariant the separation $X'mX$ must be generalized into the Poincaré–isotopic transformations for the invariance of $X'GX$.

The important point is that, despite these differences, the Poincaré–isotopic group and the conventional Poincaré group admit one, single, unified, abstract, geometric–algebraic structure. The latter group is the simplest possible realization, while the former group is the most general possible one.

The scientific implications of this result are far reaching. In fact, the result relegates the problem of space–time symmetry breaking to mere semantics. The Poincaré symmetry can be considered broken for invariant $X'GX$ only when realized in the simplest possible way (that with the simplest possible associative product AB and attached Lie product $AB - BA$). However, if the symmetry is realized in a sufficiently more general way (that with the associative isotopic product $A*B = AGB$, with attached, Lie–isotopic product $A*B - B*A$), then the Poincaré symmetry is still exact for the generalized invariant $X'GX$, and no breaking of the ultimate axiomatic foundations has actually occurred. The only condition needed is that indicated earlier, the positive–definite character of the first three elements a_1, a_2, a_3, of the metric G, and the negative–definite character of the fourth element $-C^2$.

The implications for academic politics are truly substantial. As we shall indicate better in Section 1.6, a main reason for opposing studies on the possible invalidation of the Poincaré symmetry in the interior of strongly interacting particles (hadrons) is the expectation of the consequential invalidation of the currently central hypothesis of particle physics, that yet unidentified particles called "quarks" are the constituents of hadrons. In fact, quarks are a representation of the Poincaré group.

The local isomorphism between the Poincaré–isotopic and the conventional Poincaré group renders this expectation without scientific value. In fact, it implies the possibility that quarks can exist exactly as conceived today, even if the special relativity and the Poincaré symmetry are broken in the interior of hadrons.

The only difference would be in regard to the realization of the theory, which would acquire a generalized character in the

interior of hadrons as compared to the conventional character for the description of the exterior dynamics. In turn, these differences, as we shall see in Section 1.6, rather than being a drawback to quark theories, appear to permit the resolution of some of their most fundamental open problems (such as the confinement of quarks and the identification of their own constituents with physical particles).

The covering character of the generalized relativity over the special relativity and that worked out by Bogoslovsky.

The generalized relativity of ref. [32] is a covering of the special relativity in the sense indicated earlier in this section. In fact, the former relativity applies to a physical arena broader than that of the latter; it is based on more general mathematical tools; and it recovers the special relativity identically, whenever the original physical conditions are recovered identically.

Intriquingly, the generalized relativity of ref. [32] is also a covering of the Bogoslovsky–Einstein–Lorentz–Poincaré relativity [29]. This can be seen from the fact that the generalized invariant $X'GX$ admits the Finsler's invariant as a particular case (but the inverse is not generally true).

This situation was expected, because physical conditions I'), II') and III') are broader than those of ref. [29]. The mathematical methods of ref. [32] (Lie–isotopy) are also broader than those of ref.s [29] (which are conventionally Lie). The covering character of the former relativity over the latter must then occur for consistency.

As a further comment, it should be mentioned that the generalized relativity of ref.s [32, 33] is non–Einsteinian in the sense that it is not necessarily of the type of Einstein's general theory of gravitation. In fact, physical conditions I'), II') and III') are not related to gravitation. At any rate, the metric G is generally dependent on local velocities. As well known (see, for instance, ref. [27]), such a dependence is excluded in the Riemannian geometry of the general relativity.

Intriguingly, the methods of Lie–isotopy are applicable also to the case when G is the metric of Einstein's theory of gravitation, thus permitting the construction of the explicit form of the general coordinate transformations that leave invariant current gravitational theories. In fact, as indicated earlier, the Lie–isotopic theory demands no restriction on the functional dependence of the metric, thus permitting the gravitational case as a particular case.

The predictions of the generalized relativity that are confirmed by experimental evidence.

To this writing, some of the predictions of the generalized

relativity of ref. [32] are verified, others are plausible but experimentally unverified.

First, the generalized relativity recovers the well known Cerenkof effect in water. This is a physical condition concerning ordinary electrons which, in water, can travel faster than the speed of light in the same medium, thus emitting the bluish light visible in the pool of nuclear reactors. In fact, the speed of light in water is of the order of 2/3 that in vacuum, while ordinary electrons can travel in the same medium much faster than 2c /3. This case, which is fully established, is naturally represented by the generalized relativity of ref. [32] (see Figure 1.4.2 for more details).

The possibility of breaking the speed of light as the barrier of maximal possible speed in the interior of protons and neutrons, or in the core of stars.

As a complement to the Cerenkof light, the generalized relativity predicts maximal speeds C of causal signals higher than that of light in vacuum, in which case ordinary particles such as electrons, could travel at speeds higher than c . It is well known that such an occurrence is impossible for the original physical conditions I), II) and III) of the special relativity. Nevertheless, the occurrence has been proved as possible for generalized conditions I'), II') and III').

The possibility of ordinary massive particles (such as electrons) being accelerated beyond the speed of light in vacuum was predicted, apparently for the first time, in ref. [31] as a consequence of contact/nonlocal/nonpotential forces due to motion of extended particles within material media. In fact, these forces, having no potential energy, have dynamical implications fundamentally different than those of the action–at–a–distance, potential forces of the special relativity.

A typical arena for the realizations of conditions I'), II') and III') indicated in ref. [31] is that of the structure of strongly interacting particles (hadrons), such as proton, neutron, pions, etc. In fact, experimental evidence establishes that the wave–packets of the constituents of these particles must be in a state of mutual penetration and overlapping one within the space occupied by the other. The motion of each constituent can therefore be conceived as occurring within a medium constituted by other particles (the hadronic medium), thus resulting exactly in conditions I'), II') and III').

As a consequence, generalized relativity [32] predicts the possibility that the constituents of hadrons could be massive particles traveling at speeds higher than that of light. It should be stressed that these deviations from the special relativity are conceivable only in the interior of a hadron while the center–of–mass of the same particle remains strictly conformed to the

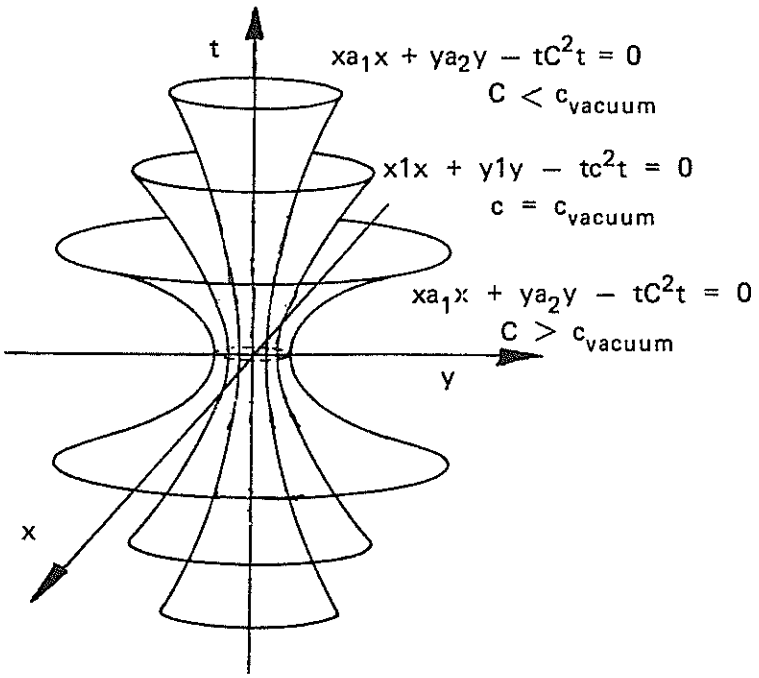


Figure 1.4.2. A reproduction of Figure 1, page 553, of ref. [32]. The central cone depicts the celebrated cone of light of the special relativity. The deformed cones are those predicted by the proposed covering relativity. The inner cone represents the case when the speed of light is smaller than that in vacuum because of propagation in transparent media such as water. In this case, ordinary particles such as electrons can propagate faster than light itself. This case is experimentally established and known as the Cerenkof effect. The outer cone is a prediction of the generalized relativity conceivable mostly for the physical conditions in the interior of strongly interacting particles or in hadronic matter, such as in the interior of a neutron or of a star (Section 1.6). In this latter case, the speed C is higher than that of the light in vacuum, c . In summary, the central prediction of the special relativity regarding c as the maximal possible speed of causal signals is tenable only under the conditions for which the special relativity was conceived, conditions I), II) and III) of the text. The surpassing of the speed c by physical, causal signals becomes conceivable for more general physical conditions. In turn, this opens up a truly vast horizon of potentially fundamental advances in numerous sectors of theoretical and applied physics. The currently available experimental information, even though far from a conclusive character, is encouragingly in favor of the hypothesis [31], that the maximal speed in the interior of hadronic matter is different than that in vacuum. It is given by recent re–elaborations of the dependence of the mean life of unstable hadrons in flight at different energies, which show quite clear deviations from the predictions of the special relativity (see ref.s [35, 36, 37]). These experimental aspects are considered in detail in Section 1.7.

special relativity.

To put if differently, ref. [31] proved the consistency of the relativistic generalization of the classical, Newtonian notion of closed/non–Hamiltonian system, whereby generalized physical laws for the interior of a proton or a neutron are fully compatible with conventional relativistic laws for the center-of–mass motion of the same particle. (See Figure 1.4.3 for more details).

Preliminary experimental information of support.

The possible significance of these generalized views for the solution of some of the problems of contemporary particle physics (such as the achievement of the so–called confinement of quarks) will be indicated in Section 1.6.

We here limit ourselves to the indication that the hypothesis of ref. [31] was submitted to a subsequent independent elaboration by De Sabbata and Gasperini in ref. [34]. By using the so–called gauge theories, these authors identified the first specific value $C = 75c$ as the maximal speed of causal signals for the interior of a hadron and, thus, as maximal speed of propagation of hadronic constituents.

As stressed by the authors, the calculations are based on a number of plausible assumptions. Thus, the value 75c of ref. [34] must be considered as merely indicative. The important point is the confirmation of a maximal speed greater than c. The actual value of the speed is of subordinate physical relevance.

Currently available re–elaboration of the data on the behaviour of the mean life of unstable hadrons at different energies appear to confirm the relativity of ref. [32]. In particular, the re–elaboration of the data on the mean life of charged pions and kaons by Nielsen and Picek [35] have confirmed the apparent existence of deviations from the special relativity, and, in particular, from the Minkowski separation $X'mX$. The applicability of the generalized relativity of ref. [32] is then consequential.

Independent but equivalent results have been achieved by Aronson et al [36] for the mean life of the neutral kaons. Additional, independent studies by Huerta and Lucio [37] also confirm the same findings of ref.s [35, 36]. Further studies can be found in ref. [38].

It should be stressed that all studies [35, 36, 37] are preliminary. The final resolution of the issue demands the conduction of a comprehensive experimental program, including the repetition of the direct measures of the mean life of unstable hadrons at different energies. These experimental aspects will be considered in more detail in section 1.7.

What is important for this presentation is that the deviations from the special relativity of ref.s [35, 36] as well as others not quoted here for brevity, are all particular cases of the

NONEINSTENIAN SYSTEMS

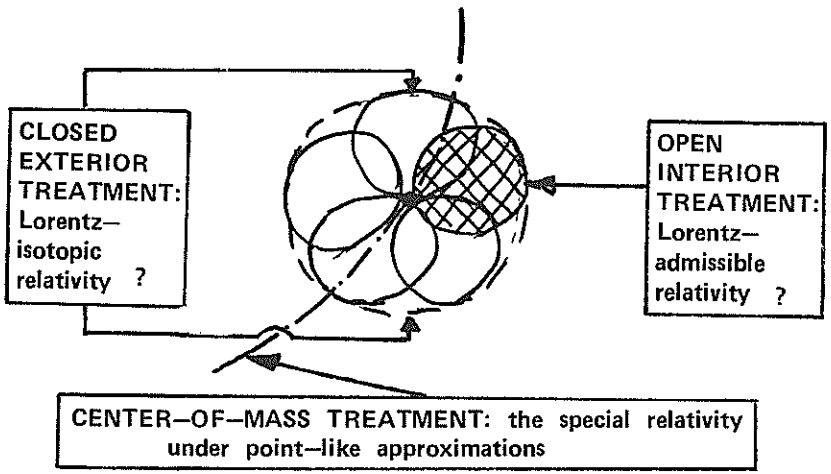


Figure 1.4.3. A pictorial view of the relativistic extension of the Newtonian notion of closed/non–Hamiltonian system (Section 1.3), worked out in ref.s [31, 32, 33]. The system is assumed to move in empty space. Its center–of–mass is therefore restricted to verify the special relativity, that is, to verify the conventional Minkowski invariant $X'mX$ described in the text. The constituents of the same system, however, are permitted to verify a dynamics fundamentally more general than the special relativity, that is, to verify the generalized invariant $X'GX$ also described in the text. It follows that the speed of the center–of–mass is bound by c, the speed of light in vacuum, while the maximal speed C of the constituents depends on local physical conditions (coordinates, velocities, density, etc.), but is otherwise unrestricted. Studies have furthermore indicated that, under contact/non potential/nonlocal forces, the speed C of the constituents can exceed the speed of light in vacuum. This is due to the fact that contact forces are capable of accelerating particles without any potential energy by assumption (the interaction being of contact type). This implies an alteration of the conventional relativistic dynamics and, thus, of the maximal speed. The achievement of a maximal speed C higher than c is then only a question of proper local physical conditions. As an example, consider a proton moving in the high vacuum chamber of a particle accelerator. As such, the proton experiences only action–at–a–distance, potential forces (of electromagnetic type). The special relativity then applies for all speeds achieved so far (see earlier remarks for speeds very close to that of light). Thus, when seen from an outside observer, the proton verifies the special relativity. Nevertheless, its constituents can verify the structurally more general relativity of ref. [32]. In particular, they can travel at speeds higher than that of light in vacuum. This latter possibility, rather than being far–fetched, is supported by preliminary experimental information (see Section 1.7 for details). In particular, rather than being against established knowledge in particle physics, the hypothesis appears to permit the resolution of some of the vexing open problems in quark theories, such as the achievement

of a strict confinement of quarks and the identification of their constituents with physical particles (see Section 1.6 for these latter issues).

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generalized separation $X'GX$ of the relativity submitted in ref. [32].

Evidently, we do not know at this moment whether or not generalized relativity [32] is verified in the physical reality. Nevertheless, we can state that, whenever the generalized Lorentz and Poincaré transformations are needed for invariants $X'GX$ in an explicitly computed form, the Lie–isotopic methods of ref. [32] apply, by providing the desired results. Other methods that may be conceivably identified in the future will be inevitably equivalent to those of ref. [32].

To state it differently, the explicit construction of generalized Lorentz and Poincaré transformations for each element of the multiple infinity of possible invariants $X'GX$, $G = G(X, \dot{X}, d, \ldots)$, have been identified in ref. [32] for the first time, and this is the priority of that publication.

The incompleteness of this presentation.

It should be stressed that, by no means, this presentation is exhausting all mathematical, theoretical and experimental studies on the limitations and possible generalizations of the special relativity.

As a result, this presentation is grossly deficient in completeness. I would like to apologize to all authors for my inability to present a comprehensive review of their work. In fact, such a presentation would have been so voluminous, to call for a separate book.

Nevertheless, I would like to encourage authors to keep me informed of their past and forthcoming contributions on the limitations and possible generalizations of the special relativity. In fact, numerous editorial initiatives are under way at the Institute for Basic Research, in Cambridge, U.S.A., such as the possible organization of reprint volumes on all these studies. The availability of the information could therefore offer the possibility of remedying the deficencies of this presentation at some future time.

The interruption of the research.

Paper [32] is a summary letter, as one can see. The extended presentation of the generalized relativity is contained in manuscript [33] which is yet untyped to this writing.

It is significative here to note that the research on the topics presented in this section (as well as others) was interrupted for the writing of this book, and this included the interruption in the completion of paper [33] which is perhaps the

most important one of my research life.

The reasons for such a rather extreme sacrifice are numerous. The first reason is due to my conviction that, lacking a serious consideration and containment of the problem of ethics in the U. S. physics, studies on the generalizations of Einstein's relativities constitute mainly a waste of time. Whenever the attempts to suppress them fail, the studies are generally discredited at birth in academic corridors.

In particular, I do not foresee the possibility that the U. S. physics community can undertake the comprehensive experimental program needed for a scientific resolution of the issue, unless the problem of ethics in physics is first tackled in a serious way.

Another reason for the interruption of the research is due to the termination of my research support from the Department of Energy, as well as the rejection of each and every one of the considerable number of inter–related research grant applications filed by our Institute on behalf of internationally renouned, senior, mathematicians, theoreticians and experimentalists.

Most distressing is the language of the referee reports used by governmental agencies for the rejection of all these applications, such as "trash", and other offensive language we shall review in detail in Section 2.5.

The historical legacies of Lagrange, Hamilton and Liouville.

The limitations of the special relativity in classical mechanics are not of my own invention. They are deeply rooted in the history of physics. In fact, they are a modern day version of legacies of the founding fathers of science that have remained opened to this day.

Some of the legacies directly related to the limitations of the special relativity are those of the founding fathers of analytic mechanics, Lagrange and Hamilton, and of a founding father of statistical mechanics, Liouville (see, for instance, memoir [39], Section 2.1).

Contemporary analytic mechanics is based on equations called precisely Lagrange's and Hamilton's equations. When these equations are formulated for three–dimensional Euclidean space and time, they constitute the analytic foundations of Galilei's relativity. When the same equations are formulated for Minkowski space–time (in a special version due to subsidiary constraints), they provide the foundations of the special relativity.

In all cases, the equations are based on the knowledge of the total energy of the system, that is, the sum of the kinetic energy and the potential energy of all action–at–a–distance forces.

In the preceding section, we have shown that the breaking

of Galilei's relativity in Newtonian mechanics is due to the fact that Newtonian forces, in general, are not derivable from a potential, and they are of potential type only in special cases.

In the transition to the breaking of the special relativity, the dynamical origin is essentially the same. In fact, it is associated to contact effects (deformations, motion in resistive media, etc.) which do not admit a potential energy.

The knowledge of the total energy is then insufficient to represent the system in its entirety owing to the presence of internal nonpotential interactions that are outside the capabilities of the Hamiltonian function (Figures 1.3.2 and 1.4.3).

This situation implies the inapplicability of the analytic foundations of the Galilean and of the special relativity, because Lagrange's and Hamilton's equations of the contemporary literature are unable to represent the equations of motion in their entirety.

The situation is not new. In actuality, it was known before the conception of the special relativity, and, predictably, it was identified by Lagrange and Hamilton themselves. For these reasons, the case is known under the name of "legacy of Lagrange's and Hamilton's" (see ref. [39], p. 1700).

I took my Ph. D. in theoretical physics in the town (Torino, Italy) where Lagrange lived and wrote his most important papers. Being interested in mechanics, it was my duty to study Lagrange's original work (some of which was published in Italian).

Unlike numerous contemporary physicists (see below), Lagrange was fully aware of the fact that part of the forces of the physical world are of potential type and part are not. For this reason, he formulated his famous equations with external terms representing precisely the nonpotential forces. It has been only since the beginning of this century that Lagrange's equations have been "truncated" with the removal of the external terms, by acquiring the form generally used in the contemporary physical (and mathematical) literature.

The situation for Hamilton's equations is similar. In fact, the equations were also originally written with external terms. Only since the beginning of this century the external terms have been "truncated", by restricting the representational capabilities to systems with only potential forces.

The legacy of Lagrange and Hamilton is now clear. In fact, whenever the external nonpotential terms are re–established according to their original conception, the invalidation of the Galilean and of the special relativity follows from numerous technical reasons independent of those indicated earlier (for instance, external terms in Hamilton's equations imply a violation of the Lie character of the theory; see ref. [8], p. 300).

The legacy of Liouville is the statistical counterpart of that of Lagrange's and Hamilton's. For brevity, the interested

reader is referred to ref. [39], p. 1702.

The attitude of ethically sound scholars toward the limitations of the special relativity.

The situation depicted in this section is routinely accepted by all ethically sound scholars.

As limpidly expressed by Einstein himself, the special relativity was specifically conceived for point–like particles moving in empty space. As a consequence, the relativity is intrinsically unable to describe extended–deformable particles moving within inhomogeneous and anisotropic material media.

Physicists interested in the advancement of scientific knowledge are expected to disagree on the appropriate form of generalization. But the insufficiency for extended–deformable particles of a relativity conceived for point–like particles is out of the question for all ethically sound scholars.

The posture of dishonest academic barons in face of the limitations of Einstein's special relativity.

Unfortunately, the acknowledgment of the limitations of the special relativity is the exception, and the suppression of the information, or its distorsion or adulteration is more likely the rule, particularly in high ranking academic circles in the U.S.A.

My hope is that the fellow taxpayer will initiate actions aimed at a containment of academic dances perpetrated with the intent of protecting vested interests, in disrespect of the proper use of public funds.

The elements to corner the corrupt academic baron have already been provided for the classical profile of the problem (see Section 1.6 for the quantum mechanical one).

Suppose an academician tells you that Einstein's special relativity is perfectly fine in classical mechanics and that its alleged limitations are nonsensical.

Then the fellow taxpayer is recommended to ask the same academician to prove that the special relativity can describe the re–entry of satellites in Earth's atmosphere. The academic baron at this point will likely retort by saying that this is not a relativistic system, that is, the speeds are minimal; Newton's equations of motion are enough; and there is no need to use the special relativity.

Fellow taxpayer, I beg you not to be blinded by these academic dances of mumbo–jumbo talk. An essential part of the special relativity is the Galilean particularization for low speeds. All low speed systems violating the Galilean relativity constitute direct violation of the special relativity. Period! The rumors emanating from the vocal cords of the academic

baron have therefore no scientific meaning.

You should then insist and not leave the issue open–ended. Consult an engineer or a military expert on drag (such as satellites and missiles in atmosphere). Ask these applied scientists the equations of motion describing the system (you will generally see integral equations approximated via power series expansions in the velocities which have lately reached the fifth and even the sixth power). Confront the academic baron with these equations and ask him/her to prove their compatibility with Einstein's special relativity. Chances are that, at the very sight of these equations, the academic baron will remain speechless. His scheme to protect vested academic–fincanical–ethnic interests in disrespect of human values is then proved beyond a reasonable doubt.

The satellite during re–entry is only one case. Numerous other ways to confront seemingly dishonest academicians have been provided in these pages, such as: particles experiencing deformations; the motion of extended objects within inhomogeneous and anisotropic, material media; the dependence of the speed of light on local physical quantities; etc. All these classical phenomena are simply outside the technical capabilities of the special relativity. Period! The efforts to retain old knowledge as much as possible and at whatever cost is nothing but a manifestation of scientific dishonesty.

A small "pearl": the episode of my visit to L. C. Biedenharn at Duke University.

The following small "pearl" may be appropriate for the closing of this section.

In spring 1981, I decided to visit Larry C. Biedenharn, Jr., of the Department of Physics of Duke University in Durham, North Carolina. My primary motivation was of experimental character. In fact, while under a research contract with the Department of Energy, I was studying the problem of testing the possible alteration of the magnetic moments of nucleons under the condition of the controlled fusion (Section 1.2) via the so–called neutron interferometric techniques.

As indicated earlier, the alteration of the magnetic moments is expected to be due to the breaking of the rotational symmetry. In turn, the ultimate physical origin of such a breaking can be seen in the non–conservation of the angular momentum of a satellite during re–entry.

L. C. Biedenharn is a leading expert in the rotational symmetry, having published two monographs in the field [20, 21] and many different articles. I had met him the first time at a Conference in Coral Gables, Florida, in 1968. Our contacts had then increased in time. In 1978, Biedenharn had accepted my invitation to become a member of the Editorial Council of a

Journal in theoretical physics and applied mathematics (called the "Hadronic Journal") I had organized while at Harvard. Our relationship at that time could not possibly be more cordial, cooperative, and mutually respectful.

My tasks in visiting Biedenharn at Duke were: (a) to analyze the dynamical origin of the breaking of the rotational symmetry in classical mechanics; (b) to review the on–going studies on the generalizations of the rotational symmetry for systems with non–conserved angular momenta, and, most importantly, (c) to review with him in detail certain particle experiments via neutron interferometry that were apparently indicating a breaking of the rotational symmetry in quantum mechanics. In particular, as we shall see in Section 1.7, the confirmation o r disproof of these experiments would resolve the crucial problem of alteration of the magnetic moments under the fusion conditions.

The schedule of my visit had been all prepared in advance, and consisted of arrival in the morning, deliver a seminar in the afternoon, and then spend the following morning in technical discussions on the experimental test of the rotational symmetry in particle physics.

I therefore drove one and one–half days with the old Cadillac of the Hadronic Journal, to reach Durham, N. C. from Boston, MA. My arrival was on schedule. At the time of my seminar, I noted a rather unusual lack of physicists in an otherwise well populated department. In fact, only three people entered the conference room, L. C. Biedenharn, one of his friends (of whom I do not remember the name) and A. A. A., a young European physicist then visiting Duke University.

My seminar lasted well below 60 seconds. I began by recalling the Skylab re–entry and by drawing an idealized trajectory on the blackboard expressing the decay of the angular momentum, with consequential, manifest breaking of the rotational symmetry. At these latter words, I was attacked in a hardly believable way, primarily by Biedenharn's friend although Biedenharn himself participated with evident side on the criticisms. A. A. A. was so shocked by the situation that he remained totally silent for the entirety of the episode.

The criticisms were those reported earlier in Section 1.3. All my attempts at bringing Biedenharn and his friend to scientific reasons were shattered by an ever increasing tone of their voices.

At one point, at the peak of his furor, Biedenharn's friend lost control of himself, and unmasked the true reason of his criticism. In fact, I still remember when, turning his head toward Biedenharn, he acknowledged that the breaking of the rotational symmetry for the satellite during re–entry is a starting point for insufficiencies of Einstein's special relativity!

A constructive scientific process genuinely intended for

the pursuit of novel physical knowledge was naive and laughable under these circumstances. I broke the chalk and terminated this useless session.

I then drove to my hotel with A. A. A. where I expelled some of my rage. Once alone, A. A. A. asked me questions. Being employed under a contract with the U. S. Government, I could not lie. At any rate, this young fellow was capable of smelling problems miles away. In this way a European physicist became aware that considerable public sums were used by the Department of Physics of Duke University on research projects crucially dependent on the exact validity of the rotational symmetry in particle physics. The manifestly uncooperative attitude during my efforts to appraise the limitations of the symmetry, and the continued use of public funds while the symmetry is manifestly broken in our classical world, created an evident problem of scientific accountability at Duke University.

On the subsequent morning, I cancelled the research session, and left as early as possible, with the firm determination never to return to Duke University.

A few years later, as reported in Section 2.5, more serious episodes forced me to ask Biedenharn to terminate all scientific and human contacts.

1.5: THE INCOMPATIBILITY OF EINSTEIN'S THEORY OF GRAVITATION WITH THE PHYSICAL UNIVERSE.

Academic politics in gravitation.

I believe that, among all branches of contemporary physics, the general theory of relativity is, by far, the most controlled by vested, academic–financial–ethnic interests and, therefore, it is the least scientifically sound.

I have written only one paper in gravitation, ref. [40], and soon thereafter I decided to abstein entirely from any additional contribution in the field. This decision was the result of rather incredible excesses I have personally experienced in the denial of incontrovertible physical evidence, and in the lack of scientific process of due examination and rebuffal of published critical studies.

Contemporary views on gravitation are, therefore, the most representative of the current totalitarial condition of the U. S. physics. The views are simply imposed via shear academic power and control of the various aspects of research (jobs, papers, grants).

Predictably, among all the branches of physics supported by governmental agencies, gravitation is, by far, the most questionable. In fact, to the best of my knowledge, governmental agencies continue to disperse public funds to leading academicians on gravitational theories that have been proved to be fundamentally inconsistent in refereed technical journals, while these critical studies continued to be totally ignored.

This situation, which is per se distressing, is compounded by the virtual total lack of any possibility of improvement of the scientific accountability in the use of public funds. In fact, governmental agencies act on the basis of peer reviews by leading scholars in the field. In turn, these leading scholars have proved beyond a reasonable doubt their lack of cooperation and desire to initiate a scientific process in technical journals of due examination of the inconsistencies of Einstein's gravitation accumulated in the recent decades. Such a very tight governmental—academic circle then implies the continuation of the status quo ad infinitum.

Owing to this situation, the most drastic possible recommendations of this book have been made precisely for the funding of research in gravitation. In fact, in Section 3.3, I have recommended the initiation of class actions against federal agencies by organized groups of taxpayers to halt the monopolistic funding of models proved to be inconsistent in refereed journals. The circles of governmental—academic interests are such that, lacking suitable class actions, the unperturbed dispersal of public money in seemingly erroneous theories, and the suppression of potentially fundamental advancement, will continue indefinitely.

The purpose of this section is to provide the taxpayer with elements of judgment whether this situation is real or only imaginary. For this purpose, we must first clear Albert Einstein of any wrong doing, the responsibility of the situation being exclusively in the hands of academic barons currently in control of the field. We shall then go at the roots of the technical problem, by comparing current views in gravitation with the physical reality.

As the taxpayer will see, the basic ideas are readily understandable with a minimum of openmindedness toward science, and without any need of a Ph. D. in gravitation.

The ethical and scientific stature of Albert Einstein.

Albert Einstein has reached a towering stature in history, not only because of his physical intuitions, but also because of his scientific and human integrity.

Such an integrity transpares from his writings to this day in a number of ways, beginning with the identification of the limits of applicability of the theories he considered, and then passing to a critical self—examination of the results. By com-

parison, most of the contemporary papers and books in physics lack even the intention of implementing this ethical process, let alone its realization.

In the preceding section, I have recalled the identification by Einstein of the physical arena of applicability of the special relativity. In regard to his general theory, Einstein used to compare the left–hand–side of his gravitational equations to the left wing of a house made of "fine marble", and the right–hand–side of his equations to the right wing of a house made of "base wood".

This was one way to express his uneasiness, that is, the existence of yet unsettled problems. As we shall see in a moment, subsequent studies proved Einstein's doubts to be correct, by therefore confirming his ethical and scientific vision.

Einstein was also known for having stated that the society of true researchers has very few members at all times. This statement could not be more significative for the contemporary U. S. physical community!

The separation of the problem of gravitation into an exterior and an interior part.

Astrophysical bodies, such as the sun, the planets, and far away stars, consist of a region of space occupied by the bodies themselves, and the surrounding space permeated by their gravitational field. The former region characterizes the interior problem of gravitation, while the latter region characterizes the exterior one.

This distinction is evident. The interior region is the minimal surface where the totality of the mass lies. As a result, it is the region where the gravitational field is expected to originate. The exterior region is that experiencing the propagation of the field.

This distinction of gravitation into an exterior and an interior problem was fully identified in the early stages of the theory, although the distinction has progressively disappeared in subsequent treatments, up to the current condition of virtual complete silence in the contemporary literature.

In this presentation, I shall return to the original conception of the theory, and consider separately the two problems.

The main ideas of the general theory of relativity for the exterior gravitational problem.

By putting aside technical aspects, Einstein's gravitational equations represent the equality of two quantities. The left–hand–side (called Einstein's tensor G_{ij}) characterizes the curvature of space via a suitable geometric structure, as one form of

representing the presence of gravitation (see, for instance, ref.s [26, 27]).

The right—hand—side represents all possible sources of the field, that is, mass (expressed via the matter tensor M_{ij}), and total electromagnetic quantities such as total charge, total magnetic moment, etc. (represented via the electromagnetic tensor T_{ij}).

The equations are then given by $G_{ij} = k(M_{ij} + T_{ij})$, where k is a certain constant (inessential for this presentation). Since the theory considered here is purely classical, contributions from short range, particle interactions are ignored.

When studying the exterior problem of gravitation, the mass contribution disappear and the equations reduce to the simpler form $G_{ij} = kT_{ij}$. In fact, as recalled earlier, mass is contained in the interior problem.

Finally, when the total electromagnetic quantities of the body considered are null (null total charge, null electric and magnetic moments, etc.), the term T_{ij} is also null. Einstein's equations then reduce to the form, $G_{ij} = 0$.

We reach in this way a most representative hypothesis of Einstein's general theory of relativity, that the gravitational field has no source in the exterior problem considered. It is a purely geometric quantity represented by the local curvature of space (or metric).

At any rate, even when the total charge and magnetic moment are not null, their contribution is truly minimal, particularly when compared to that of the mass. As such, it can be ignored in first approximation. The equations $G_{ij} = 0$ then hold for the exterior problem of virtually all astrophysical bodies.

A typical example is the gravitational field of our earth. As we all know, the intensity of its magnetic field is truly small, particularly when compared to the value of the total mass of our planet. As a result, the contributions, say, in the moon's orbit due to the earth's magnetic field is ignorable. A similar situation holds for earth's total charge. The reduced equations $G_{ij} = 0$ therefore represent the true, ultimate, foundations of Einstein's gravitation.

The irreconcilable incompatibility of Einstein's exterior gravitation with the charged structure of matter and Maxwell's electromagnetism.

Consider an astrophysical body with null total electromagnetic phenomenology. Even though the total charge is null, that body is made up of a very large number of elementary charges of opposite sign.

This charge structure of matter begins to manifest itself at the level of the structure of the atoms composing the body. In fact, as we all know, atoms are composed of peripheral electrons

of negative charge and of a nuclear structure of positive charge.

The charge structure of matter manifests itself a second time in the structure of the nucleus, which is composed of protons (positively charged) and neutrons (neutral).

The same charge structure finally manifests itself a third time, at the level of the structure of each nuclear constituent. In fact, recent experiments in particle physics have established that protons and neutrons are composite states of charged constituents.

The theory of electromagnetism, called Maxwell's theory, establishes beyond any possible doubt that, even though the total charge of the astrophysical object is null, the electromagnetic field (say, E_{ij}) due to the oppositely charged constituents is not null. Explicit calculations show that, such a field E_{ij} is so large, that can conceivably account for the entire (gravitational) mass of the object. Einstein's equations $G_{ij} = 0$ must then be replaced with the equations $G_{ij} = kE_{ij}$.

The only possibility for this field to be very small (and, thus, ignorable) is to have a sufficiently small number of charged constituents moving at sufficiently small speeds. These conditions are not verified in astrophysical bodies.

The only possibility for this field to be identically null is when all charges are superimposed in the same point without relative motion. These conditions are also not realized in ordinary astrophysical bodies.

We must therefore recognize the existence of an electromagnetic field due to the charged constituents of matter that, not only is large, but it can be so large to account for the total mass of the object, that is, its total gravitational field.

This situation establishes the irreconciliable incompatibility of Einstein's entire gravitational theory with Maxwell's electromagnetism.

The invalidation begins with the exterior gravitational equations for the bodies with null total electromagnetic data, $G_{ij} = 0$. In fact, a null, total electromagnetic field for the charged constituents of the body, $E_{ij} = 0$, would require a radical revision of Maxwell's electromagnetism, contrary to over one century of experimental verifications.

The invalidation then continues for the case of bodies with non–null total electromagnetic data (i.e., a non–null total charge and magnetism), $G_{ij} = kT_{ij}$. Even though the addition of the tensor T_{ij} representing these total quantities is correct, the lack of the tensor E_{ij} for the charged constituents persists, by keeping in mind that E_{ij} is much bigger than T_{ij}, as indicated earlier.

To put it differently, in order to achieve consistency with the physical reality, it is not sufficient to consider only the total values of charge and magnetism. Instead, a consistent theory must consider the contributions due to charges and magnetic mo-

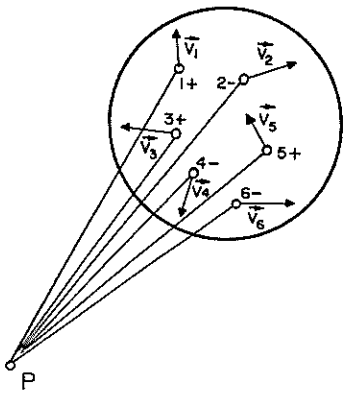


Figure 1.5.1. A reproduction of Fig. 1, p. 111 of ref. [40] illustrating the invalidation of Einstein's gravitation due to the charged structure of matter. The figure provides a schematic view of one neutron as a collection of charged constituents in highly dynamical conditions. Even though the total charge is null, at a point P outside the neutron the electromagnetic field due to the charged constituents is far from being null. Calculations conducted in ref. [40] for the simpler case of the neutral pion indicate that this electromagnetic field can be so large to account for the entire gravitational mass of the particle considered. An extrapolation to astrophysical bodies then leads to the presence of a large electromagnetic field which is missing in the right–hand–side of Einstein's gravitation, as well as in the virtual entirety of current extensions (e.g., of gauge type) and generalizations (e.g., of supersymmetric type). All these models have been proved to be incompatible with the charge structure of matter. Despite a considerable propagation of the information via distribution of preprints, reprints, letters, etc., the inconsistency has been ignored since its appearance, and continues to be ignored in contemporary papers, books and research contracts in gravitations. Any evidence to the contrary would be gratefully appreciated.

ments of each individual constituent of the body (or at least approximate them via suitable statistical means). Once this more appropriate approach is followed, the contributions due to total charge and magnetic moment follow as a consequence.

Finally, the invalidation involves the ultimate foundations of the theory, the interior equations $G_{ij} = k(M_{ij} + T_{ij})$, as indicated below in more details.

The incompatibility of Einstein's gravitation with Maxwell's electromagnetism was established in paper [40].

The litany of theoretical and experimental inconsistencies of Einstein's exterior gravitation identified by the U. S. physicist H. Yilmaz.

The invalidation of Einstein's gravitation due to the charged structure of matter is only the beginning of the problematic aspects.

A truly considerable number of additional, independent inconsistencies have been identified by the U. S. physicist H. Yilmaz (see ref.s [41–48] and quoted papers).

These deficiencies are of both theoretical and experimental character. In fact, the studies identify additional, inconsistencies of the right–hand–side of the equations (that made of "base wood" according to Einstein himself). In addition, and most importantly, the studies disprove beyond a reasonable doubt that the theory verifies the celebrated gravitational tests, contrary to a rather popular belief.

The deficiencies of Einstein's gravitation focused by Yilmaz were long suspected, as well as, at times, considered in incidental ways. Yilmaz has been the first, to my best knowledge, to articulate them into a coordinated construction encompassing all possible aspects. Also, Yilmaz has not limited the analysis to unproductive criticisms, but has worked out a significant generalization of the theory.

Quite intriguingly, Yilmaz's studies [41–48] are in agreement with the invalidation of the right–hand–side of Einstein's equations studied in ref. [40].

Since the financial and ethical implications of Yilmaz's studies are considerable, it is important for the fellow taxpayer to have an outline of them.

Yilmaz's submission of papers to the Hadronic Journal.

I first met Yilmaz back in 1979 when I was a member of the Department of Mathematics of Harvard University. He came to visit me in my capacity of editor in chief of the Hadronic Journal.

Among his several papers in gravitation, Yilmaz did submit and publish a number of papers in the Hadronic Journal [44, 45, 46]. This gave me a rather unique, dual opportunity, the first, as an individual physicist who has studied his work, and the second, as an editor who has contacted referees, studied their reports, consulted them by phone for elaboration and proof of their statements, etc.

The academic politics on Yilmaz's research.

This situation has also given me a direct, rather unique experience of the decaying of ethics in the U.S. physics. Renowned physicists currently controlling gravitation are generally uncooperative and some become even hysterical at the very mention of the studies. My insistence in due scientific process of critical examination of dissident views and presentation of counter–criticism in published articles,has generally failed.

After almost one quarter of a century from their original publication [41], "leading" physicists in gravitation still con-

tinue to ignore completely Yilmaz's work, that is, they continue to ignore research challenging their own work.

Almost needless to say, nobody is asked to accept passively Yilmaz's theory or any theory for that matter. Nevertheless, physicists working in conventional gravitational models under federal support have a strict ethical duty, first, to quote Yilmaz's work, and then to disprove it. Yilmaz's work invalidates conventional models, that is, it challenges the ultimate reasons for the granting of federal support to begin with. Silence on his work is therefore strictly unethical.

I often wandered why this silence has been kept for so long. One possible explanation is due to the fact that no counter–criticism truly exists on Yilmaz's work to this writing. I am not referring to counter–criticism ventured in academic corridors, or in adulterated reviews of research grant applications. I am referring to serious counter–criticism published in refereed journals.

Central aspects of Yilmaz's analysis.

The central aspects of Yilmaz's critical examination of Einstein's studies are the following.

1) Einstein's assumed that matter only is responsible for space–time curvature. The stress–energy of the gravitational field itself was omitted from both the conceptual structure of gravitation and its mathematical realization;

2) Einstein did not equip his gravitational theory with a clear, unique, operational procedure for measurement which is compatible with that of the special relativity.

From these two basic deficiencies, a number of physical mismatches and inconsistencies follow throughout the entire theory, to the point of rendering it unusable for a genuine representation of gravitation.

Inability of Einstein's gravitation to recover the Newtonian description of the planetary motion.

The omission of stress–energy (represented with the tensor t_{ij}) implies the inability of the gravitational equations to recover the Newtonian description of the planetary orbit. This point has been proved by Yilmaz beyond a reasonable doubt in paper [46], although the arguments are included in his earlier work.

The fellow taxpayer should recall the fundamental character of the Galilean–Newtonian description of planetary sys-

tems, stressed in Sections 1.2 and 1.3. In fact, no gravitational theory can be considered valid unless it is compatible with the Galilean–Newtonian description. After all, this description is established by centuries of experimental observation. All other theories, including the general theory, are mere refinements.

Yilmaz has essentially proved that the nonrelativistic limit of Einstein's general relativity is not Newtonian mechanics, but the so–called Hooke's mechanics. This is a mechanics in which the sun has infinite inertia, and the law of action and reaction is generally absent.

This point can be anticipated by any physicist with a minimum of knowledge of both Newtonian mechanics and Einstein's general relativity. The former is centrally dependent on the capability to represent orbital motion via Hamilton's equations (Section 1.3). On the contrary, the latter is known to lack a consistent Hamiltonian formulation (e.g., because the Hamiltonian is, in general, identically null). The incompatibility of the two theories is therefore predictable. Yilmaz has been the first to prove it in all necessary technical details.

Incompatibility of Einstein's gravitation with the special relativity.

One of the main properties of the special relativity is the capability of providing a consistent relativistic generalization of the Galilean relativity (this is the reasons why the special relativity, when inapplicable, can be at most claimed to be approximated, but not as being "wrong").

In particular, the special relativity succeeded in achieving a consistent relativistic formulation of the conservation laws of total energy, linear momentum, angular momentum, charge, etc.

Another point achieved by the special relativity is the proper generalization of the process of radiation of electromagnetic waves, for instance, by an accelerating electron. This is an historical success of the special relativity inasmuch as quantum mechanics had to be constructed precisely in order to understand the lack of radiation from the electrons of the atomic structure.

Yilmaz has achieved a proof beyond reasonable doubts that Einstein's general relativity is unable to reach these fundamental physical properties at the relativistic limit of null curvature. I am differentiating here the nonrelativistic/Newtonian limit of the preceding comments from the relativistic setting under consideration here.

More specifically, Yilmaz has proved that the general relativity is unable to recover the energy–momentum conservation laws of the special relativity. Only the rest–mass conservation law is recovered by Einstein's gravitation, but this is known to violate the special relativity. Yilmaz has furthermore proved that the origin of this occurrence is, again, the lack of the stress–

energy tensor in the right—hand—side of the gravitational equations.

Yilmaz has furthermore proved that the general relativity is unable to provide a proper representation of the phenomenon of radiation of energy, already within a curved framework, with consequential inability to recover the relativistic treatment of radiation for null curvature. Yilmaz has also established that this additional inconsistency is, again, due to the lack of the stress—energy tensor.

Incompatibility of Einstein's gravitation with experimental tests on gravitation.

In the Newtonian mechanics there are three kind of masses, the "inertial mass", the "passive gravitational mass" and the "active gravitational mass". They are all equal among themselves. This property is called in the literature the "strong principle of equivalence".

Yilmaz has proved that the general relativity violates the identity of the active and passive gravitational masses of the same body, and that this is due, again, to the lack of stress—energy tensor in the right—hand—side of the equations.

One of the most visible and important consequences is the inability of Einstein's gravitation to represent the experimental information on the perihelium of Mercury, contrary to a long standing claim by vested interested in the field.

According to historical experimental evidence accumulated throughout the centuries, the perihelium of Mercury advances 575" per century. The first point the fellow taxpayer must know is that the major portion of this advancement, 532", is fully representable by the Galilean—Newtonian formulation of gravitation. In fact, an advancement of 532" per century has long been established as being due to the Newtonian perturbation of the other planets (mostly Jupiter and Venus).

The problem facing Einstein was the representation of the remaining 43". Yilmaz has essentially proved that Einstein's gravitation does recover the small, relativistic correction of 43", but it is unable to represent the primary, nonrelativistic contribution of 532"!

The ultimate reasons can now be seen from the known lack of a Hamiltonian formulation of the general theory of relativity, which implies the lack of a Hamiltonian formulation at the Newtonian limit. In turn, this implies the inability to represent the primary contribution of 532".

This logical line of scientific thought has been bypassed until now via quite involved argumentations aiming at the derivation of a consistent Newtonian—Hamiltonian formulation, from an inconsistent gravitational—Hamiltonian one. Yilmaz has however proved that these salvage attempts are per se plagued by

a host of direct and indirect inconsistencies. The simple scientific truth is that the general theory of relativity violates the Hamiltonian character of mechanics. Period.

But this is only the beginning of the experimental insufficiencies identified by Yilmaz. Another insufficiency is the inability to provide a consistent interpretation of the celebrated bending of the light rays when passing near the surface of the sun, the earth or any other astrophysical body. This is due to the inability of the theory to achieve the identity of the passive and active gravitational mass. As a result, the currently available "explanation " of the bending of the light rays, when worked out in details, implies an infinite value of the mass of the attracting body, contrary to the finiteness of the masses in the physical reality.

Numerous additional experimental inconsistencies have been identified by Yilmaz, but they are omitted here for brevity.

Incompatibility of Einstein's gravitation with quantum mechanics.

This additional incompatibility has been known for decades. It is due to numerous technical problems in achieving a consistent formulation of Einstein's equations in the formalism of quantum mechanics (operators acting on Hilbert spaces; see next section).

This additional incompatibility acquires particular relevance in this presentation because it completes the range of incompatibilities of the theory with the remaining branches of physics describing orbital motion.

In fact, from the studies under consideration it emerges that Einstein's general theory of relativity is incompatible with Maxwell's electromagnetism, it is incompatible with the Galilean—Newtonian formulation of planetary motion and its experimental data; it is incompatible with the special relativistic formulation of dynamics; and, finally, it is incompatible with the quantum mechanical formulation of the same dynamics.

Yilmaz has, of course, considered the latter incompatibility. His contribution is the identification of the origin of the incompatibility which, again, has resulted to be the lack of stress—energy tensor of the gravitational field in the right—hand—side of Einstein's equations.

Yilmaz's "new theory" for the exterior problem of gravitation.

By far the most important contribution by Yilmaz has been the construction of a significant generalization of Einstein's gravitation for the exterior case. In fact, as it is the case in any valuable scientific occurrence, the identification of the insuffi-

ciency of Einstein's theory was merely introductive to the constructive part.

In essence, Yilmaz has generalized Einstein's field equations $G_{ij} = 0$ described earlier into the more general form $G_{ij} = k\, t_{ij}$, where t_{ij} represents the stress–energy tensor of gravitation, and k is a suitable constant.

Yilmaz has therefore proved that the generalized theory (which he calls "the new theory") is compatible with:

1) the Galilean–Newtonian description of planetary dynamics;

2) the special relativistic description of planetary dynamics;

3) the generalization of planetary motion offered by quantum mechanics.

The capability of Yilmaz's new theory of being consistent with available experimental evidence on gravitation is then a consequence. I remember, both as a physicists and as an editor, to be keenly interested in inspecting, verifying, and re–verifying the experimental consistency of Yilmaz's new theory. My original doubts had to give the way to the physical evidence originating not only from my own study of the issues, but also from (ethically sound) referees of his articles submitted for publication to the Hadronic Journal.

The technical reasons for such, rather astonishing successes of Yilmaz's theory are, again, conceptually simple (although predictably involved on technical grounds). The addition of the stress–energy tensor t_{ij} to the right–hand–side of the equations essentially implies the regaining of a consistent Hamiltonian formulation, that is, the theory can be consistently represented via the knowledge of the total energy of the system, when properly expressed in a curved space–time.

Such Hamiltonian character has first the merit of permitting a ready compatibility with the Galilean–Newtonian description of the orbital dynamics. In fact, the theory was consistently Hamiltonian to begin with, and remains Hamiltonian at the nonrelativistic level. Most importantly, this implies the capability of the new theory to represent consistently the Galilean–Newtonian contribution of 532" per century in the advancement of the perihelium of Mercury, as well as all other nonrelativistic experimental data.

Secondly, the Hamiltonian character achieves compatibility with the special relativity, including the relativistic formulation of conservation laws, the gravitational extension of the famous formula $mc^2 = E$, etc. Again, the special relativity is of Hamiltonian character (although of a particular type due to

contraints). The important point is that such character persists in the transition from the special to Yilmaz's new theory, while it is violated in the transition from the special to Einstein's theory.

Most importantly, this latter consistency permits the achievement of a representation of the relativistic correction to the Newtonian experimental data, such as the representation of the additional 43" per century in the advancement of the perihelium of Mercury.

Finally, the restoration of the Hamiltonian character of the theory permits Yilmaz's new theory to achieve compatibility with quantum mechanics. This can also be understood by the general public without the need of graduate studies in theoretical physics. As all other branches of physics considered here (such as Galilean and relativistic mechanics), quantum mechanics is fundamentally dependent on the Hamiltonian character of the theory. In fact, most of the known methods of quantization are dependent on the existence of a Hamiltonian description. Lacking a consistent Hamiltonian formulation, Einstein's theory resulted to be incompatible with quantum mechanics. Owing to the presence of a consistent Hamiltonian description, Yilmaz's new theory, instead, is compatible with quantization.

Limitations of Yilmaz's revision of Einstein's exterior gravitation.

It is the fate of all physical theories to possess specific limitations, insufficiencies and drawbacks. As predictable, Yilmaz's revision of Einstein's exterior gravitation does not escape this fate.

To the best of my understanding, the major problematic aspect of Yilmaz's approach is that it is not fully compatible with the electromagnetic fields E_{ij} of the charged structure of matter. In fact, owing to certain technical reasons, Yilmaz's stress–energy tensor t_{ij} cannot be identified with E_{ij}, i.e., $t_{ij} \neq E_{ij}$. This signals the lack of terminal character of Yilmaz's approach as predictable. In fact, his equations for the exterior problem, $G_{ij} = k\, t_{ij}$ (for null, total, electromagnetic fields T_{ij}), need a suitable modification of the right–end–side to incorporate the tensor E_{ij}.

Despite this limitation, Yilmaz's approach remains preferable over Einstein's theory. In fact, Einstein's equations for the exterior problem (also for the case $T_{ij} = 0$) read $G_{ij} = 0$, by therefore resulting to be "irreconcilably incompatible" with the charged structure of matter, as stressed earlier. Yilmaz's revision $G_{ij} = k\, t_{ij}$ is manifestly better, e.g., because the tensor t_{ij} may well incorporate at least part of E_{ij}.

A number of additional problematic aspects also exist for Yilmaz's approach, but they are of technical nature and not conducive for this presentation.

NEWTONIAN LEVEL	GALILEAN RELATIVITY
RELATIVISTIC LEVEL	EINSTEIN–LORENTZ–POINCARÉ RELATIVITY
GRAVITATIONAL LEVEL	EINSTEIN–YILMAZ RELATIVITY ?

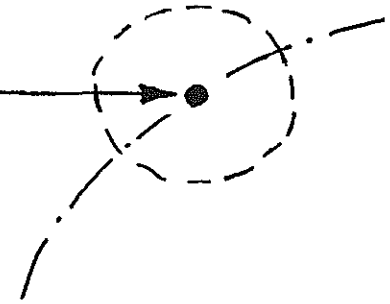


Figure 1.5.2. A schematic view of the status of our classical descriptions of particles that can be well approximated as massive points while moving in empty space, at the nonrelativistic, relativistic and gravitational levels. Each level is characterized by the applicable relativity. Also, the relativity of each level is a covering of that of the preceding level in the sense indicated in Section 1.4. The fundamental relativity is the Galilean one, followed by the Einstein–Lorentz–Poincaré relativity for speeds approaching that of light which, in turn, is a particular case of Yilmaz's revision of Einstein's exterior gravitation. While the Newtonian and the relativistic levels are fully resolved to this writing (for the physical conditions considered here), the gravitational level is, by far, unresolved, as elaborated in the preceding text for the exterior problem (see below for additional problematic aspects related to the interior problem).

The possible elimination of the problem of unification of gravitation and electromagnetism.

As well known, the problem of the unified theory vexed Einstein in the last year of his life. As also well known, Einstein failed to reach a solution of the problem. After Einstein's death, numerous additional attempts were made throughout a number of decades without major results.

The combination of the research of ref.s [40] and [41–48] apparently removes the existence of the problem.

Again, the conceptual bases are simple and understandable to all. As indicated before, Einstein did not consider the charged structure of matter in his gravitational theory. This led him to the inconsistency pointed out earlier, but also to a fundamental misrepresentation of the problem of unification.

Owing to the way gravitation had been approached, Einstein faced two different fields, the gravitational and the electromagnetic fields (plus short range, quantum mechanical interactions here ignored). Along these lines, it was rather natural to look for the "unification" of the two fields into a single entity.

When the problem of gravitation is approached as in this section, beginning with the primary contribution from the electromagnetic field of each matter constituent, the perspective

of the problem is fundamentally changed.

In fact, the studies of ref. [40] were presented as a theory on the "origin of the gravitational field". Most importantly, the contributions from the charged constituents of matter resulted as being able to account for the entire gravitational mass of the bodies. This implied the possibility to "identify" the electromagnetic and the gravitational field. Under these conditions, their "unification" becomes not only unnecessary but actually meaningless.

More particularly, paper [40] proposed a theory whereby the gravitational field is identified with (a particular form of) the electromagnetic field of the charged constituents of matter, plus short range particle contributions. The curvature of space–time is a mere consequence of the intensity of the electromagnetic field of the matter constituents.

Short range, particle contributions must evidently be taken into account, but they are a consideration of the interior problem (see below). So far, we have considered only the exterior problem. It is evident that, at large interplanetary distance, only the long range electromagnetic field of the matter constituents is present in a direct form.

I have halted, years ago, the research on this possible resolution of the historical problem of unification, and no active studies have been conducted by other researchers along the same lines to my knowledge.

The reasons for the truncation of research of such manifestly relevant character have been indicated earlier.

Physical resolutions cannot be achieved alone. They demand a scientific process involving the entire physics community in the sector, and comprising a variety of steps, such as: verbal consultations with colleagues; constructively critical analysis of preliminary results; constructive refereeing processes in the submission of papers and of federal grant proposals; achievement of consensus on the conduction of new experiments; etc.

In my personal opinion and experience, each of these essential aspects is unrealizable in contemporary U. S. physics for all research that is contrary to vested, academic–financial–ethnic interests.

In fact, all my attempts to contact leading U. S. physicists in gravitation for advice and constructive criticisms have resulted in failure after failure repeated over a rather extended period of time. Whether intended or accidential, this has the result of suppressing, jeopardizing or discouraging any study that might even remotely lead to a generalization of Einstein's idea.

Scientific accountability in gravitation research.

Physical research is (hypothetically) based on freedom, but also implies precise responsibilities of scientific and societal character.

Whenever a physicist uses public funds, he automatically acquires a direct responsibility of societal character known as scientific accountability.

Among the multiple duties of scientific accountability there is that of taking in due consideration ALL dissident views on his/her own research. This duty alone is of multiply nature. In fact, it demands the quotation of the dissident views in ALL scientific material, from grant applications, to papers, to books, to talks, etc. Furthermore, it demands publication of disproofs of dissident views whenever the later are published in refereed journals.

The dimension of the ethical responsibility of researchers using public funds evidently varies from case to case. There is first the case of initiation of dissident views published, say, only once or just appeared in print. It is understandable that in this case researchers may not necessarily be aware of dissident views on their work. Then, there is no violation of scientific ethics, provided the researchers, when informed of the dissident views, acquire a documented record of active cooperation, examination and eventually disproof also in refereed journals.

To be repetitive in this crucial aspect of scientific ethics, when dissident views are published in refereed journals or other equivalent scientific vehicles, counter–criticism cannot be limited to exchange of informal letters, or to corridor's talks, but MUST be presented in the same scientific vehicles of the original criticism: refereed publications.

It is evident that the problem of ethics grows with time. In fact, when the original dissident views have been published, republished, treated, and retreated by an increasing number of independent authors, the problems of scientific ethics and accountability grow proportionately.

The tactics used by leading gravitational experts to avoid knowledge of dissident views.

Yilmaz's new theory, by now, has been published, and quoted in papers spanning about one quarter of a century.

It is evident that, under these circumstances, Yilmaz's studies constitute a sizable problem of ethics for ALL physicists conducting research in Einstein's gravitation under public support. The ignorance of Yilmaz's studies simply magnifies the ethical problems.

As well known, corrupt academicians are masters in denying knowledge of undesired lines of research. Such denial, however, is simply untenable for the case of Yilmaz's studies for any physicist who can qualify him/herself as an "expert" in gravitation. This is due to the following reasons.

Authors of dissident views generally enter into a progressive and intensive propagation of the information of their

work. The first action is that of mailing a preprint to most of the leading physicists in the sector asking for advice in the revision and completion of the manuscript.

When this first step remains without acknowledgments, the action is generally continued by mailing copy of the reprint of the published version of the paper, and again asking for the courtesy of comments. The assumption is that academicians are generally very busy and do not visit libraries. They must therefore receive directly on their desk copies of papers presenting criticisms of their work.

But, academicians do not read papers (or at least so they claim whenever convenient, just to claim the opposite one minute thereafter, whenever they need qualifications for passing judgment . . . .). As a result, the original mailing of preprints, followed by the mailing of the reprints, is generally complemented with a third action consisting of a letter summarizing the essential elements of the dissident views, and, again, asking for the courtesy of counter–criticisms whether or not these (by now published) views have sense.

The understanding is that, if the academicians do not read preprints and reprints, they may well read a nice, personalized, individualized letter. Right? Wrong! Academicians do not read even letters addressed to them, of course, when containing undesired scientific lines. At least this is a logical conclusion whenever you see that their subsequent papers are totally silent on published dissident views.

At this point, the dishonesty of the academicians can be considered as proved beyond any reasonable doubt. Then, what do you do? Dishonesty feeds on silence which is, therefore, complicity. So, you decide to talk. But to whom? You cannot approach other academic barons because the loyalty of academic alliances is known to be so strong to dwarf that in organized crime.

These are the roots of the problem of ethics in U. S. physics. These are also some of the reasons why this book was written.

To my knowledge, Yilmaz and/or his friends (including myself) have exhausted all possible or otherwise conceivable means for the propagation of the information on the studies. As a result, no true expert in the field can claim lack of their knowledge at this time.

I have followed the iterim of exhaustive information on dissident views not only for the case of the invalidation of Einstein's gravitation due to the charged structure of matter, but also in other cases. (See, for instance in Section 1.6 the case of the paper of criticism on quarks distributed in 15,000 copies).

The roots of the ethical problem in U. S. gravitation.

Let us now focus our attention on the problem of ethics in gravitation caused by:

1) the publication in refereed technical journals of a truly considerable number of independent invalidations of Einstein's gravitation for over one quarter of a century;

2) the rather sizable propagation of the information to individual researchers in the field done independently by Yilmaz, myself and others; and,

3) the rather complete silence in technical papers, books and talks by leading U. S. physicists in gravitation on the above problematic aspects.

No physicist who is mentally sound will ever ask passive acceptance of these invalidations. But then, no physicist who is ethically sound can continue to ignore them for decades after decades.

But after decades and decades of impunity, there are no reasons to expect changes in the behaviour of governmental–academic circles. After all, why should an academician change his/her posture if he/she continues to enjoy governmental support? Similarly, why should governmental agencies change their own posture if they continue to receive positive reviews by leading peers?

These are the reasons why I have recommended the fellow taxpayer in Section 3.4 to organize class actions aimed at the truncation of the use of public money in the unilateral funding of research on Einstein's gravitation without any consideration of its published inconsistencies.

The uncooperative attitude by S. Deser, A. Pais, S. Weinberg, and J. A. Wheeler from the U.S.A. and Y. Ne'eman from Israel.

I am a physicist. As such, I am primarily interested in constructive research and not in seeking unnecessary scandals that are damaging to all, beginning with myself.

Thoughout the years, I have therefore attempted anything in my power to implement an orderly scientific process, but I have failed.

Even as recently as early 1984, I was still hoping that leading U. S. physicists in gravitation could be brought to scientific reasons; that an orderly scientific process of resolution of the inconsistencies of Einstein's gravitation could be initiated; and that I would have found myself without reasons to write IL GRANDE GRIDO, or at least avoid the writing of this section in gravita-

tion.

Facts proved that my hopes were unfounded.

On January 3, 1984, I wrote a letter to the following leading physicists in gravitation: Stanley Deser of Brandeis University; Abraham Pais of Rockefeller University; Steven Weinberg of The University of Texas at Austin; John A. Wheeler also of The University of Texas at Austin; and Yuval Ne'eman of Tel—Aviv University in Israel.*

As one can see from the Documentation (p. II—708), the letter was written in a way as respectfully as possible; it summarized the scientific lines of this section; it included the most recent preprint and references; and concluded with its most important point: asking for assistance in the organization and conduction of a workshop on all views, in favor and against, the problematic aspects of Einstein's gravitation, and in the publication of its proceedings.

The rationale of the proposal was that the most effective way to initiate the orderly resolution of the issue was precisely via an international workshop with the participation of experts of different views.

All the physicists indicated above answered with a few, dry lines without any scientific content. None of them indicated interest in the organization of the workshop, and some of them did not even acknowledge the petition for its organization.

At the same time, owing to the current totalitarian nature of the U.S. physics, the organization of a workshop without the participation of leading experts in the field has no true weight in the community.

The inclusion of this section on gravitation in this public appeal was therefore unavoidable. My gentle and respectful call for due scientific process to Deser, Pais, Weinberg, Wheeler and Ne'eman was my very last try.

The refusal by the Department of Physics of Boston College to list a seminar by H. Yilmaz in the Boston Area Physics Calendar.

As well known, the Boston area is populated by universities, colleges and research laboratories. *The Boston Area Physics Calendar* is a weekly list of all seminars in mathemati-

* Y. Ne'eman was selected for the mailing of the letter dated January 3, 1984, because, even though he conducts research outside the U.S.A., he has used a considerable amount of money of the U.S. taxpayer both directly (via federal contracts from the international branch of the National Science Foundation dealing with U.S.A.—Israel exchanges) and indirectly (via financial support from U.S. Departments of Physics he has visited throughout the past decades, said support being drawn from governmental contracts). As a result, Y. Ne'eman has acquired a direct scientific accountability with the U.S. taxpayer for his gravitational research.

cal, theoretical and experimental physics, as well as philosophy of science. The Calendar is a very useful guide for all scholars in the area, including visitors. Listings in the Calendar require the mailing or phoning of the information.

Production of the calendar is done by a local Physics Department, which generally changes from one academic year to the next. Subscriptions are granted upon paying a yearly fee.

The production of the Calendar for the current academic year (1983–1984) is done by the Physics Department of Boston College, Chestnut Hill, MA. The editorial responsibility of the calendar rests with S. Lynch, an employee of Boston College, under the supervision of the current chairman of the Physics Department, R. A. Uritam.

Following the publication of his article [46], in early March 1984, H. Yilmaz came to visit me in my capacity of President of the Institute for Basic Research. He wanted to deliver a talk along the lines of his studies entitled "Problematic aspects of the general relativity for planetary orbits". He therefore asked my assistance for the organization of the seminar at our institute in the hope of receiving constructive criticisms, in the interest of a resolution of the historical open problems reviewed in this section.

The seminar was set for March 26, 1984. I therefore wrote a letter to S. Lynch providing the information needed for the listing with copy to Yilmaz. The letter was mailed as a regular first class mail on March 7, well in time for the listing of March 26. The Calendar for the week of March 26–30 DID NOT contain the listing of Yilmaz's seminar because, as indicated by Ms. Lynch, my communication had arrived late for the listing!

We therefore rescheduled the seminar for April 16, 1984. A new communication dated March 27, 1984, was mailed to S. Lynch, this time via certified letter, return receipt requested, with copy to R. A. Uritam as chairman of the Physics Department of Boston College. The Calendar for the week April 16–21, 1984, arrived at the I.B.R. on April 11, 1984. TO MY ENOURMOUS SURPRISE YILMAZ'S SEMINAR HAD NOT BEEN LISTED! The calendar contained no mention of it. The listing had been simply suppressed without any communication whatsoever to our Institute or to Yilmaz (Doc. pp. I–197–211).

I immediately wrote a certified letter, with return receipt requested to Father Donald J. Monan, President of Boston College,* asking for a public investigation of the case, with the soliciation to terminate the employment of all persons responsible for the occurrence.

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*See Doc. p. I–211. Father Monan never acknowledged my letter. One of the first copies of IL GRANDE GRIDO was therefore mailed to the State Department of the Vatican in Rome, Italy, with an accompanying report.

By no means, the fellow taxpayer should think that this is an isolated occurrence. Not at all. In fact, the episode is nothing but a continuation of similar episodes occurred while the Calendar was produced by the Physics Department of Tufts University, as we shall see in detail in Section 2.1. The only difference is that the former episodes have much more serious elements of possible discrimination of research under governmental support (in fact, the seminars refused for listing were under contract with the U. S. Department of Energy!).

The questions raised by Yilmaz's case are evidently endless. Did Boston College act alone, or was the decision to refuse the listing reached under consultation and possible complicity of other local departments? As we shall see in Section 2.1, at the time of the incidents with Tufts University, the chairman of that physics department disclosed that the prohibition to list I. B. R. seminars under D. O. E. support had been voiced by senior members of the Department of Physics of Harvard University. Any investigation of Yilmaz's case must therefore clarify, in a way as open to the public as possible, whether or not Harvard University and /or other local colleges were also responsible.

I hope the fellow taxpayer will not be blinded by "explanations". The Boston Area Physics Calendar has been published since its inception in a very informal (simply typed) way, without ever indicating restrictions for listing, and with the illusory face of democracy. At any rate, restrictions in the listings would invalidate the very title of "Boston Area Physics Calendar".

Since the Boston College (as well as Tufts University) never released any indication of the reasons for the lack of listings, we are currently unaware of covert legal aspects. But, fellow taxpayer, bear in mind that, even assuming that Boston College and the other local universities will one day be claimed to be right by a Court of Law, the episodes are and will remain strictly undignifying for America! If nothing else, where is the alleged, traditional, scientific hospitality in the U.S.A.?

The refusal by Boston College (and Tufts University) to list seminars by renowned scholars is only one of the too many episodes providing a clear, cold blood, identification of the decaying status of the U.S. physics community.

But why reach such hysterical extremes? The most plausible reasons are obvious to me. The physicists who suppress due scientific process are not stupid or uneducated. They are fully aware that Yilmaz's criticisms of Einstein's ideas are correct and incontrovertible. This is why they retort to covert suppression of scientific process. They have no other choice. It is all done in full knowledge, in plain daylight, and, most regrettably, with our own money.

I hope, fellow taxpayer, you begin to see the reasons why, by being silent, I could not look at my children with clear eyes.

Enough is enough. The control of science by such academic–financial–ethnic greed in the U.S.A. has simply passed the limits of human decency, and must be halted at whatever cost. Only the accomplices can tolerate it.

The irreconciliable invalidation of Einstein's gravitation for the interior problem.

Despite their number, diversification and relevance, all the invalidation arguments considered until now constitute only half of the presentation. In fact, the arguments deal exclusively with the exterior problem of gravitation. The remaining half is evidently that of the interior problem.

The irreconciliable invalidation of Einstein's equations for the interior problem of gravitation is established quite forcefully by the mere inspection of physical reality, not that of far away stars (as preferred by several academicians), but instead that of our earth.

Interior trajectories are those within our atmosphere, or, more generally, those of extended objects moving within a resistive medium, such as satellites during re–entry.

As indicated in the preceding sections, these systems violate the foundations of the Galilean and of the special relativity. The violation of the general relativity is a mere consequence.

When approximated via local power series in the velocities, the equations of motion are simply outside the technical capabilities of the general relativity. Any other view is a mere attempt to manipulate fundamental human knowledge.

It is sufficient to recall the episode of Skylab during re–entry (Section 1.3). No matter what treatment is used, the general relativity simply cannot represent this motion in any meaningful way (this was the reason why the NASA scientist would have chased out of NASA premises the professor expert in current theories of gravitation . . . .).

What Einstein did for the interior problem was to assume an idealized situation whereby astrophysical bodies are made up of massive points, much along the conceptual lines of the special relativity. The important aspect (that re–inforces rather than weakens Einstein's ethical stature) is that he stressed the limited capability of the theory.

The responsibility of bringing the theory to the current religious level lies entirely in his followers.

It is evident that, for the idealized body made up of massive points, the action can only be at a distance, whether in flat or curved space–time. But nature is much more complex than that. In fact, the forces of the physical reality are not necessarily of action–at–a–distance type.

Simple inspection of our environment proves it, by establishing the irreconcilable inability of the general theory of relativity to represent the physical reality of the interior problem

of gravitation.

The invalidation of the Riemannian geometry for the interior gravitational problem.

All dynamical formulations are based on a given geometry. This is the case also of Einstein's gravitation. Its underlying geometry is called Riemannian and essentially consists of mathematical formulations suitable for the representation of a curvature in space–time. The geometry is of the so–called local and differential character, in the sense recalled in Section 1.3.

To avoid an insidious misconception, we must now go back and reconsider first the exterior problem of gravitation. Then we shall consider the interior problem on a comparative basis.

Einstein's biggest contribution to gravitation has been the left–hand–side of his equations for the exterior case. It introduced for the first time the Riemannian geometry for the treatment of gravitation.

The aspect that must be clarified to avoid unnecessary misrepresentations, is that the Riemannian geometry is fully valid for the exterior problem of gravitation. In Einstein's own words, the left–hand–side is the left wing of the house made of "fine marble". All criticisms reviewed above deal exclusively with the right–hand–side of the equations, that is, with the source terms.

The physical reasons of consistency can be readily understood. When considering the exterior gravitational problem, whether in flat or curved space, we are dealing with objects moving in empty space. Then (see Section 1.3), the actual shape and structure of the bodies do not affect the dynamics. The bodies can therefore be approximated as being massive points, along Galilei's vision. Under these conditions, the geometry can indeed be local and differential.

The selection of the Riemannian geometry is then a mere technical consequence.

In the transition from the exterior to the interior problem, the situation becomes fundamentally different. In the interior problem, we do not have any more points moving in empty space. We have instead extended objects experiencing contact effects besides action–at–a–distance ones. This is the case for satellites during re–entry, or for the atoms in the interior of the sun, or for neutrons in the interior of a neutron star.

In every case, we have objects with a finite, extended, character experiencing collisions with other extended objects. These phenomena simply cannot be reduced to massive points.

A study of the situation soon reveals that the primary characteristics of the Riemannian geometry, its local and differential characters, fail to be effective for the new physical situation considered. In fact, interior trajectories such as those

of satellites during re–entry, demand integro–differential equations, that is, equations having integral and differential terms. The applicable geometry is then expected to be of at least integro–differential type, although a full integral geometry is expected to be more appropriate (Section 1.8).

Mathematical studies on the construction of such geometries have already been initiated in the mathematical literature. Nevertheless, to my best knowledge, we do not possess to this writing a generalization of the Riemannian geometry which, on one side, constitutes a generalization of the Riemannian one, and, on the other side, permits an effective treatment of the interior problem of gravitation. Indications of suitable geometries would be gratefully appreciated.

Lacking the underlying geometry, we simply have no way to construct a meaningful gravitational theory for the interior problem.

In short, for the exterior problem, we do have a promising theory: Yilmaz's revision of Einstein's theory. For the interior problem, instead, we have no consistent theory to this writing. This is the reason why, in my own solitary efforts, I had to start with the attempt to generalize Galilei's relativity. The corresponding generalization of the special relativity (also for interior trajectories) is the second problem. The achievement of a consistent generalization of Einstein's interior gravitation can be tackled only upon achieving consistency in the preceding two layers of physical reality.

The legacy of Cartan.

The invalidation of the general theory of gravitation in the interior problem is not my own invention. Instead, it was identified by one of the founders of geometry, Cartan, and is known today as the "legacy of Cartan" (see, for instance, ref. [39], page 1712).

In fact, Cartan had indicated that the Riemannian geometry is unable to recover Newton's equations of motions at the limit of null curvature. This is evidently due to the infinite variety of possible Newtonian forces with arbitrary functional dependence in the velocities and other physical quantities, when compared with rather restricted rails of the Riemannian structure.

It is very regrettable that the legacy of Cartan is ignored in the virtual totality of scientific literature in gravitation except rare occasions.

The incompleteness of this presentation.

As done for the relativistic case, I must stress again the incompleteness of this presentation and apologize with all authors I have regrettably not quoted at this time.

NEWTONIAN LEVEL	GALILEI–ADMISSIBLE RELATIVITY ?
RELATIVISTIC LEVEL	LORENTZ–ADMISSIBLE RELATIVITY ?
GRAVITATIONAL LEVEL	LOCALLY LORENTZ–ADMISSIBLE RELATIVITY ?

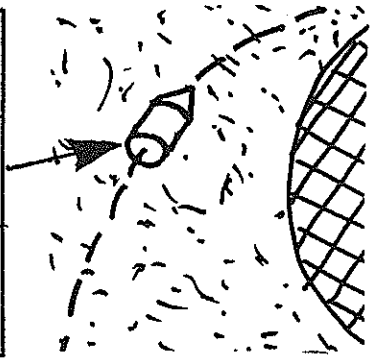


Fig. 1.5.3. A schematic view of the insufficiency of our current knowledge for the classical description of extended–deformable particles moving within inhomogeneous and anisotropic material media, as typical for all levels of interior trajectories, the Newtonian, the relativistic and the gravitational one. None of the relativities for the exterior case (Figure 1.5.2) is now applicable because of inconsistencies pointed out in the text. Only very preliminary and tentative studies are available at this writing for the applicable relativity. In the exterior case, a central problem is the interpretation of the stability of the orbit of particles under central–force fields. This stability is interpreted via the conservation of the angular momentum which, in turn, is represented via the symmetry under rotations. The Lorentz symmetry follow for the relativistic extension. As a result, a necessary condition for an exterior gravitational theory to be consistent is that it is locally–Lorentz, that is, it recovers the special relativity in the neighborhood of each space time point. In interior trajectories, the central problem is the representation of time–rates–of–variations of angular momenta due to contact effects in such a way to admit the conventional conservation as a particular case. A conjecture to develop a generalization of Galilei's relativity along these lines (called Galilei–admissible relativity) has been submitted in ref. [8]. The corresponding relativistic case has been touched in ref. [12]. The gravitational case has not been considered so far, to my best knowledge. One aspect is however known. Any gravitational theory, to be physically meaningful for the interior case, cannot be locally–Lorentz in character, that is, it MUST NOT admit the special relativity in the neighborhood of each point. In fact, such locally–Lorentz character implies, in particular, the local validity of the conventional rotational symmetry, that is, the local conservation of the angular momentum. The incompatibility of the general relativity for interior trajectories (such as Skylab during re–entry) is therefore due precisely to the locally–Lorentz character of the theory. If the conjecture of building a Galilei–admissible relativity will be proved meaningful, and admitting of a relativistic extension, then, the interior gravitational theory can be constructed accordingly, that is, by searching for a theory that is locally–Lorentz–admissible in character. Note that Yilmaz's "new theory" holds only for the exterior problem. In Section 1.6.1, I shall outline the inconsistencies of the reduction of the non–Hamiltonian interior trajectories of the real world to Hamiltonian trajectories of the constituent particles (which is at the basis of the current, widespread use of the Riemannian geometry for the interior problem).

However, unlike others, I am fully cooperative for the

remedial of my faults. I therefore invite all interested authors to let me know of their work on the limitations of conventional, exterior and interior gravitational theories. I shall than take all the necessary initiatives for their proper quotation in future work. At the Institute for Basic Research in Cambridge, we are interested in organizing reprint volumes of all relevant articles in the problems considered in this section. It will be my duty to make sure that all relevant articles brought to my attention are reprinted or at least properly quoted in such review volumes.

A first group of contributions here considered relevant are those identifying explicitly the limitations of available gravitational theories.

A second group of relevant contributions are those generalizing available exterior gravitational theories along the lines considered in this section, Yilmaz's revision of Einstein's gravitation (with corresponding revisions of gauge, supersymmetric and other models).

A third group of relevant contributions are those treating conceivable generalizations of the interior gravitational problem along the lines indicated earlier, that is, in such a way to achieve the capability of crude, but meaningful treatments of interior trajectories (satellites during re—entry, damped oscillators, decaying spinning tops, etc.).

Need for the taxpayer to exercise care in the acceptance of views by so—called "experts".

If you submit to The Physical Review a paper on the inconsistencies of Einstein's gravitation, the editors will inevitably send your paper to the leading "experts" in gravitation at leading colleges (Harvard University, the Massachusetts Institute of Technology, Yale University, and a few others). The rejection of the paper is then inevitable.

If you submit a research grant application to Governmental Agencies (such as the National Science Foundation or the Department of Energy) also on the limitations of Einstein's gravitations, you would also be waisting your money and time. The application would also be submitted to leading "experts" at leading institutions. The chances of acceptance are so minute to be ignorable.

This is the way U. S. physics is structured and operates at this time

Dear fellow taxpayer, you can do much better in the selection of "experts" and in the verification of their qualifications PRIOR to accepting their judgment.

To be qualified "experts on the limitations of Einstein's gravitation", physicists must have a record of publication of papers in refereed journals specifically in the limitations themselves.

Therefore, fellow taxpayer, PRIOR to accepting judgments on the inconsistencies of current gravitational theories, I urge you to ask for documentation of qualification. If the guy presents you a long list of publications in famous journals, do not be blinded. Keep going. Ask first for inspection of at least ONE publication in a refereed journal, and then request that passages be shown to you containing explicit words such as "invalidations", "inconsistencies", "incompatibilities" of Einstein's gravitation and similar sentences. If these physical problems are not addressed directly and explicitly, chances are that you are not facing a scientist.

Of course, ethically sound scholars in conventional gravitation do exist in the U.S.A. When consulted in the limitations of their own work, these people generally identify explicitly in the report their vested position, and stress the partial value of their view, of course, in favor of old ideas. Judgments of this clean type should indeed be considered and respected. The point is that no mature judgment can be achieved based only on them. Judgments by true experts in the inconsistencies of Einstein's gravitation, remain the most important ones.

After all, the formers discourage, while the latters promote advancements of physical knowledge.

Comments on the books in gravitation by Weinberg, by Misner–Thorne–Wheeler, and by Pais.

As indicated earlier, a most distressing aspect of gravitational literature is the lack of quotation of the problematic aspects of Einstein's general theory, which therefore acquires the artificial vest of perfection and terminal character.

In turn, the presentation of fundamental physical theories without the joint treatment of their limitations is one of the most antiscientific possible practices, inasmuch as it can assassinate at birth all sparks of creativity, particularly in young readers. As such, possible scientific services are overshadowed by the antiscientific aspect of preventing or otherwise discouraging advances.

This is by and large the status of the virtual totality of books in gravitation written by contemporary leading experts (evidence of the erroneous nature of this statement would be gratefully appreciated).

This presentation would have no value without specific cases of physically identified authors.

Among a variety of choices, I would like to comment on the following three books.

In 1972, I was intensely involved in the preparatory work of paper [40] (which was subsequently printed in 1974). The appearance at that time of book [26] in gravitation by S. Weinberg, then at Harvard University, was for me a rather shocking

experience. I had been warned by B.B.B., a graduate student in physics who had attended the lectures on gravitations by Weinberg. At that time,* B.B.B. was also interested in fundamental open problems of gravitation. He communicated to me a sense of anguish in listening to Weinberg's lectures because of the presentation of Einstein's theory with a sort of an iron curtain of totalitarial validity, without a spark of possible fundamental advances.

The reading of Weinberg's book confirmed these feelings. Most distressing for me was the presentation of the terminal character, not only of the general theory of relativity, but also, and primarily that of the special relativity. I subsequently learned that B.B.B. and myself were not the sole people to read Weinberg's book with a sense of distress. In fact, I now know of a number of authors who have quoted Weinberg's book essentially along these critical lines. But, in 1972 Steven Weinberg was a distinguished professor of physics at Harvard University. I therefore kept my impressions to myself and remained silent.

Only one year passed and then there was the appearance of the rather massive book in gravitation by Charles W. Misner of the University of Maryland, Kip S. Thorne of the California Institute of Technology, and John A. Wheeler, then at Princeton University (ref. [27]).

At that time, I was working at the final drafting and re–drafting of paper [40] as well as at the preliminary elements of monograph [11].

Again, I was shocked by the presentation of Einstein's special and general relativities as terminal descriptions of nature, without any meaningful hint of their limitations.

Perhaps too pessimistically, I recalled B.B.B. who had left physics in the meantime, and I imagined a negative impact of book [27] in the minds of countless young readers throughout the world.

This time I decided to initiate at least some action of containment of the scientific damage I was expecting from book [27]. I therefore began the drafting of a critical analysis of ref. [27], that was subsequently published in 1978 in Section 3.4 of ref. [11], page 458 and following.

Prior to releasing the criticism for printing, as scientific ethics demands, I did mail a preliminary copy of the manuscript to each of the three authors.

Regrettably, I have lost the correspondence with the passing of time. Nevertheless, I recall lack of reception of any acknowledgment by W. Misner. I also remember a rather cooperative attitude expressed by the remaining two authors, K. S.

* B.B.B. subsequently abandoned physics for business. I regretted dearly the loss for physics of his young mind which was one of the sharpest and most inquisitives I ever met. Who knows how many other young minds have left the pursuit of novel scientific knowledge for other, more rewarding lives? What an immense loss for America as well as for the human society.

Thorne and J. A. Wheeler, which I report here with sincere pleasure.

But, by far, the most shocking experience was the reception of the more recent book [28] by Abraham Pais of Rockefeller University. As one can see, the manuscript was written some twenty years ago. I have no doubt that, if published at that time, the book would have been scientifically valuable and appropriate.

But the publication of the same book twenty years later had, for me, a most distressing effect. The book is a presentation of Einstein's theories without any mention of the limitations and inconsistencies that have been accumulated during the past twenty years. For instance, book [28] does not quote critical literature on Einstein's theories, such as Yilmaz's work [41–48].

I still remember quite vividly the evening of 1982 when, back home from a long day of study, I found among my mail Pais' book. By scanning through the various sections and the literature, it took me minutes to realize the potentially immense damage to the advancement of human knowledge, if not the creation of a modern obscurantism, that can be promoted by Pais' book, especially at a time in which courageous scientists throughout the world are resolving some of the limitations of Einstein's relativities.

I therefore went into my room, I locked the door, and, with this book on my knees, I cried.

Note added in proof: the generalization of Einstein's gravitation for the interior problem by the Italian physicist M. Gasperini.

Upon completion of the typesetting of this section, I received a paper by the Italian physicist M. Gasperini entitled "A Lie–admissible theory of gravity", ref. [50], with complementary comments presented in ref. [51].

Gasperini has essentially initiated the generalization of Einstein's interior gravitation indicated as lacking in this section, that incorporating all possible Newtonian systems, as needed for realistic trajectories in the interior problem of gravitation.

In fact, Gasperini's interior gravitation is, first of all, of open/non–conservative character and, second, it is locally Galilean–admissible in the sense of ref. [8], as well as locally Lorentz–admissible in the sense of ref. [12]. As such, Gasperini's relativity enjoys the direct universality of all physical theories possessing a Lie–admissible structure. By comparison, the representational capabilities of Einstein's interior gravitation is of extremely minute nature (in fact, it can represent only interior trajectories of "perpetual–motion–type"). The non–incremental advance from Einstein's to Gasperini's interior relativity is then evident.

1.6 THE AGING OF GALILEI'S AND EINSTEIN'S RELATIVITIES IN PARTICLE PHYSICS.

Scientific, economic and military implications of the validity or invalidity of Einstein's ideas in particle physics.

By far, the most important implications of the validity or invalidity of Einstein's ideas occur in particle physics.

Scientifically, we are talking about the ultimate foundations of current physical knowledge, with a direct or indirect bearing on numerous branches of science, such as theoretical biology or solid state physics.

Economically, Einstein's ideas are known to be at the basis of the nuclear energy and other aspects. Their possible generalization can conceivably permit the discovery of new, more efficient forms of energy currently unthinkable. After all, strongly interacting particles (hadrons) are the biggest energy reservoir known to mankind. With fission and fusion we have barely touched the surface of this reservoir.

Militarily, the implications are equally far reaching. It is today generally believed that only a few nuclei are fissionable and therefore usable to build bombs. If suitable generalized views are valid in the interior of hadrons, new, currently unthinkable weapons could be possible.

Dear fellow taxpayer, I detest weapons as much as you do. But, the security of my children depends on the military strength of America. The inclusion of a military profile in this presentation has been rendered necessary by the rejection of research projects submitted by the Institute for Basic Research to U.S. military agencies (the Defense Advance Research Project Agency—DARPA—and others). The limited information (evidently without any detail) presented in this book is known by a number of foreign physicists. While the U.S. government is apparently not interested in military applications originating from generalizations of Einstein's ideas in the interior of hadrons, other governments may think otherwise. Besides an ethical profile, there is an evident aspect of national security. When vested academic—financial—ethnic interests on Einstein's ideas endanger or jeopardize even minimally the security of this Country, I cannot be silent.

In this section, I shall attempt an outline of the central aspects underlying the above profiles.

Regrettably, all contemporary treatments of particle physics depend on abstract mathematical formalisms in a truly essential way. All nontechnical reviews are therefore inherently deficient. This review is no exception.

The fellow taxpayer, however, should not feel discouraged by the abstract content of this section. In fact, the ultimate physical ideas remain accessible to all. In turn, an understanding of

the basic ideas and of their plausibility (and not of their mathematical treatment) is essential to achieve a mature judgment of the problem of ethics in the scientific, economic and military sectors of the U.S. physics.

Central aspects of nonrelativistic and relativistic quantum mechanics.

Quantum mechanics (see, for instance, the book by Dirac [52]) is often differentiated into nonrelativistic and relativistic formulations. The former is characterized by the applicable relativity, the Galilean one, while the latter is characterized by the special relativity. All formulations are quantum mechanical in the sense that they are characterized by local–differential operators acting on a particular type of carrier spaces called Hilbert spaces (par contre, the corresponding classical formulations are expressed via ordinary functions of local variables).

The formulation of the relativities via operators on Hilbert spaces implies a number of principles which are typical of the particles world, such as: Heisenberg's uncertainty principle (expressing our inability to measure jointly positions and momenta of particles with unlimited precision); Pauli's exclusion principle (expressing the impossibility that more than one identical particle with half–odd–integer spin occupies the same state with given quantum numbers); and others. It should be recalled that the mutual compatibility and inter–dependence of the various parts of quantum mechanics are so rigid, that deviations from any principle would necessarily imply deviations from the underlying relativities, and vice versa.

The mathematical structure of quantum mechanics is characterized by local–differential operators, say, A,B,C, . . .acting on Hilbert spaces over complex numbers. Operators essentially represent physical quantities such as coordinate r, momentum p, energy H, etc. The multiplication of operators is the ordinary product AB verifying the associative rule (AB)C = A(BC). The set of all possible operators A,B,C, . . .equipped with the product AB is called the enveloping associative algebra. Said algebra permits, for instance, the calculation of squares of operators (say, p^2=pp) which, in turn, are generally needed to compute physical quantities (such as, for instance, the kinetic energy T = pp/2m).

Most important equations representing the dynamical evolution of quantum mechanical particles are given by the celebrated Heisenberg's equations. They can be written for an arbitrary physical quantity operator A as $i\hbar\dot{A}$ = AH – HA, where: H is the total energy; AH is the associative product considered earlier; AH–HA is the Lie product attached to the enveloping algebra (see also Section 1.4); and $\hbar$ is Planck's constant.

All space–time symmetries, including the Galilean and the Lorentzian symmetries, are expressed via groups of transformations of the so–called unitary type. They are given by operators of the type $U = \exp(iwA)$ verifying certain conditions.

Whether in nonrelativistic or relativistic mechanics, the time evolution is represented by the unitary transformation $U = \exp(itH)$ where t is time and H is the total energy. For infinitesimal values of time, the unitary time evolution yields precisely Heisenberg's equations which, as such, acquire a fundamental character not only for the representation of the dynamical evolution, but also for the characterization of the structure of the applicable relativities.

An arena of unequivocal applicability of quantum mechanics: the atomic structure.

An arena of unequivocal applicability of quantum mechanics is well known. It is given by systems of particles under electromagnetic interactions, that is, particles which:

A) can be effectively approximated as being point–like;

B) move in empty space conceived as homogeneous and isotropic; and are such that

C) gravitational effects are ignorable.

On a comparative basis with the arenas considered in the preceding sections, we have essentially permitted "quantum effects", that is, processes of emissions and absoption of energy in discrete amounts that are multiples of Planck's constant $\hbar$.

An illustration of the arena considered is given by the atomic structure. After all, we should not forget that the mechanics was conceived and constructed, specifically, for the understanding of the atomic structure, and for that structure it resulted to be correct beyond the best expectations of its builders.

Doubts on the exact validity of quantum mechanics for the nuclear structure.

One of my first duties as a graduate student in theoretical physics was to conduct an in depth study of the application of quantum mechanics to the atomic structure. During these studies, I was soon fascinated by the beauty of the theory as well as by the amount of direct experimental verification, that still impress me to this day.

During the same program, I had subsequently to study the application of quantum mechanics to the different physical arena of the nuclear structure. This time, however, I experienced

considerable uneasiness which has remained with me to this day. The reasons are due to the fact that the physical conditions of the nuclear structure are profoundly different than those of the atomic structure. Even though the approximate validity of quantum mechanics in nuclear physics is, and should remain, unquestionable, the problem of its exact validity remains basically open.

The physical differences between the atomic and the nuclear structure are well known (although rarely emphasized in the contemporary technical literature). The mutual distances of the peripheral electrons in the atomic structure are so large, that the size of their wave–packets can be ignored. In the transition to the nuclear structure, the situation is different. In fact, the constituents of nuclei (protons and neutrons) have extended charge distributions and wave–packets whose size is of the order of 10^{-13}cm. Nuclear volumes are also known. Simple calculations then show that the constituents of nuclei are so close together to be actually in (average) conditions of mutual overlapping of about 1/1000 parts of their volume.

This situation has implications at all levels of study. In fact, while quantum jumps of energy can be readily justified in the atomic structure owing to the distance among stable orbits and their occurrence in empty space, the visualization of the same situation in nuclear structures creates uneasiness. Even though stable orbits may be somewhat conceived,quantum jumps of energy similar to those of the atomic structure are not possible, trivially, because the nuclear volume is filled up with hadronic matter. The nuclear constituents are not, therefore, "free to jump" from one orbit to another. In short, the extended character of the constituents of nuclei and their conditions of mutual penetration creates doubts on the final character of the truly central notion of quantum mechanics, the "quantum" of energy.

Most significantly, while the atomic two–body, the hydrogen atom, admits an infinite, discrete, spectrum of excited states, the corresponding nuclear two–body, the deuterium, has revealed no excited state at all, by therefore resulting to be one, single, unique structure. This differentiation alone was, for me, sufficient to warrant the search for a generalization of quantum mechanics, inasmuch as the nuclear emphasis is in the suppression of the atomic spectrum of energy.

The dynamical roots of possible departures from quantum mechanics: nonlocal effects due to mutual wave overlappings of wave–packets of particles.

Once the conditions of mutual penetration of the wave–packets of protons and neutrons are truly considered, they imply the lack of applicability of the mathematical foundations of quantum mechanics, let alone the mechanics itself. In fact, the

conditions imply the presence of contact interactions which do not admit potential energy (Section 1.3), and thus, cannot be mediated by particle exchange, that is, by exchange of discrete amounts of energy. In turn, contact interactions have a number of implications, such as: the inability to represent the system considered via only one operator, the total energy operator (Hamiltonian); the inapplicability of the local–differential character of the underlying geometry in favor of nonlocal/integro–differential generalizations, etc.

This process of critical examination of the validity of quantum mechanics in nuclear physics should not be misrepresented. In fact, the approximate validity of the mechanics in the arena considered is and remains out of the question. After all, the successes of quantum mechanics in nuclear physics are well known. The problem that is open at this time is the possibility of corrections in the quantum mechanical description of nuclei. Said corrections are expected to be essentially small in value because the conditions of mutual penetration of nuclear constituents are small, as recalled earlier. However, the implications of the corrections would be far reaching, because they would imply a generalization of the ultimate physical and mathematical foundations of the theory.

The expected insufficiencies of quantum mechanics for the interior of hadrons.

In the transition to the problem of the structure of neutrons, protons, and all hadrons, the departures from quantum mechanics are expected to increase. In fact, all strongly interacting particles possess a size which is of the order of magnitude of the range of the strong interactions, about 10^{-13}cm. This implies that the constituents of hadrons are expected to be in conditions of mutual penetration much greater than those of the nuclear constituents. As an example, for a proton and an electron to reach a bound state of the order of the size of the neutron, the two particles must be in conditions of total mutual penetration and overlapping of their wave–packets. The departure of these physical conditions from those of the hydrogen atom are then clear.

It is evident that, while conceivable deviations from conventional relativities and quantum mechanics can be at best small for the nuclear structure, they can be much greater for the hadronic structure.

If we pass to the problem of the structure of the core of stars, say, undergoing gravitational collapse, deviations from quantum mechanics are expected to be maximal, not only because of the additional presence of gravitational effects, but also because of the maximization of the conditions of mutual overlapping of the particles, that is, of the departures from the

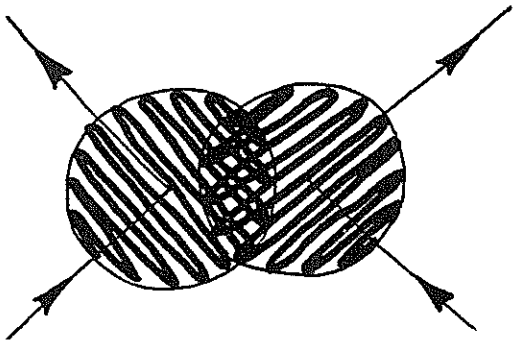


Figure 1.6.1. A reproduction of Table 5, p. 1214 of ref. [53] intended to illustrate the insufficiency of point–like abstractions of particles for a deeper understanding of strong interactions. According to a rather widespread view in contemporary physics, the entire universe can be reduced to a collection of points (resulting into the so–called local theories), with only action–at–a–distance interactions (resulting into theories of potential type). According to this view, the entire universe can be described by only one quantity, the Lagrangian or the Hamiltonian, defined locally, at a collection of distinct points. In fact, all known interactions are today reduced to local–differential and potential treatments. I am referring to electromagnetic, weak, strong and gravitational interactions. Now, the existence of interactions that can be effectively treated via these local–differential and potential techniques is unquestionable, as typically the case of the electromagnetic interactions. However, the existence of interactions which are structurally beyond local–differential and potential techniques is equally unquestionalbe. This is typically the case for the strong interactions whose range is exactly of the order of magnitude of the size of all hadrons, 10^{-13}cm. The diagram above therefore depicts the conditions of mutual penetration of the wave–packets of particles which are necessary to activate the strong interactions. It is then evident to all that wave–packets in conditions of deep mutual penetration cannot be effectively reduced to isolated, dimensionless points, unless extremely crude descriptions are desired. The diagram above therefore identifies the insufficiency of the contemporary reduction of the universe to a collection of isolated points (locality) with only action–at–a–distance interactions (potentiality), in favor of suitable, non–local/integro–differential generalizations. Regrettably, the mere view of the experimental reality depicted by the diagram above generally creates semi–hysterical reactions by physicists with vested interests in local/potential models; by therefore precluding the implementation of a constructive scientific process of trial and error in the selection of the appropriate generalizations. In fact, the diagram presents a visible illustration of the lack of exact character for strong interactions of the most essential structures of contemporary particle physics, the special relativity, quantum mechanics and Lie's theory. Note that the symbol of the I.B.R. is given precisely by two overlapping circles representing hadrons under strong interactions.

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atomic structure (see Figure 1.6.1).

A dominant physical characteristic of all strongly interacting systems is therefore that motion cannot be conceived as

occurring in vacuum, because it occurs in a material medium consisting of other hadrons, called "hadronic medium" [14]. It is evident that this medium is not, in general, homogeneous or isotropic, thus implying the breakdown of the prerequisites for the applicability of the Galilean and special relativities, exactly along the corresponding occurrences in classical mechanics (Section 1.3 and 1.4).

The proposal to construct hadronic mechanics as a generalization of quantum mechanics specifically conceived for strong interactions.

The considerations above identify the following arena of expected insufficiency of quantum mechanics. It is given by systems of extended particles/wave–packets which:

A') cannot be effectively approximated as being point–like;

B') move in inhomogeneous and anisotropic hadronic media; and are such that

C') gravitational effects are ignorable.

A proposal to construct a generalization of quantum mechanics for the broader physical conditions A'), B'), and C') was submitted in memoir [14]. The name of "hadronic mechanics" was recommended for the new mechanics to emphasize the intended applicability of the generalized mechanics only to the hadronic phenomenology, as well as to stress the medium in which motion occurs, the hadronic medium.

Hadronic mechanics was recommended to be a "covering" of quantum mechanics, that is: to apply for physically broader conditions; to possess a mathematically broader structure; and to admit quantum mechanics not only as a particular case, but also in first approximation. The latter requirement is evidently essential to recover the known achievements of quantum mechanics in particle physics (see Figure 1.6.2 for more details).

A comprehensive mathematical, theoretical and experimental program was initiated on the construction of the hadronic generalization of quantum mechanics, as we shall review below in this and the remaining sections of this chapter. Despite these efforts, it must be stressed that the studies are at the beginning and far from being conclusive.

What we can claim today is the mathematical existence and self–consistency of hadronic mechanics, but we do not have conclusive evidence of its effectiveness for the representation of nuclei, hadrons and stars.

The situation for quantum mechanics is essentially the same.

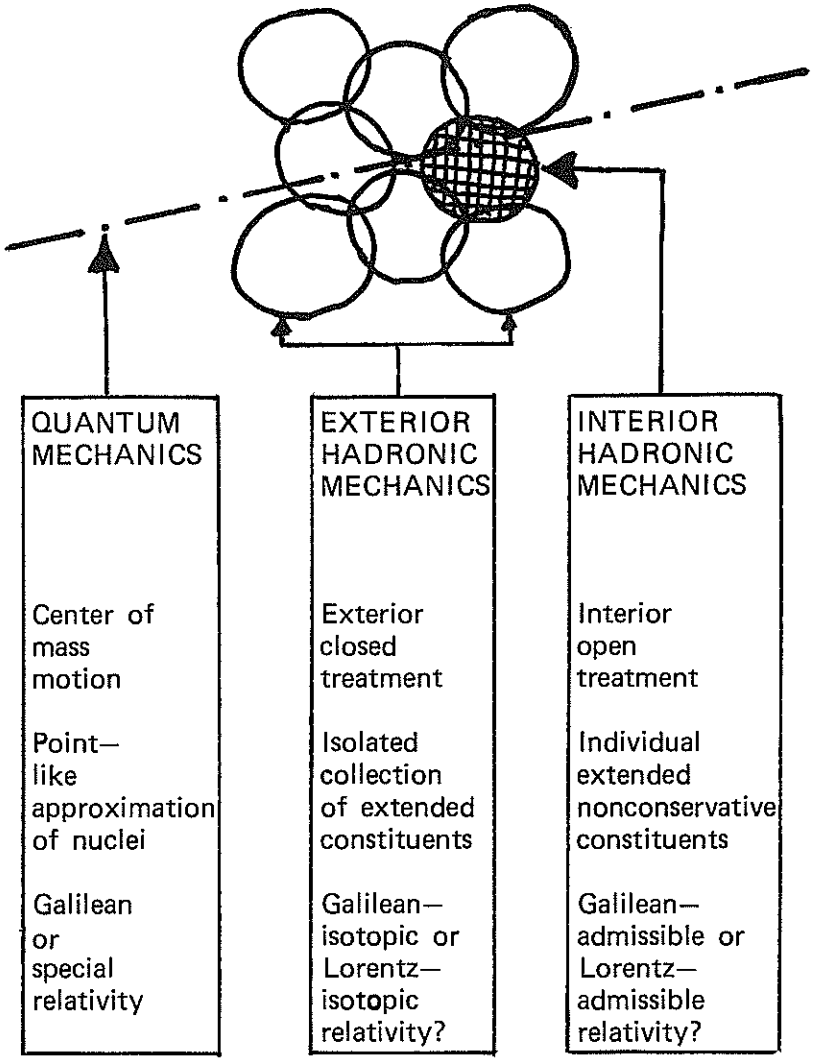


Figure 1.6.2. The three conceivable layers of the descriptions of a system of strongly interacting particles, such as a nucleus or a hadron. First, one can consider the system as moving in empty space under long range electromagnetic interactions. In this case, the system can be approximated as being a massive, charged, point. The theory is purely Hamiltonian, that is, the knowledge of only the total energy H is sufficient to characterize the time evolution of an arbitrary (total) physical quantity A according to the celebrated Heisenberg equations, which I write in the form $idA/dt = A*H - H*A$, where $A*H = A(1/\hbar)H$, and the products are the ordinary associative products. Quantum mechanics then strictly applies, with the underlying Galilean and special relativities. Their time component is given by the exponentiated form of Heisenberg's equations, which I write in the form $A' = \exp(itH/\hbar)A\exp-(itH/\hbar)$. The same mechanics and underlying physi-

cal laws are today assumed as valid also for the characterization of the structure of strongly interacting systems. Quantum mechanics, however, can only represent protons and neutrons (and their constituents) as massive, dimensionless points, as well known. As a result, quantum mechanical models of nuclei are intrinsically unable to represent the extended character of the nuclear constituents and related phenomenology (such as the possible deformability of neutrons and proton when within a nuclear structure, with consequential alteration of their magnetic moments; see below in the main text). Greater insufficiencies occur for the problem of the hadronic structure (see below). In the hope of reaching advances in these latter problems, the construction of a generalization of quantum mechanics under the name of "hadronic mechanics" was suggested in memoir [14]. An objective was that of achieving, in due time, an operator version of the closed, non–Hamiltonian systems of our Newtonian reality, such as our Earth (Section 1.3), where the contact, non–Hamiltonian, internal forces are precisely a representative of the extended character of the constituents. Besides possible new insights in strong interactions, hadronic mechanics could then permit the attempt of regaining the currently lacking unity of physical and mathematical thought (see below in the main text). As well known, under point–like approximation of hadrons, quantum mechanics can characterize both a strong system as a whole and each of its open constituents. Under contact/non–Hamiltonian internal forces among extended constituents, the situation resulted to be different inasmuch as a formulation effective for the exterior, closed, treatment resulted to be not necessarily effective for the characterization of each individual open constituent much along the classical counterpart. The construction of hadronic mechanics was therefore recommended along two different, yet complementary branches, one for the exterior treatment of isolated strongly interacting systems, and one for the complementary interior treatment of each individual open constituent. The emphasis in the former case is therefore in the achievement of total nonconservation laws under non–Hamiltonian internal forces, while the emphasis in the latter case is in the maximization of the nonconservation of the physical characteristics of each constituent, evidently, as a condition to maximize the internal interactions. The exterior–closed treatment was restricted to possess the same mathematical structure (Lie–isotopic theory) of its classical counterpart, the Birkhoffian mechanics [10], while the interior–open description was restricted to possess the same mathematical structure (Lie–admissible theory) of the classical Birkhoffian–admissible mechanics [12].

EXTERIOR, LIE–ISOTOPIC BRANCH OF HADRONIC MECHANICS. An important element of quantum mechanics is the unit. This is an element I of the operator algebra verifying the rules $IA = AI = A$ for all operators A, where the product is the trivial associative product recalled earlier in this section. This unit has fundamental physical relevance inasmuch as it represents Planck's constant. The mathematical relevance is equally fundamental, because Lie's theory, space–time symmetries, and conventional relativities can be constructed beginning from the unit element. A central idea of hadronic mechanics is that of generalizing the unit element I into nontrivial operator forms. For the case of the exterior–closed branch, the generalized unit can be written $\hat{I} = g^{-1} \neq \text{diag}(1,1,1,...)\hbar$, and follows from the generalization of the conventional associative product AB of quantum mechanics into the form $A*B = AgB$, g = fixed, for which $\hat{I}*A = A*\hat{I} = A$. The product $A*B$ is an isotope of AB in the sense that it

preserves the original associative character of the envelope. The new envelope is then called "isoenvelope". The generalization of the quantum mechanical unit implies the consequential generalization of the totality of the theory. In fact, the antisymmetric product attached to the isoenvelope is now given by $A*B - B*A$ and it is still Lie. Physically, this implies the generalization of the fundamental dynamical equations, Heisenberg's equations, into the isotopic form $idA/dt = A*H - H*A = AgH - HgA$ first proposed in ref. [14], p. 752. One can see the need of two quantities to characterize a strong system, the total energy operator H and the isotopic operator g, the latter one representing precisely the internal non–Hamiltonian forces. When $\hat{1} = I\hbar$, $I = diag(1,1,1,...)$, hadronic mechanics recovers quantum mechanics identically. When $\hat{1}$ is close to $I\hbar$, we have small deviations from quantum mechanics (as conceivable in the interior of nuclei) otherwise we have finite deviations (as conceivable in the interior of hadrons and of stars). The generally non–local integro–differentive operator $\hat{1} = g^{-1}$ can therefore be conceived as a generalization of Planck's constant $\hbar$ for particles under mutual wave–overlapping. The total energy is trivially conserved because of the antisymmetry of the product, $idH/dt = H*H - H*H = 0$. The conservation of other total quantities then follows much along conventional lines. In this way hadronic mechanics achieves total conservation laws under non–Hamiltonian internal forces, as desired. Quantum mechanics admits a single infinity of possible models, those characterized by the all possible Hamiltonians H. The exterior branch of hadronic mechanics admits a double infinity of possible models, those characterized by all possible Hamiltonians H and isotopic operator g which must therefore be selected from experimental information on the system considered. The isotopic generalization of Heisenberg's equations admits a consistent exponentiation into a group of non–unitary transformations called unitary–isotopic. In turn, this implies the generalization of the Galilean and special relativities, beginning with their time component, from the conventional unitary form recalled earlier, to the generalized form $A' = \exp(iH*t)A\exp(-it*H)$. The generalization is called Lie–isotopic because it preserves the essential axiomatic structure of Lie's theory. The underlying carrier space of quantum mechanics, the Hilbert space, is also subjected to an axiom–preserving generalization, resulting into a structure called isohilbert space. The quantum mechanical action $A\psi$ of operators A on elements ψ of the Hilbert space is generalized into the isotopic form $A*\psi$ resulting into a generalization of all the remaining parts of quantum mechanics such as Schroedinger's equations, eigenvalue equations, operations on Hilbert spaces, observables, etc. [77]. The compatibility of the exterior branch of hadronic mechanics with the center–of–mass, quantum mechanical treatment has been recently established [55]. I am referring to the proof that generalized quantum mechanical laws for the interior nuclear and hadronic problem (such as generalized Heisenberg's uncertainties) are compatible with conventional quantum mechanical laws for the center–of–mass treatment (such as conventional uncertainties). As a result, the validity of quantum mechanics for the dynamical evolution, say, of one proton in a particle accelerator constitutes no evidence whatsoever for the validity of the same laws for the interior structural problem.

INTERIOR LIE–ADMISSIBLE BRANCH OF HADRONIC MECHANICS. The physical requirement of reaching the nonconservation of physical quantities of ONE individual constituent, is permitted by a dual generalization of the quantum mechanical unit, one for the product to the right, $I^{\triangleright} = f^{-1}$

and one for the product to the left, $\lhd_1 = g^{-1}$, $g \neq f$. In turn, this implies two different isoenvelopes, one for the action to the right $A \rhd \psi = Af\psi$, and one for the action to the left $\psi \lhd A = \psi g A$. Physically, the cases describe evolutions moving forward and backward in time. The cases are therefore connected by time reversal. One reaches in this way a further generalization of Heisenberg's equation of the type $idA/dt = A \lhd H - H \rhd A = AgH - HfA$ first proposed in ref. [14], p. 746, which is called of Lie–admissible type for certain mathematical reasons (see Section 1.8), where H now represents only the energy of the individual particle considered. Its nonconservation then follows from the lack of antisymmetry of the product, $idH/dt = H(g-f)H \neq 0$. A similar situation occurs for other quantities under the evident condition that these internal nonconservations must be compatible with total conservations. The Lie–admissible generalization also admits an exponentiation into the form $A' = \exp(iH \rhd t) A \exp(-it \lhd H)$ which is now no longer of Lie character. This suggested the construction of a further, Lie–admissible generalization of the Galilean and special relativities [14], this time for the characterization of one nuclear or hadronic constituent (rather than a strongly interacting system as a whole). The underlying mathematical structure is called a Lie–admissible bi–module [86–88]. The generalization of the remaining aspects of the Lie–isotopic formulations into the more general Lie–admissible form is then consequential. For a review, the interested reader may consult ref. [78]. Despite its abstract mathematical structure, the interior Lie–admissible branch of hadronic mechanics essentially consists of an algebraically consistent re–formulation of the nonunitary time evolutions conventionally used in quantum mechanical treatments of open, nonconservative, particle reactions. These latter transformations can be written $A' = \exp(i\mathcal{H}t) A \exp(-it\mathcal{H}^\dagger)$, where $\mathcal{H}^\dagger$ is the so–called hermitean conjugate of $\mathcal{H}$. Their infinitesimal version is given by $idA/dt = A\mathcal{H}^\dagger - \mathcal{H} A$, and does not characterize a consistent algebra owing to its trilinear character. The decompositions $\mathcal{H}^\dagger = Hg$, $\mathcal{H} = fH$, $H = H^\dagger$, $g^\dagger = f$, then implies the Lie–admissible form above which restores the bilinearity of the product and the consistency of the algebra. The regaining of a consistent algebra implies the possibility of physical calculations that would be otherwise difficult or impossible [59]. Note that the interior branch of hadronic mechanics is intrinsically irreversible, in the sense that the time evolution of each constituent is generally non–invariant under time inversion even when its Hamiltonian H is time–reflection invariant. Such time–reflection–asymmetry generally disappears in the transition to the exterior, Lie–isotopic form (see below the comments on the origin of irreversibility. Particularly important is the "direct universality" of hadronic mechanics established in 1979 (ref. [39], p. 1820). I am referring to a theorem stating that, under sufficient topological conditions, all possible, generally nonunitary time evolutions can be written in the Lie–admissible form indicated above. The Lie–isotopic and the conventional, quantum mechanical forms are then trivial particular cases. Also important is the property that hadronic and quantum mechanics admit a single, unique, abstract mathematical structure. In fact, the isotopic products are associative in the same measure as that of the conventional product; the isohilbert spaces are Hilbert; etc. Quantum mechanics is the simplest possible realization of these mathematical axioms, while hadronic mechanics is the most general possible realization. The understanding is that a generalization of quantum mechanics is applied only when warranted by sufficient physical conditions. The abstract unification of hadronic and quantum mechanics is the operator counterpart of the corres-

ponding classical occurrence, the unification of Hamiltonian and Birkhoffian mechanics into single, abstract, Lie/symplectic structures. This property therefore confirms the achievement of hadronic mechanics as an operator version of Birkhoffian mechanics.

APPLICATIONS, DEVELOPMENTS AND COMMENTS. The hadronic generalization of quantum mechanics was suggested for the representation of the possible alteration of the intrinsic magnetic moments of protons and neutrons when under sufficiently intense fields and/or collisions, for the possible identification of the origin of irreversibility, for the possible identification of the hadronic constituents with physical, experimentally detected particles, and other primary applications reviewed in the main text. A number of additional applications have also been initiated in the literature, such as the hadronic generalization of gauge theories, of quantum field theory, of the interior gravitation, and others. The theoretical physicists who have primarily contributed to the construction of the hadronic mechanics following proposal [14] are the following: R. Mignani (Italy), G. Eder (Austria), A. Kalnay (Venezuela), M Gasperini (Italy), C. N. Ktorides (Greece), J. Fronteau and A. Tellez–Arenas (France), P. Caldirola (Italy), A. Jannussis (Greece), M. Nishioka (Japan), J. Lohmus, M. Koiv and L. Sorgsepp (U.S.S.R.), Chun–Xuan Jiang (China), E. Kapuscik (Poland), A. Schober and R. Trostel (West Germany), and others. A primary mathematical contribution has been provided by H. C. Myung (U.S.A.). Other mathematical contributions will be listed in Section 1.8. Experimental contributions will be identified in Section 1.7. Regrettably, U.S. governmental agencies rejected a considerable number of research grant applications for the construction of the hadronic mechanics filed over a three year period (from the founding of the I.B.R. in 1981 until 1983). Even grant applications for possible military developments were rejected (see below). A plea to all primary U.S. private foundations resulted to be a total waste of time and money. As a consequence of these rejections, all physical research on the hadronic mechanics has been halted in the U.S.A., but it is continued abroad at a fast growing pace. In fact, at the time of writing this page (May 15, 1984) there is absolutely no U.S. physicist working on the construction of the hadronic mechanics, to my best knowledge (I have myself halted all research in the sector, as indicated earlier). Even the conduction of scientific meetings (Conferences, Workshops and research sessions) have all been moved abroad, evidently, because of the financial impossibility of their conduction in the U.S.A. This condition is per se instructive. In fact, the fellow taxpayer can readily compare the large number of reserach contracts along minute incremental advances on established trends, versus the evident fundamental relevance in the construction of a new discipline. This suppression of research via the systematic prevention of funding is however only part of the issue. To achieve a mature judgment of the current condition of basic physical research in the U.S.A., the fellow taxpayer must be informed of the remaining facets, such as the impossibility of publishing articles in the hadronic mechanics at the journals of the American Physical Society, the impossibility of obtaining jobs, the refusal of academic hospitality for the mere needs of library facilities, and numerous other aspects reviewed in Chapter 2.

The first quantitative predictions of hadronic mechanics in nuclear physics: alterations of spin and magnetic moments under intense, external, strong collisions.

As recalled in Section 1.1, early studies in nuclear physics lead quite naturally to the hypothesis that the value of the magnetic moments of protons and neutrons change in the transition from the electromagnetic to the strong interactions.The hypothesis emerged quite naturally from the fact that total nuclear magnetic moments still escape full understanding [2,3]. As also recalled in Section 1.1, studies of the hypothesis were subsequently halted, apparently because of its implications for academic politics, despite the manifest plausibility and the equally manifest relevance for controlled fusion and other aspects. To this day, the magnetic moments of protons and neutrons have been measured and re-measured countless times, but all times when the particles move in empty space under long range electromagnetic interactions, while no measures of the same quantitity under strong nuclear conditions exist

The studies of the hypothesis were resumed in memoir [14] according to the following main lines.: Quantum mechanics represents protons and neutrons as points which, being dimensionless, cannot be deformed, thus preserving their intrinsic characteristics for the life of the particles. The constancy of the magnetic moments (and all other intrinsic characteristics) then follows under any possible external field.

Memoir [14] suggested the construction of the hadronic generalization of quantum mechanics for the purpose of representing protons, neutrons and all hadrons as they actually are in the physical reality, extended particles with a charge distribution of about two Fermis. The representation of hadrons as extended implies the consequential possibility that they can experience deformations under sufficiently intense external fields and/or collisions. In turn, such a deformation of shape necessarily implies the alteration (called "mutation" in hadronic mechanics) of the magnetic moments.

These results were reached in memoir [14] via the hypothesis that the intrinsic angular momentum (spin) of proton, neutron, and the hadron in general, may experience deviation-mutation from the conventional quantum mechanical values under sufficiently intense collisions with other particles, much along the established classical counterpart. The alteration of spin would then imply the necessary alteration of the magnetic moment.

These are evidently the most general conceivable conditions for the mutation of magnetic moments of hadrons, with nontrivial consequences. In fact, the alteration of spin ½ of the proton or the neutron would imply their lack of strict verification of Pauli exclusion principle, trivially, because the particles are no longer exact fermions. In turn, mutation of spin implies corresponding deviations from the Galilean and special relativities. For these reasons, ref. [14] promoted the test of the special relativity and Pauli principle beginning from the title.

The assistance by distinguished U.S. mathematicians, such as H. C. Myung and others (see Section 1.8), permitted the initiation of quantitative studies [60,61]. The first contact with experiments occurred in paper [62], where the use of available experimental data permitted the fit with the (average) value of spin 0.49777 for neutrons under strong nuclear interactions due to Mu-metal nuclei. In the hope of minimizing possible misrepresentations, it was stressed in the literature, beginning with ref. [14], that the conceivable value of spin 0.49777 was specifically intended for neutrons under the OPEN NONCONSERVATIVE conditions caused by EXTERNAL NUCLEAR INTERACTIONS, and that conventional total value of angular momentum are recovered if one considers the system neutron-nucleus.

These remarks are important, not only to identify the proper conditions for meaningful experiments, but also to maximize the conditions for the mutations of spin and magnetic moments predicted by hadronic mechanics (see Section 1.7).

The second quantitative predictions of hadronic mechanics in nuclear physics: alteration of magnetic moments while preserving conventional values of spin.

In the preceding paragraph, I have reported the state of the art in the problem of mutation of spin and of magnetic moments as of August 1980.

Fundamental advances in the problem were subsequently achieved by the Austrian physicists G. Eder, a senior expert in nuclear physics (see his book [63]). His most important contribution, presented in articles [64, 65, 66], is that the magnetic moments of protons and neutrons can mutate while preserving the conventional value of the spin of the particles. In addition, Eder reached a specific, quantitative, prediction of 1% mutation ("fluctuation" in his words) in the angle of spin precession for neutrons in the intense electromagnetic fields in the vicinity of silicon nuclei (see ref. [65], p. 2031).

Thus, prior to Eder's contributions, the emphasis was first on the mutation of spin under external strong interactions, with consequential mutation of magnetic moments. Eder showed that the mutation of magnetic moments can also occur under sufficiently intense, but purely electromagnetic interactions, without the necessary presence of the strong. In this latter case, the values of spin can remain the conventional ones.

Eder's studies opened up a new experimental orizon we shall review in the next section. At this moment, we indicate the following hierarchy of descriptions and related experimental verifications.

First, we have protons and neutrons (as well as any other hadron) moving in empty space under interactions that do not

imply an appreciable deformation of their shape. Under these conditions, the particles can be well approximated as being point–like. Quantum mechanics then strictly applies, jointly with the preservation of conventional values of the magnetic moments. A large body of experimental verifications exist for these conventional conditions, as generally reported in nuclear physics books.

Second, we have the conditions discovered by Eder, whereby the value of the spin of protons and neutrons remains ½, but the value of the magnetic moments is altered because of deformations of the shape of the particles and other dynamical effects. Since the value of the spin is not changed, the protons and neutrons under these conditions are expected to obey Pauli's exclusion principles. The mutations can be measured directly via the so–called neutron interferometer experiments. Most importantly, the predictions of hadronic mechanics are well within available experimental capabilities. Even more importantly, the cost of the experiments is truly minimal (in the range of $ 50,000) particularly when compared to the large costs of current high energy experiments (that can reach millions of dollars).

Third, we have the full case of memoir [14], interactions and/or collisions sufficiently more intense than those of the preceding level, to cause an alteration of the value of the spin, with consequential mutation if the magnetic moments and departures from Pauli's exclusion principle. These latter predictions can be today tested via the scattering of sufficiently energetic neutrons on tritium and other means, as we shall see in the next section.

Hadronic regeneration of space–time and internal symmetries that are quantum mechanically broken.

One of the biggest misrepresentations of the studies on the construction of hadronic mechanics is the alleged intention of the theory to "break" fundamental space–time and other symmetries. This misrepresentation generally occurs because of lack of knowledge of the available literature (or because desired for reasons of academic politics).

The reality is the opposite of that. Hadronic mechanics offers genuine possibilities of regenerating space–time and other symmetries that are broken at the level of quantum mechanics.

The rotational symmetry is the best illustration of this occurence. Consider a proton or a neutron, and assume that they are perfectly spherical (which is already debatable to begin with), i.e., they have the structure discussed in Section 1.4: $R' R = xx + yy + zz = 1$. In this case, the conventional rotational symmetry is exact.

Suppose now that the particles experience a deformation of their shape due to external forces and collisions, as indicated

earlier. Assume the simplest possible deformations, those into ellipsoids. Then the sphere is replaced by the equations also considered in Section 1.4: $R'gR = xa_1x + ya_2y + za_3z = 1$, where the a's are positive–definite quantities expressing the three principal axis of the ellipses, and the metric g generally depends on all possible local quantities, such as coordinates R, speeds $\dot{R}$, etc., $g = g(R, \dot{R}, \ldots)$.

Under these conditions, the rotational symmetry is manifestly broken. After all, the symmetry was conceived for point–like particles. For extended–deformable particles, ethically sound physicists may disagree on the appropriate generalization, but not on the breaking of the conventional rotational symmetry at the quantum mechanical level.

The main idea of the generalized rotational symmetry suggested by hadronic mechanics for extended–deformable particles is the following. It is that given by the Lie–isotopic generalization of Lie's symmetries discussed in Section 1.4. It begins with the generalization of the associative algebra, from the trivial form AB of quantum mechanics to the less trivial form $A*B = AgB$ of hadronic mechanics, where g is precisely the metric of the deformed shape of the particles. It then implies the generalization of each and every aspect of the conventional rotational symmetry, from the unit, to the group structure, to the Lie algebra, to the representation theory, etc., as presented in ref. [19, 32, 54].

Most important is the property that the isotopic rotation group is locally isomorphic to the conventional group [54]. Thus, the ultimate, axiomatic foundations of the symmetry remain exact in the transition from the perfect sphere to the ellipsoids, and only specific realizations are broken.

In this way, the "breaking of the rotational symmetry" is reduced to the level of mere academic parlance without a true scientific value. In fact, the abstract rotational symmetry cannot be considered broken for the ellipsoids. Only its realization in the structurally most simple possible form is broken, that via the trivial associative product AB. On the contrary, if the same symmetry is realized in the less trivial way, then it is exact, as proved for the isotopic product $A*B = AgB$.

This illustrates the possibilities offered by hadronic mechanics of regenerating exact space–time and internal symmetries that are quantum mechanically broken, that is, that are violated when realized in their simplest possible way.

Apparently, this feature is not restricted to the rotational symmetry, but extends to other space–time and internal symmetries, including the so–called discrete ones (see below).

In fact, the regeneration of the exact character of the symmetry via the Lie–isotopic generalization has been proved for the following additional cases of continuous transformations:

- the Lorentz symmetry [32] ;
- the so—called unitary symmetries, as studied by the Italian physicist R. Mignani [67] ;
- the so—called gauge symmetries, as studied by the other Italian physicist M. Gasperini [68] ; and others.

The case of discrete transformations will be considered in the next paragraph.

These discoveries are not purely formal, because they have a number of implications for experiments.

In fact, an experiment "to test the breaking of the rotational symmetry" can be deprived of true physical contents, unless properly conceived, and the results expressed with care.

This situation is evidently due to the preservation of the abstract axioms of the rotational symmetry in the deformation of the sphere, while the explicit forms of conventional and isotopic rotations are basically different, as they must be.

The situation becomes even more delicate when passing to the special relativity. In fact, the underlying axiomatic structure remains unchanged in the transition from the conventional to the isotopic relativity, as reviewed in Section 1.4. In particular, the abstract structure of the Lorentz symmetry is preserved.

Despite that, we can have massive, ordinary particles moving inside hadronic matter at speeds exceeding that of light in vacuum (Section 1.4).

As a result, we can speak of a "breaking of the special relativity" in the sense that: the explicit form of the conventional Lorentz transformations no longer provide the invariance of physical laws; the speed of light in vacuum is no longer the upper bound for causal signals; etc. Nevertheless, the terms "breaking of the Lorentz symmetry" have no scientific meaning.

Use of hadronic mechanics for the identification of the origin of irreversibility in nature.

The most visible and perhaps most fundamental problematic aspect of quantum mechanics is its incompatibility with the established irreversibility of the macroscopic world. I am referring to the fact that the Newtonian and statistical layers of the physical reality violate the invariance under time inversion (which is an example of discrete transformation), while quantum mechanics is intrinsically reversible, that is, its structure is invariant under time inversion, as well known in the technical literature.

Inspection of our environment establishes the incontrovertible irreversibility of the classical reality. In fact, if the

time–reversal symmetry was exact in our Newtonian environment, a phenomenon such as a bullet breaking through a wall should admit its time–reversed image, the automatic regeneration of the wall and the expulsion of the bullet without firing a shot!

The existence of irreversibility in statistical mechanics is equally established by incontrovertible evidence. In the ultimate analysis, entropy is a manifestation precisely of the irreversible character of the physical world.

On the contrary, currently preferred quantum mechanical treatments are reversible, as well known.

The lack of unity of physical and mathematical thought is then self–evident.

Hadronic mechanics permits new frontiers in this truly fundamental, open problem, by recovering the unity of physical thought via a unique mathematical structure that applies at all levels of treatment, whether in Newtonian, or statistical, or particle mechanics.

The fundamental question is the origin of the irreversibility in classical and statistical mechanics. Once this origin is identified jointly with its abstract mathematical structure, the particle description MUST be adapted accordingly. The other approach, that of attempting compatibility of a reversible particle description with macroscopic irreversibility cannot but be plagued by a host of inconsistencies (Figure 1.6.3).

Compatibility of the reversibility of the center–of–mass descriptions of particle interactions with the irreversibility of the interior dynamics.

At this point, we must clear a basic, rather widespread misrepresentation. It is generally believed that the reversibility of the center–of–mass description of high energy particle collisions implies the reversibility of the particle reaction considered.

Nothing could be more fallacious than that.

The fellow taxpayer can readily understand the point, and see the implications for scientific accountability (see next paragraph), by ignoring complicated papers in high energy physics, and going back to the observation of our Newtonian environment.

Look at our earth. Its interior trajectories, such as that of Skylab during re–entry (Section 1.3), are generally irreversible. Nevertheless, the motion of the center–of–mass of earth within the solar system is fully reversible. This illustrates the physical reality according to which the reversibility of center–of–mass descriptions, by no means, implies the reversibility of interior processes.

SYSTEM	POINT–LIKE PARTICLES	EXTENDED PARTICLES IN CLOSED–CONSERVATIVE TREATMENT	EXTENDED PARTICLES IN OPEN/NONCONSERVATIVE TREATMENT
UNIFYING MATHEMATICAL STRUCTURE	LIE ALGEBRAS [74]	LIE–ISOTOPIC ALGEBRAS [8]	LIE–ADMISSIBLE ALGEBRAS [75]
NEWTONIAN DESCRIPTION	HAMILTONIAN MECHANICS [6]	BIRKHOFFIAN MECHANICS [10]	BIRKHOFFIAN–ADMISSIBLE MECHANICS [12]
STATISTICAL DESCRIPTION	HAMILTONIAN STATISTICS [76]	PRIGOGINE'S STATISTICS [71]	STATISTICS BY FRONTEAU, TELLEZ–ARENAS ET AL. [69,70]
PARTICLE DESCRIPTION	QUANTUM MECHANICS [52]	EXTERIOR BRANCH OF HADRONIC MECHANICS [77]	INTERIOR BRANCH OF HADRONIC MECHANICS [78]

Figure 1.6.3. One aspect of contemporary theoretical physics which is carefully avoided in orthodox presentations, is the lack of unity of physical and mathematical thought, with such inconsistencies and incompatibilities in the transition from one layer to another, to create a clear problem of scientific ethics (see next paragraph). Newtonian and statistical mechanics are intrinsically irreversible, that is, they violate the symmetry under inversion of time, as established by trajectories in our atmosphere, the notion of entropy, and countless other phenomena. The ultimate physical origin of such irreversibility is well established and consists precisely of the contact/nonlocal/non–Hamiltonian forces considered throughout this presentation. This physical reality at the Newtonian and statistical levels is contrasted with quantum mechanics which is intrinsically reversible, as well known. The incompatibilities of quantum mechanics with the preceding descriptions are such to constitute a second litany (besides that for Einstein's gravitation of Section 1.5). To avoid excessive length, I merely recall here the following facts, well known to every physicist: (a) irreversible Newtonian trajectories are generally non–Hamiltonian; (b) reversible quantum mechanical trajectories are Hamiltonian; and, consequently (c) the reduction of classical irreversible trajectories to a large collection of quantum mechanical, reversible trajectories is strictly inconsistent. Period! Thus, the reduction of Skylab to a large collection of reversible, quantum mechanical constituents is intrinsically inconsistent because of the non–Hamiltonian character of the former system versus the strictly Hamiltonian character of the latters. The only classical and Newtonian descriptions truly compatible with quantum mechanics are those depicted in the figure (Hamiltonian mechanics and statistics). But they are generally reversible, to begin with. Besides, they represent only part of the systems and, as such, are not suited for an overall view on the unity of physical thought and underlying mathematical structures. Hadronic mechanics was proposed

in ref. [14] also in the hope of regaining, in due time, the currently missing unity of physical and mathematical thought. In fact, the mechanics is, first of all, differentiated into one branch for the exterior/conservative treatment, and a different, but compatible branch for the complementary open/nonconservative problem. Secondly, each of these branches is constructed in such a way to possess exactly the same mathematical structure of the corresponding statistical and Newtonian layers of description. Only the verification of this rule can avoid fundamental inconsistencies, as occurring in current physical theories. Intriguingly, all the formulations of the second column can be constructed via the use of the transformation theory applied to the corresponding formulations of the first column. For instance, the structure of Birkhoffian mechanics can be reached via non–canonical transformations of Hamiltonian mechanics [10]. Similarly, the structure of Prigogine's statistics [72] and of the exterior branch of hadronic mechanics [79] can be obtained via non–unitary transformation of corresponding statistical and quantum mechanical settings. After all, as stressed throughout this text, the Lie and Lie–isotopic descriptions can be reduced to the same, abstract, realization–free axioms. The true novelty of description from an axiomatic viewpoint is that depicted in the third column. This can be readily seen from a mathematical point by the fact that Lie–admissible formulations cannot be reached via suitable transformations of the Lie–isotopic ones, thus establishing their novel character [79]. As a result, the true, ultimate, physical and mathematical description, from which all the others can be derived, are those for the OPEN conditions. Closed–conservative descriptions constitute an academic abstraction because no system can be truly considered as isolated in the universe. In regard to irreversibility, the emphasis on open/nonconservative conditions becomes essential not only for the theoretical description, but also for the conception and realization of experiments (Section 1.8). When additional branches of sciences are included in this overall view, the findings above are strengthened, rather than weakened. For instance, a theory of gravitation for the interior problem, to be meaningful, must represent the trajectory of Skylab (at least qualitatively!). This means that it "must" be locally Galilean–admissible [12], owing to the direct universality of the Lie–admissible formulations for Newtonian systems (as a consequence of which, other results are necessarily equivalent to the Lie–admissible treatment). If we include theoretical biology, the situation is more reinforced. It is well known in the specialized literature that neural systems are strictly non–Hamiltonian, thus in line with the second and third column of the diagram, but not with the first. We can therefore conclude by saying that **the entirety of science has now an established non–Hamiltonian structure, including Newtonian mechanics, statistical mechanics, interior gravitation, theoretical biology, etc., not to mention mechanical engineering. The only and last branch of science that still remains stubbornly anchored to Hamiltonian descriptions (or equivalent lagrangian ones) is particle physics (inclusive of nuclear physics), despite a litany of manifest inconsistencies, let alone an evident lack of unity of physical and mathematical thought.** This situation is a central motivation for writing this book. In fact, the thesis submitted to the U.S. taxpayer for his/her own independent judgment is that this stubborn misoneism is a manifestation of the scientific obscurantism imposed for decades in the U.S. physics by vested, academic–financial–ethnic interests surrounding Einstein's ideas. To abandon the Hamiltonian–Lie descriptions in favor of broader physical–mathematical theories implies a necessary abandonment of Einstein's relativities in favor of suit-

able generalizations, with a manifest damage to said interests. The most visible and rumorous illustration of this situation is provided in Section 2.4 It regards an incredible stubborness of the Journals of the American Physical Society to publish a paper on the views presented in this paragraph (which was then readily published in Europe, ref. [59]. Every possible effort on my part, including the written request of resignation of two editors, the filing of documented reports to high governmental officers, etc. proved to be totally fruitless. After over one year of useless fights, I wrote to the editor in chief of the A.P.S. that I had been forced "to cross the Rubicon". This book IS my Rubicon.

To put it differently, we have a situation similar, and actually complementary to that for relativities. The validity of Galilei's relativity for the center—of—mass of earth, by no means, is evidence of the validity of the same relativity for the interior trajectories. At a deeper study, it emerges that the departures from Galilei's relativity in the interior problem constitute precisely the physical origin of the irreversibility of Newtonian mechanics. It could not be otherwise for a truly considerable number of technical reasons (such as the fact that Galilei's relativity is characterized by canonical transformations, while irreversible trajectories are generally non—canonical).

In the transition to particle physics, the situation is expected to be the same on conceptual grounds. This is the reason for the insistence that hadronic mechanics provides a nuclear (and hadronic) structure model as an operator version of our earth.

We know now that the validity of the Galilean (or the special) relativity for the center—of—mass motion of, say, a nucleus, by no means, is evidence of the validity of the same relativity for the interior dynamics. We therefore construct a structure model of the nucleus in such a way to admit interior irreversible processes, while possessing a time—reversible center—of—mass motion. This is precisely the hadronic model proposed in ref. [14] (see Figure 1.6.2).

The experimental implications are intriguing, inasmuch as they imply the lack of conclusive character of all experiments on irreversibility conducted until now for the closed—conservative approach, that is, in the center—of—mass system, as we shall see better in the next section.

Hadronic mechanics and its underlying mathematical structures can therefore provide the identification of the ultimate origin of irreversibility of the universe which, according to Tellez—Arenas [70] and others, is given precisely by the contact/non—local/non—Hamiltonian interactions, whether for Newtonian systems moving within a resistive medium, or for the collision of molecules, or for the mutual penetration of the wave—packets of hadrons.

To summarize, the center—of—mass trajectories of nuclear (as well as particle) reactions is expected to be time—reversal in-

variant in the conventional quantum mechanical sense.

The hadronic–isotopic description of the same reactions, with internal non–Hamiltonian effects, is also expected to be time–reversal invariant, of course, in the associative–isotopic sense indicated earlier.

The ultimate manifestation of irreversibility is therefore seen in OPEN/NONCONSERVATIVE nuclear (and particle) reactions. But then, I do not need experiments for that. In fact, all these dynamical evolutions are non–unitary and, as such, intrinsically irreversible [59]. Their extension into a closed form inclusive of the external systems cannot but preserve the internal irreversibility, thus reaching the nuclear structure provided by hadronic mechanics.

We essentially have a situation similar to the closing of Skylab into an isolated system, inclusive of earth atmosphere (Section 1.3). Such closure simply cannot change the intrinsic irreversible character of Skylab.

The same situation is expected to occur in nuclear (and particle) physics. No more, no less. Experiments can only provide the quantitative resolution of the internal irreversibility.

But, again, the existence of an internal irreversibility in systems under strong interactions should remain out of the question.

The deprecable condition of scientific ethics in irreversibility.

As everybody can see, the ideas on irreversibility summarized in the preceding paragraph are so simple, to be understandable by everybody.

The same ideas, however, encounter extremes of opposition by leading physicists in leading U.S. institutions, as we shall see. In fact, the central episodes of Section 2.4 are related to questionable editorial actions aimed at preventing the appearance of the ideas in the journals of the American Physical Society. I am referring not only to theoretical studies (Section 2.4), but also to experimental studies by international teams of experimentalists (Section 1.7). As as shall see, the publication of the same papers in European Journals was routinely done without difficulties, We are therefore facing, specifically, a problem in the U. S. Physics.

It is time to point out openly and plainly the most plausible reasons for these obstructions in due scientific processes. The final judgment, of course, belongs to the fellow taxpayer.

Stated in a nutshell, the time–reversal symmetry is one of the foundations of Einstein's special relativity. In fact, the fundamental invariant of the special relativity, the Minkowski form $X'mX$, $X = (R,ct)$, $m = \text{diag}(+1,+1,+1,-1)$, considered in Sec-

tion 1.4, is left invariant by the change of the direction of time, that is, by the replacement of t with –t. Evidently, the time–reflection symmetry affects the structure of a rather fundamental part of the relativity, the time evolution. As evident from the preceding sections, the representation of irreversibility in Newtonian and statistical mechanics has requested the generalization of the time evolution. The need for the construction of suitable generalizations of Einstein's special relativity is then a mere consequence.

To put it different, a further incontrovertible invalidation of Einstein's special and general relativities is given precisely by the irreversibility of the physical world.

The most plausible reasons for the current difficulties in establishing a corresponding irreversibility in particle physics is now evident. Such irreversibility would establish the invalidation of Einstein's special relativity with consequential, manifest damage to vested, academic–financial–ethnic interests. It is always the same, ultimate, root of the ethical problem in U.S. physics.

Again, there are means for the fellow taxpayer to separate corrupt academic manipulations, from physical truths, without the need of a Ph.D. in physics.

For this, the fellow taxpayer is asked to contact any nuclear physicist, or to consult any (well written) textbook in the field, and identify the equations for dissipative nuclear processes or for all particle processes involving the loss of energy (such as for beams of protons or neutrons interacting on an external, fixed target).

All these processes are represented by non–unitary time evolutions, as well known. In turn, all these time evolutions are intrinsically irreversible, and strictly in conflict with Einstein's special relativity (which demands unitary laws, as a necessary condition to admit a Lie structure).

The reformulation of non–unitary time evolutions via the Lie–admissible/hadronic form is useful for the reasons indicated earlier, including: (a) the regaining of a consistent algebraic structure; (b) the regaining of the capability to achieve numerical predictions for all quantities essentially dependent on the consistency of the underlying algebra; and, last but not least, (c) the possibility of initiating the generalization of Einstein's special relativity for open, irreversible particle reactions (via the generalization of the currently used, one side, modular–unitary realization of the Poincaré group into the most general covering known at this time, that provided by the Lie–admissible bimodules; see the mathematical section later on).

The point which is relevant here, is that the irreversibility IS NOT a consequence of the Lie–admissible re–formulation of non–unitary time evolutions. In fact, the irreversibility is intrinsic in the original formulation. The Lie–admissible re–formulation merely maximizes the visibility of the violation of the

time—reflection symmetry (precisely via the differentiation of the right and left modular action).

The violation of Einstein's special relativity is therefore already there, printed in the books and articles. The violation itself IS NOT quoted because of apparent political reasons. But the authors of those books and articles know well that, whenever the unitarity of the time evolution is gone, the special relativity is also gone.

When such open/nonconservative conditions are closed into a conservative—isolated form, the internal irreversibility persists with the inevitable breaking of the special relativity. In fact, the change of observational frame simply cannot alter the physical reality.

The following incidental note may be instructive. Another discrete symmetry, which is also part of Einstein's special relativity, is the space inversion, that is, the change of the space coordinates R into the form −R. This discrete transformation also leaves invariant the basic Minkowski separation of Section 1.4, X'mX.

The possibility of violating the space—reflection symmetry in particle physics (called parity) was conjectured in the U.S.A. by T. D. Lee and C. N. Yang a number of decades ago, and subsequently confirmed experimentally in certain (weak) interactions (see book [80]).

The incidental note I would like to bring to the attention of the fellow taxpayer is that, after some initial opposition, the violation of parity was indeed accepted by leading physical circles in the U.S. On a comparative basis, the violation of time—reflection symmetry continues to be opposed, decade after decade.

The most plausible reasons for this rather awkward occurrence (recall that the irreversibility cannot be denied for disdipative nuclear and particle treatments!) is, again, the vexing ethical problem of vested interests on Einstein's ideas.

The violation of parity does not directly affect the structure of the special relativity. As a result, models treating parity violation in weak interactions have been constructed in such a way to verify (at least the authors believe*) Einstein's special relativity. The same thing simply cannot be done for irreversibility. The violation of Einstein's special relativity in this case

* I believe that parity violation alone implies the invalidation of the entire special relativity. Apparently, the same view is shared by a number of other independent physicists. The reasons are due to the fact that parity—violation has been merely "described" until now, via semi—empirical, quasi-pheomenological models. If the "dynamical origin" of the breaking is instead considered, the invalidation of the entire special relativity then becomes unavoidable. In fact, such dynamical origin seems to be precisely the internal, contact/non—local/non—Hamiltonian effects due to mutual wave—overlappings.

is too apparent to be disguised via artificial manipulations.

Silence, suppression of evidence, and other questionable practices, then appear to be preferred in academia.

In this case too, the entanglement of the situation at the governmental—academic complex is such that no self—corrective procedure appears possible. Again, editors (governmental officers) will keep sending out papers (grant applications) to leading physicists in the field at leading U.S. institutions for the so—called "peer review". In turn these "peers" will continue to reject papers (grants) supporting the irreversibility in nuclear and particle physics. The scientific obscurantism in the sector is therefore expected to continue indefinitely.

The only hope is for the taxpayer to intervene and organize suitable actions aimed at preventing the dispersal of public funds in academic, corporate and military research on reversible models which ignore the critical literature in the field.

Expected contributions of hadronic mechanics to hadron physics.

The contributions of hadronic mechanics in hadron physics are expected to be more fundamental than those in nuclear physics. This is due to the fact indicated earlier that the approximate validity of quantum mechanics in nuclear physics is out of question, thus relegating the role of hadronic mechanics to possible refinements and deeper understandings of results achieved via the use of quantum mechanics.

In the transition to hadron physics, we cannot exclude the possibility of finite departures from quantum mechanics due to the much greater conditions of mutual penetration of the wave—packets of the constituents, when compared to the nuclear conditions. As a consequence, we expect the possibility of achieving resolutions that have been prohibited until now by quantum mechanics.

Recall that the primary and, by far, most fundamental achievement of quantum mechanics in nuclear physics was the identification of nuclear constituents with physical particles (protons and neutrons).

Despite massive efforts, the application of quantum mechanics to hadron physics has not provided until now the final identification of the hadronic constituents with physical particles, that is, particles identified via direct experiments.

As well known, hadrons are today thought to be composed of some sixteen different particles called quarks, and their sixteen different antiparticles (with the possibility of additional quarks and antiquarks in sight).

This hypothesis, even though of proved physical relevance, has not resolved the identification of hadronic constituents with

physical particles for numerous reasons, such as:

(a) Quarks are not produced free in the spontaneous decays of unstable hadrons; they are also not produced in hadronic collisions up to the highest possible energies attained in particles accelerators; and they have not been detected via any additional experiment until now, despite a rather large search.*

(b) Since quarks are not produced free in the spontaneous decays, they are thought to be "confined" in the interior of hadrons. Despite additional, also massive efforts, a theoretical model of confinement of quarks has not yet been achieved to this writing. In particular, a strict form of confinement of quarks, that with an identically null probability of tunnel effects of free quarks, is impossible whenever quantum mechanics is assumed as exactly valid in the interior of hadrons. This is due to the fact that, according to quantum mechanics, the probability of tunnel effects of free constituents of a bound state cannot be rendered identically null, irrespective of the potential barrier used.

(c) Quarks are today no longer considered as being elementary. A central open problem of current quark theories is precisely that of identifying the constituents of quarks with more elementary particles.

A primary objective of hadronic mechanics is to achieve, in due time, the identification of hadronic constituents with physical particles. Furthermore, these physical constituents should be such to be consistently identifiable as the quark constituents. Finally, the constituents should be such to permit the achievement of a strict confinement of quarks in the interior of hadrons, with an identically null probability of tunnel effects.

*Note that, the conceivable experimental detection of only one quark would leave the problem of hadronic constituents still fundamentally unresolved, because of the need to identify experimentally each of the conjectured sixteen different quarks and each of the sixteen different antiquarks. It is appropriate to recall here the known historical case when the experimental detection of the neutron was not considered evidence for the existence of the antineutron, which had to be detected independently. The need to follow exactly the same scientific rules for each quark and for each antiquark is then evident. Experimentalists have reported intriguing indications of measurement of fractional charges (which are one of the penculiarities of quarks). However, these measures alone, even if confirmed, by no means constitute evidence of the experimental detection of quarks, because of the need to measure jointly all the rather numerous additional characteristics of quarks (mass, spin, parity, magnetic moments, and others).

The three historical rules emerged from the resolution of the structure of atoms and nuclei.

The resolution of the problem of the structure of atoms identified three fundamental rules.

RULE 1: The atomic phenomenology demands different, yet compatible models: a first model for the classification of atoms into families (the famous Mendeleyev table); and a different, yet compatible model for the structure of each individual atom of a given family.

RULE 2: The atomic constituents can be produced free either spontaneously, or via suitable bombardment of the atomic structure.

RULE 3: The number of atomic constituents increases with mass.

In the transition from the atomic to the nuclear structure, history repeated itself. The three fundamental rules resulted to be fully verified, except some technical modifications.

In fact, the model of so–called unitary classifications of nuclei cannot produce a meaningful nuclear structure, which is instead interpreted via different models. Similarly, the nuclear constituents can indeed be produced free either spontaneously, or via suitable bombardments. Finally, the number of nuclear constituents also increases with mass, exactly as it is the case at the atomic level.

For additional remarks along these lines, the interested reader may consult the introductory parts of ref.s [14, 11, 49].

Use of the hadronic mechanics for the construction of a structure model of hadrons along the three historical rules of atoms and nuclei.

Hadronic mechanics was proposed for the purpose of attempting a structure model of hadrons exactly along the historical Rules 1, 2 and 3 emerged from the nuclear and atomic structures.

For this reason, the available models of unitary classification of hadrons into families were assumed as being of terminal charactr [14,11,49]. The desired structure model was then restricted to achieve compatibility with such classification, exactly along the dychotomy classification/structure of the atomic and nuclear phenomenology.

Second, the constituents of hadrons were assumed to be suitably selected, massive particles produced free in the spontaneous decays. In turn, each particle was subjected to the same re-

duction, until reaching electrons and positrons as the ultimate constituents. As now familiar, it was at this point that the construction of a generalization of quantum mechanics resulted to be necessary. In fact, we have a clear cut situation: either quantum mechanics is strictly valid in the interior of hadrons, in which case hadrons "cannot" be composed of massive particles produced in the spontaneous decays; or a suitable generalization of quantum mechanics holds in the interior of hadrons, in which case the consistency of the proposed structure model is reduced to the construction of an adequate covering mechanics.

Thirdly, and perhaps most importantly, the model was restricted to verify the rule of increase of number of constituents with mass.

The notion of hadronic constituents (called "eletons" and "antieletons") as characterized by hadronic mechanics.

A primary hypothesis for the development of hadronic mechanics was the identification of the constituents of hadrons with the ordinary electrons and positrons (see ref. [14], Section 5).

While electrons are at the large mutual distances of the atomic structure, the same electrons, to be hadronic constituents, must be in a state of complete mutual penetration and overlapping of their wave–packets, each one moving within the medium constituents by the wave–packets of all the other constituents. In fact, the size of the electron's wave–packets is exactly of the order of magnitude as that of all hadrons (one Fermi). This results in motion within the hadronic medium, with consequential need to achieve a generalization of quantum mechanics capable of incorporating, not only the potential interactions of the atomic structure, but also the contact/non–potential/non–local interactions due to motion within hadronic matter. This second aspect was also fully identified in the original proposal [14]. In particular, the Lie–isotopic generalization of Heisenberg's equations was proposed for the exterior treatment of electrons and positrons in conditions of total mutual penetration, while the broader Lie–admissible generalization was suggested for the treatment of each electron while moving within the sea of all other constituents.

These broader dynamical conditions generally imply an alteration of the intrinsic physical characteristics of electrons and positrons (as well as of all other particles under similar physical conditions). In fact, rest mass, intrinsic angular momentum, parity, charge and magnetic moments of one electron while totally immerged within hadronic matter are not expected to be necessarily identical to the corresponding values when the same electron moves in empty space under long range electromagnetic interactions. All available experimental information on the in-

trinsic characteristics of the electrons is restricted to the latter conditions, while we have absolutely no direct experimental information on the measurement of the same characteristics when the electron is inside hadronic matter. At any rate, the reader can easily visualize the distorsion of the wave–packet of the electrons and positrons in the transition from motion in vacuum to motion within hadronic matter. The alteration of the physical characteristics due to this distorsion is then a mere technical consequence.

This additional aspect was also identified in the original proposal [14]. Electrons were called "eletons" when inside hadrons as one way to stress the deviations from their physical characteristics when in empty space. Today we know that the notion of eleton is one of the most technically involved objects of theoretical physics (a right and left, bi–representation of a Lie–admissible generalization of the Lorentz algebra acting on a bi–modular isohilbert space).

In particular, a progressive chain of "mutations" of the intrinsic characteristics were suggested as possible in ref. [14], beginning with minimal mutations (say of the magnetic moment only) for miminal conditions of wave–overlapping, and then passing to the mutation of additional characteristics for deeper departures from the atomic conditions.

Preliminary bound states of eletons and antieletons obeying the covering hadronic mechanics were also worked out in ref. [14] in a rudimentary local approximation, thus establishing the plausibility of the theory for light mesons and for the neutron (see below).

The reconsideration of these structure models of hadrons via the advances on hadronic mechanics made since 1978, had to be interrupted for the reasons indicated earlier.

The studies on the identification of electrons and positrons as the quark constituents as well as on the achievement of a strict confinement of quarks had also to be interrupted for the same reasons. The resumption of the research is not foreseeable at this time.

Identification of the constituents of the neutral pion with one electron and one positron obeying hadronic mechanics.

Consider the problem of the structure of the lightest known hadron, the neutral pion. If quantum mechanics and conventional relativities are assumed as strictly valid in its interior, a structure model of the neutral pion as a bound state of one electron and one positron is not possible for the following reasons.

Consistent, quantum mechanical, bound states of two particles (such as the hydrogen atom or the deuterium) have a

total energy that is smaller than the sum of the energies of the constituents, including rest energy and kinetic energy. The loss of energy is the so–called binding energy. This property is well known.

An aspect that is not well known, even in the technical literature, is that when the sum of the rest energies of the constituents is much smaller than the desired total energy of the bound state, quantum mechanical equations become generally inconsistent in the sense of admitting only complex values of total energies.

This is essentially the case for the neutral pion as a bound state of one electron and one positron. In fact, the total energy of the neutral pion is 135 bigger than the sum of the rest energies of the assumed constituents. Under these conditions, quantum mechanical, physically meaningful bound states are unknown. For a study of the problem, the interested reader may consult Appendix A of ref. [40] and references quoted therein.

If contact interactions are admitted in the interior of the neutral pion because of the conditions of mutual penetration of the wave–packets of the constituents, the bound state of one electron and one positron is capable of representing all known characteristics of the neutral pion, such as: mass, mean life, spin, space and charge parity, electric and magnetic moments, etc. See in this respect Section 5.1 of ref. [14]. A pictorial view is presented in Figure 1.6.4.

The historical hypothesis on the structure of the neutron as a bound state of one proton and one electron.

The first hypothesis on the structure of the neutron was that it is a bound state of one proton and one electron. The hypothesis was based on the experimental observation that the neutron, when isolated, is unstable and decays precisely into one proton and one electron plus a massless neutrino. It was then rather natural to assume that the massive constituents of the neutron are the stable particles produced in its spontaneous decay.

The hypothesis had to be subsequently abandoned because of a number of technical difficulties in recovering all the characteristics of the neutron, such as:

1) The model is unable to recover jointly the rest energy and the mean life of the neutron. In fact, to recover the rest energy, the peripheral electron becomes so energetic that the mean life of the system is much too shorter than that of the neutron (about 15 minutes). Vice versa, if the neutron mean life is recovered, there is no sufficient internal energy to reach the neutron rest mass;

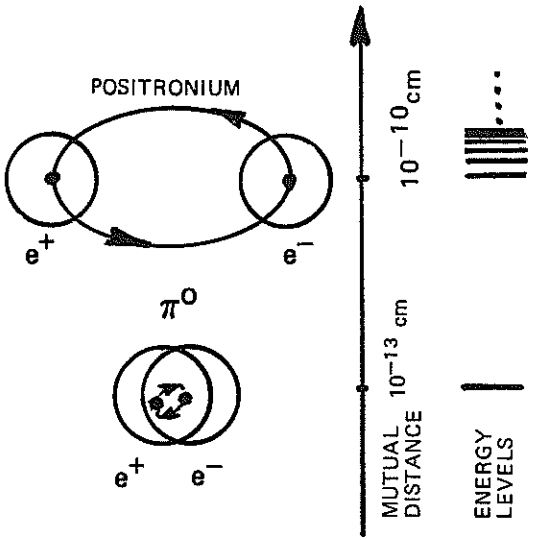


Figure 1.6.4. A schematic view of the hypothesis submitted in ref. [14], see pages 827 and following, according to which the lightest known hadron, the neutral pion, is a bound state of one electron and one positron under conditions of mutual overlapping down to the dimension of 1 Fermi. The admission of contact/nonpotential/nonlocal forces, and the use of hadronic mechanics permit the recovering of all known characteristics of the pion, such as, mass, spin, mean life, radius, electric and magnetic moments, space and charge parity, etc. [14]. Intriguingly, according to the hypothesis, the neutral pion results to be a positronium compressed down to the dimension of 1 Fermi. Recall that, when at sufficiently large mutual distances, one electron and one positron can be bound together to form the lightest known atom, the positronium, which possesses the typical, infinite, discrete spectrum of the atomic structure. Hadronic mechanics predicts the existence of an additional bound state of one electron and one positron,this time when the particles are in conditions of deep mutual overlapping. Apparently, only one such bound state is stable, resulting in the single, unique bound state that is typical of two–body nuclear states (such as the deuterium which, as recalled in the test, has no exited states). Recall that, in quantum mechanics, particles with spins can be bound together in two different ways, in the so–called singlet state (with spins antiparallel) and the triplet states (with spins parallel). It was stressed in ref. [14] that the latter bound states are highly unstable when the particles are bound one within the other, owing to the need of wave–packets rotating one against the other. For the same reason, the state of singlet is the only one expected to be stable, trivially, because the rotation of wave–packets would now be in phase, much along the coupling of gears. The unstable character of triplets states was considered per se sufficient to warrant the construction of a suitable generalization of quantum mechanics. The physical foundation of the model is the fact that the neutral pion decays spontaneously into one electron, one positron and a (massless) photon.

2) The model does not recover the total spin of the neutron. This is due to the fact that the proton, the neutron and the electron, all have the same spin ½. Now, according to quantum mechanics, two spin ½ particles can only produce a bound state with integer spin, but not the needed value ½ for the neutron.

3) The model does not reproduce the correct values of electric and magnetic moments of the neutron, as well as other difficulties of lesser relevance.

Hadronic mechanics apparently permits the resolution of all these difficulties. The understanding is that the studies are at the beginning and so much remains to be done prior to claiming any final conclusion, whether in favor or against the model.

The first difficulty is readily solved by contact/nonpotential/nonlocal forces via a mechanism similar to that of the hadronic structure model of the neutral pion.

The remaining difficulties are apparently resolved by the hypothesis that electrons experience an alteration of their intrinsic characteristics in the transition from motion in vacuum, to motion within hadronic matter, thus becoming "eletons".

The alterations were called "Lie–admissible mutations" or "mutations" for short, to indicate the transition from the mathematical theory applicable under electromagnetic interactions, Lie's theory, to the covering theory suggested for strong interactions, the Lie–admissible theory. The understanding is that, when eletons exit hadronic matter and return to motion in vacuum, they reacquire their known quantum mechanical characteristics.

The mutation of spin of the electron into that of the eleton can be readily visualized. Recall that the proton is about 1840 times heavier than the electron. It can therefore be considered as being at rest in first approximation. This means that the electron must penetrate inside a virtually stationary proton by therefore being forced to follow its intrinsic rotation.

These physical conditions have a number of consequences. First, they imply the lack of existence of the triplet state (with parallel spins) as a stable bound state (Figure 1.6.4). In fact, it would imply wave–packets continuously rotating one against the other. The only stable state is that with spins antiparallel called singlet, much along the coupling of gears. In fact, the model was called of "gear type".

Secondly, since the electron is forced to rotate "in phase" with the intrinsic rotation of the proton, the spin of the electron is forced to assume a value compatible with these physical conditions. In particular, the mutated value of the spin can apparently assume the value zero which, as such, permits to recover

the value ½ of the spin of the neutron, as desired.

The massless neutrinos,* which also have spin ½, according to the hadronic model under consideration, are the particles produced by the electron when existing the proton and returning to the conventional dynamical conditions known until now, including its value ½ of spin.

It was also indicated in ref. [39] that the mutation of the spin of the electron, from the value ½ to the value zero, may be in the final analysis a mere illusory effect in the following sense. Consider an observer ideally located at the center of the proton. Then, for that observer, the peripheral electron may appear as having null spin owing to the phase conditions of rotations needed for stability (see Fig. 8, p. 1971), of ref. [39]). For an outside observer, the same electron has both an intrinsic angular momentum and an orbital one.

Thirdly, an alteration of the intrinsic angular momentum of the electron implies that of electric and magnetic moments. In turn, these latter mutations are used to resolve problematic aspects 3).

The ideas outlined above are essentially those known in 1979, ref. [39], p. 1968. Since that time, the studies of hadronic mechanics have made considerable progress. The model can be studied today via quite sophisticated means (see Figure 1.6.5).

*According to the model of ref.s [14, 39], the massless neutrino is not a constituent of the neutron, nor of any hadron. This position was assumed because of the extremely low capability for neutrinos to interact with matter. In fact, highly intense beams of neutrinos from the sun and outer space cross the entire earth continuously, without being scattered (our entire earth is said to be "transparent" to neutrinos). This situation suggested the assumption that only electrons and positrons are the ultimate, elementary constituents of hadrons (with the proton being a separate problem —see below). I must quote, at this point, intriguing studies by the U.S. physicist A. O. Barut [81], according to which the neutron is a bound state of one proton, one electron and one neutrino. Apparently, Barut has reached a mechanism for binding the otherwise elusive neutrinos within hadronic matter. Barut's efforts are more generally oriented toward the possible identification of quarks with physical, already known particles. As such, the studies are commendable, in my view. I regret to report, however, the considerable lack of interests in these studies by "leading quarkologists" in the U.S.A., for a number of technical reasons, besides the problem of binding neutrinos inside hadrons (such as the fact that the charge and other quantum numbers of quarks cannot be identified with those of protons, electrons and neutrinos). The connections between Barut's hypothesis [81] and that I submitted in ref. [39] are quite intriguing. In essence, Barut's model can be formulated via a fully conventional, quantum mechanical theory. In fact, the additional presence of the neutrinos avoids the crucial problematic aspect 2) regarding the recovering of the spin of the neutron. Nevertheless, I believe that Barut's model can be subjected to an isotopic lifting within the framework of hadronic mechanics, thus achieving compatibility with ref. [39].

For completeness, it should be indicated that the mutation of the spin of the electron into that of the eleton is not expected to be the only possibility to reach the neutron spin. In fact, recent studies by the Indian physicists P. Bandyopadhyay and S. Roy [82] have indicated the possibility that the "angular" momentum may assume half–odd–integer values when particles are moving in a hadronic medium. This possibility is strictly precluded for motion in empty space, as stressed in all textbooks of quantum mechanics. It is evident that, if angular momentum can assume the value ½ for one electron bound within a proton, that electron can preserve the value ½ of spin to achieve the spin ½ of the neutron.

Electrons, however, would still need an eletonic form owing to the need to exhibit mutations of the intrinsic magnetic moments in order to represent the total electric and magnetic moments of the neutron (see Figure 1.6.5 for additional comments).

The structure model of the remaining hadrons.

As recalled earlier, the lightest known, strongly interacting particle is the neutral pion. The immediately next particles in the value of the rest mass are the positively or negatively charged pions. By keeping in mind the historical rule of the increase of the number of constituents with mass, the charged pions were assumed as being bound states of three eletons and antieletons (two mutated electrons and one mutated positron or two mutated positrons and one mutated electron, depending on total value of the charge). Thus, in the transition from the neutral to the charged pions, one additional constituent was assumed within the context of hadronic mechanics (by comparison, the number of constituents remains the same within the context of quark models, not only for all pions, but also for all light mesons).

The next particles in the scale of mass, neutral and charged kaons, were assumed as bound states of mutated pions and mutated eletrons. Subsequent particles were then conceived as having a similar model. A special case is that of the proton which, owing to its stability, may well constitute the most complex structural problem of contemporary physics. After all, bound states of particles and antiparticles, whether conventional or mutated, are expected to exhibit the typical instability of the particle world.

The above model, (submitted in ref. [14], Section 5) has remained mostly unexplored until now, except isolated instances, such as the studies by Jiang, Chun–Xuan [83], a physicist from the People's Republic of China, and Z. J. Allan [84], a chemical engineer from Switzerland.

No conclusion can therefore be reached at this time,

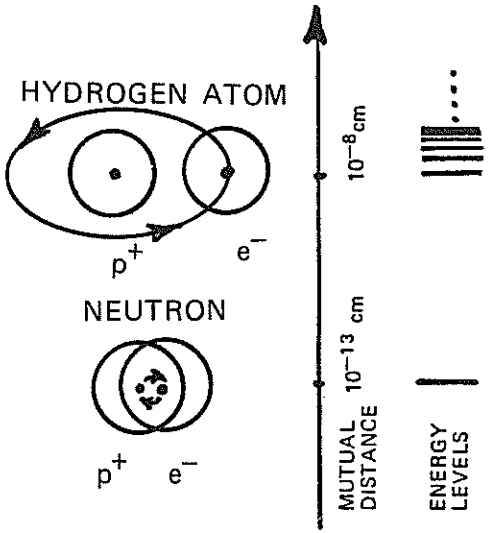


Figure 1.6.5. A schematic view of the hypothesis submitted in ref. [14] and subsequently elaborated in more detail in ref. [39], pages 1968–1974. As well known, when at sufficiently large mutual distances, one proton and one electron can bound together to form the hydrogen atom, with the familiar, infinite, discrete, spectrum of exited states. Hadronic mechanics predicts that the ordinary neutron is an additional bound state of one proton and one electron, this time bound together one inside the wave–packet of the other, in full analogy to the case of the positronium–neutral pion of Figure 1.6.4. Intriguingly, we have again one single, unique, bound state for two–body nuclear phenomenology. In turn, the absence of exited states appears to be crucial for the resolution of the problem of hadronic constituents of the remaining hadrons. The equations of structure of the model of the neutron considered here are similar to those of the neutral pion, as far as energy considerations are concerned. Nevertheless, additional technical difficulties emerge, particularly due to spin, electric and magnetic moments, and other aspects. The resolution of these difficulties is apparently permitted by the notion of eletons [Figure 1.6.4], that is, by the alteration of the intrinsic characteristics of ordinary electrons and positrons in the transition from motion in empty space, as in the atomic structure, to motion within hadronic matter, as necessary for the hadronic structure. In turn, this alteration is relevant for numerous other aspects, such as the identification of quark constituents, the achievement of their strict form of confinement, etc. These were essentially the main lines known in 1979. The model can now be re–inspected via the more recent advances due to the Lorentz–isotopic relativity [32] and the mathematical structure of hadronic mechanics [55]. The isotopic structure of the mechanics can be made to coincide with that of the Lorentz–isotopic relativity. This essentially implies the identification of the fixed operator g of the associative–isotopic product of operators, $A*B = AgB$, with the generalized metric

G of the Lorentz–isotopic relativity (see Section 1.4). The invariance of the model under the Lorentz–isotopic transformations is then ensured by construction. The selection of the generalized metric G for the interior of the neutron then constitutes the first degree of freedom of the hadronic description. An additional degree of freedom is given by the tensorial product of the (iso–)representation of the Lorentz–isotopic group identified in ref. [55]. The achievement of a total spin ½ is then consequential. Conventional total spins are computed via products of conventional representations of the rotation (or the Lorentz) group. In the transition to hadronic mechanics, the space–time symmetry groups are subjected to a first generalization; the representations of these groups are also of generalized character; and their tensorial products exhibit a third degree of freedom. The combined use of all these novel degrees of freedom permits the achievement of a total hadronic spin ½ from the bound state of two particles of original, quantum mechanical, spin ½. Note that the isotopic theory of rotations may well permit half–odd–integer angular momenta, exactly along the lines suggested by Bandyopadhyay and Roy [82]. To put it differently, at the covering isotopic level, the alternative of mutating the spin of the electron down to zero, or that of assuming angular momentum ½, may well turn out to be equivalent. Regrettably, the studies on the re–examination of the historical model of structure of the neutron had to be interrupted, among others, for the writing of this book, without any prediction of their possible resumption. Existing governmental support was truncated, while all applications submitted and re–submitted to governmental agencies for the development of hadronic mechanics and its applications over a three year period were rejected, including those for possible military applications (Section 2.5). This implied the impossibility of hiring physicists with the necessary expertise in nuclear physics.

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whether in favor or against the model.

The conceivable military applications of the hadronic generalization of Einstein's ideas.

I do not know whether or not the neutron is truly a bound state of one proton and one electron. The only thing I am sure of is the necessity of resolving the issue either in favor or against the historical hypothesis. Besides evident scientific motivations, there are non–trivial military aspects which cannot be treated too lightly.

The military establishment in the U.S.A. believes that only a few nuclei are fissionable and therefore usable for weapons. If the neutron is a bound state of one proton and one electron, virtually all nuclei could be artificially "disintegrated" therefore resulting in a new generation of weapons.

Evidently, I cannot disclose technical details here. Nevertheless, there are aspects that the fellow taxpayer has the right to know. The first, is the existence itself of conceivable military applications of the studies reported in this section. I am referring to "disintegration" of matter that, to my best knowledge, would originate in the interior of nuclei, would be activated at a dis-

tance, and would not require mass thresholds.

The second point the taxpayer has the right to know is that this "disintegration" of matter is prohibited if Einstein's special relativity is exactly valid in the interior of nuclei, hadrons and (locally) of stars. In fact, the "disintegration" becomes conceivable only when suitable generalizations of the special relativity are assumed as valid for strong interactions (such as the generalization worked out by the U.S.S.R. physicist, Bogoslovsky [29], or the more general one of Lie–isotopic type I recently proposed [32]; see Section 1.4 for details). We could therefore face a typical case whereby vested academic–financial–ethnic interests on Einstein's ideas constitute a potential threat to the security of the U.S.A.

A further aspect the fellow taxpayer has the right to know is that the "disintegration" here considered is not permitted by the current military research known to the general public under the name of "star wars". In fact, these weapons are essentially based on lasers and other beams which lack the physical characteristics needed to initiate a disintegration process in the interior of nuclei. Nevertheless, owing to its potential capability of being activated at a distance, the "disintegration" of matter here considered is fully aligned with the "star wars" objectives.

Evidently, such "disintegration" could have non–military, economic-scientific applications in a number of fields such as energy or crystallography or neural surgery. The elaboration of these aspects is avoided here owing to the need of the prior disclosure of technical details.

At the risk of being pedantic, I must stress that I am merely referring to theoretically conceivable military applications. Whether or not these applications are indeed possible and technically feasable, it is unknown at this time.

I have been aware of these military possibilities since I suggested the construction of the hadronic generalization of quantum mechanics back in 1978 [14] while at Harvard. Nevertheless, since I detest weapons, I kept them for myself. A chain of events forced the changing of my stand on the matter.

My doubts began in 1979 when the resumption of the studies on the historical hypothesis on the structure of the neutron was discussed at a meeting at Harvard (see Section 1.9), and subsequently appeared in the Proceedings of the meeting (see later in ref. [124]). Even though military aspects were carefully avoided at the meeting, I realized that the same military ideas could well be conceived by other physicists throughout the world with manifest detriment to the U.S.A. In the subsequent years, the increase of the international efforts to construct the hadronic mechanics re–confirmed my doubts. Yet, I still kept silent on military profiles.

It was only in 1983 that specific circumstances finally urged the changing of my stand. I had eyewitnessed the rejection

of a considerable number of research grant applications submitted by our Institute to the U.S. National Science Foundations and the Department of Energy on non—classified profiles of the hadronic mechanics. It was therefore clear to me that, on one side, governmental agencies would continue to reject all grant applications filed by our Institute, while, on the other side, we would be forced to transfer abroad the physical research.

This is exactly what happened. In fact, all research activities in the physical profiles of the hadronic mechanics are today conducted solely OUTSIDE the U.S.A. This refers not only to research by individual physicists, but also to all Conferences, Workshops, and research sessions planned by our group for the foreseeable future. They have been all moved abroad (see Section 1.9). This situation was readily predictable in 1983. In fact, N.S.F. and D.O.E. rejected not only all our research grant applications, but also all our applications for support of Conferences and Workshops. Our group therefore had no other choice than move the meetings to more receptive countries.

In view of this scenario, and the evident potential damage to America, I felt compelled to make one last try: submit research grant applications to U.S. military agencies with a disclosure of the conceivable new military applications. My hope was that these military profiles would break the apparent deadlock against the funding of our research programs, and permit their continuation also in the primary, basic research sector.

On March 25, 1983, an I.B.R. application entitled "Studies on hadronic mechanics" was formally submitted to Carl Romney, Deputy Director of the Defense Advance Research Project Agency (DARPA), which is the central research organization of the Department of Defense (D.O.D.). A confidential memo elaborating further the possible military applications indicated here was submitted on June 20, 1983 also to Carl Romney at DARPA.

Jointly, I prepared myself to apply for the U.S. Citizenship in order to be able to conduct classified research.

Regrettably, DARPA decided to follow the guidelines already in force at NSF and DOE, that is, rejection of all I.B.R. applications. In fact, DARPA rejected or expressed no interests, not only for the primary application for the hadronic mechanics, but also for all remaining applications submitted by our Institute. All this, despite the character of the applications manifestly aligned with the "stars wars" guidelines, the credibility of the investigators (mostly full professors with large scientific records), and the minimality of the funds that would have kept the program alive (about $ 70,000 per year).

The fellow taxpayer should know that the applications to DARPA were the VERY LAST planned by the I.B.R. As president, I am now operating the Institute under a formal decision NOT TO APPLY to U.S. governmental agencies for research sup-

port, and this decision will remain in force for as long as decided by the I.B.R. Board of Governors. Only formal invitations will be selectively considered.

For reasons of security, I have excluded in the Documentation of this book the entire file dealing with U.S. military agencies that were unsuccessfully approached by I.B.R. members and/or by myself for research support, that is, not only with DARPA, but also with the research divisions of the Air Force and of the Navy.

An additional information is needed for the fellow taxpayer to reach a mature appraisal of the current funding of research in the U.S.A. It is the fact that no known or otherwise conceivable military (and/or economic) application exists for quark theories on hadronic structure. This situation should be compared with the structure model of hadrons reviewed in this section, for which considerable military (and economic) applications are indeed conceivable. Despite that, the former theories receive the totality of public funds in the sector, while no public funds whatsoever are invested in the latter theories.

The doubt persists in my mind that this rather ackward situation is due to the fact that the former theories are aligned with vested interests on Einstein's ideas, while the latter theories are not.

Violation of the three historical rules of atoms and nuclei by the quark models of hadronic structure.

As editor of a journal in theoretical physics, then a member of the Department of Physics of Harvard (we are talking of early 1978), I felt obliged to bring to the attention of the particle physics community the fact that quark models of hadronic structure violate all three historical rules which had resulted essential for the resolution of the structure of atoms and nuclei.

The introductory part of ref. [14] was in fact dedicated exactly to this issue, which was subsequently expanded in monograph [11], and later on reconsidered in paper [49].

First, one single model, the quark model, was assumed as resolving the totality of the hadronic phenomenology. To be explicit, the quark model was assumed as providing a classification of hadrons into families and, jointly, the structure of each individual member of a given family. This is evidently contrary to historical Rule 1.

Second, according to incontrovertible experimental evidence, the quark constituents are not produced free in any spontaneous decay or collision. This is evidently contrary to historical Rule 2.

Third, the number of quark constituents does not necessarily increase with mass, and actually remains the same for all members of the same family. For instance, according to the ori-

ginal quark models, one quark and one antiquark are the constituents, not only of the neutral pion, but also of the charged pions, as well as kaons and all other members of the so—called octect of light mesons. This is evidently contrary to historical Rule 3.

The clear validity of quark models for the hadronic classification and their problematic aspects when assumed as actual structure models.

I believe that the so—called unitary models (from which quarks originate) provide the final classification of hadrons into families. They are, therefore, the Mendeleyev table for hadrons. I clearly expressed this view in the locally quoted literature. The same view is shared by the majority of physicists.

All the reservations, problematic aspects, and shear inconsistencies originate when one assumes that the same models actually provide the structure of each individual hadron. Bluntly stated, the conjecture that quarks are the ultimate, elementary constituents of hadrons is afflicted by a litany of unresolved problematic aspects and shear inconsistencies.

Quarks are representations of the Lorentz group and of suitable, internal, unitary groups (such as the celebrated SU(3) group). the former part implies that quarks exist in our physical space—time, that is, they are physical constituents of hadrons. The latter part implies that they jointly possess an internal space producing the classification.

One of the biggest historical successes of atomic physics was the achievement by Bohr of equations of structure capable of representing ALL characteristics of the hydrogen atom, such as: size, charge, energy, exited states, etc. A similar situation occurred for the lightest known nuclear structure, the deuteron, even though available structural equations are often unsatisfactory (e.g., because of the general admittance of excited states contrary to experimental evidence).

In the transition to quarks, similar equations of structure are basically missing to this day. In fact, we do not have any equation of structure of the light mesons.

The technical difficulties are the same as those for the structure model of the neutral pion (rest mass of the constituent quarks much smaller than the total mass), but there are additional problems. In fact, a consistent equation of quark structure for the light mesons should contain only eight states, and all of them should have the proper values of the mass and other quantities. Structure equations of this type simply do not exist. The reason indicated in ref. [14, 49] as probable is precisely the violation by quark models of the three historical rules.

By comparison, the eletonic structure model, despite its

rudimentary character, achieved a consistent structure model of the pions since its initial proposal, by reproducing ALL intrinsic characteristics of the particles via structural equations of Bohr type. Apparently, this was possible because the model was constructed according to the historical rules.

Another problematic aspect of the quark models is that of confinement. If the taxpayer inspects the contemporary literature on quark theories (see, for instance, the quite readable review [85]), he/she will find the insistence ~~of~~ the construction of the structure model exactly according to the atomic structure and its underlying mechanics.

But, on strict scientific grounds, these assumptions imply the irreconciliable invalidation of the quark structure model (only, and not of the classification). In fact, the more the physicist insists on the compliance with quantum mechanics, the more evident is the existence of a finite, non—null, probability of tunnel effect for free quarks contrary to the experimental evidence.

I believe that this aspect alone has sizable ethical implications, and I shall dwell on them later on.

Par contre, the eletonic structure model resolves this problematic aspect. The free production of the constituents is assumed "ab initio" precisely because of the impossibility to confine physical particles within small regions of space.

The quark models of structure have been plagued by a considerable number of additional problematic aspects and/or inconsistencies, that either I noted on my own, or they were brought to my attention by ethically sound referees during my editorial functions.

One particular aspect (which is at the basis of an episode recalled at the end of this section) deals with the incontrovertible inconsistencies of certain nonrelativistic quark models that were fashionable in 1979—1980. I am referring to Galilean treatments of quark models, either per se, or as suitable limits of more general models.

As we shall see below, these models violated beyond any reasonable doubt numerous, independent, necessary, conditions for the applicability of the Galilean relativity.

By comparison, this additional inconsistency of quark structure models is resolved by the eletonic model. In fact, the latter model assumes the violation of Galilei's relativity and works out a suitable generalization.

To avoid excessive length, the interested taxpayer is referred to the locally quoted references for the remaining part of this third litany of problematic aspects (the first being that for Einstein's gravitation, and the second that for the origin of irreversibility).

In summary, there exist a considerable number of elements according to which the unitary classification of hadrons into

families is of final physical character, but the joint quark models of structure of each individual element of a given family, are still inconclusive because afflicted by several, unresolved, fundamental problems when considered within the context of conventional quantum mechanics.

To avoid misrepresentations on the scientifically constructive intent of the above remarks, let me indicate that, even in case the quark hypothesis on the hadronic structure is invalidated by future evidence, this would basically leave unchanged the beautiful achievements of the theory. In fact, these achievements are essentially of classification nature, such as the prediction of new particles from the knowledge of existing ones. As a result, one cannot exclude the possibility of reformulating the theory at the pure classification level, via a suitable re—interpretation of the numbers currently attributed to quarks (for instance, the quantities currently thought to be the masses of the various quarks could, in the final analysis, result to be suitable parameters mixing different representations of the unitary groups, and the like).

Use of the hadronic mechanics for the identification of quark constituents with the ordinary electrons and positrons.

Incontrovertible experimental evidence establishes that the scattering of the (negatively charged)electrons on the (positively charged) positrons can produce all hadrons,

$$e^+ + e^- \rightarrow \text{hadrons}.$$

Vice versa, hadrons generally admit spontaneous, sequential decays whose ultimate, massive, elementary products are precisely electrons and positrons (plus the massless photons and neutrinos).

It is then rather natural to assume that the hadronic constituents in general, and the quark constituents in particular, are the ordinary electrons and positrons.

As well known, this hypothesis is inconsistent when conventional quantum mechanics is assumed as exactly valid in the interior of hadrons. However, the hypothesis can be consistent under a suitably generalized mechanics. In fact, the hadronic generalization of quantum mechanics has been proposed precisely to achieve a consistent structure model of hadrons whose constituents are the ordinary electrons and positrons.

In particular, hadronic mechanics can well "build" quarks as suitable granules of electrons and positrons, when in the conditions of deep mutual overlapping indicated earlier.

The main ideas are essentially simple. In conventional

quantum mechanics, electrons and positrons obey the Lorentz symmetry resulting into given, fixed, physical characteristics. Under the high nonconservative conditions due to motion within hadronic matter, the same electrons and positrons can be interpreted as verifying suitable Lie–admissible generalizations of the Lorentz symmetry.* In clustering these Lie–admissible mutations of electrons and positrons into granules, one can reach all physical characteristics of quarks, including their fractional charge.

In short, hadronic mechanics offers novel possibilities for the future resolution of the ultimate problem of hadronic structure: the identification of the hadronic constituents with physical particles.

Use of hadronic mechanics for the achievement of a strict form of quark confinement.

Academicians can manipulate their human academic environment, but not physical laws. If quantum mechanics is assumed as exactly valid in the interior of hadrons, the probability of tunnel effects of free quarks CANNOT be reduced to zero. As a result, the assumption of quantum mechanics and the achievement of a true confinement of quarks are intrinsically incompatible.

The best academicians can do is to minimize the probability of tunnel effects for free quarks (qualitative confinement) via the selection of suitable potentials. But the achievement of a strict confinement (identically null probability of tunnel effects for free quarks) is and will remain unachievable within the context of quantum mechanics. The phenomenon of barrier penetration is directly dependent on the basic laws of quantum mechanics and simply cannot be annulled without altering the same laws, that is, without subjecting quantum mechanics to a suitable generalization.

As a result, the generalization of the underlying mechanics is needed, not only for the identification of quark constituents with physical particles, but also for the resolution of the biggest problematic aspect of current quark theories: the achievement

*This is technically realized via two sequential generalizations. First the modular action of symmetry groups on the underlying carrier space (the Hilbert space) is lifted from the conventional modular form $A\psi$ to the isotopic form $A*\psi = Ag\psi$, where g is the isotopic operator indicated earlier in this section. This produces a Lie–isotopic generalization suitable for closed–exterior treatments [54]. Nonconservative conditions for each constituents are achieved via a differentiation between the right and left modular–isotopic action, thus resulting in the so–called Lie–admissible bimodules [86–88]. In fact, the differentiation implies the lack of conservation of physical quantities, trivially, because the product characterizing the time evolution is no longer antisymmetric (Figure 1.6.2).

of a strict confinement.

Again, hadronic mechanics appears to possess unique features for the achievement of a strict form of quark confinement.

The main ideas are simple and deserving an outline. Recall that quarks are representations of the product of two Lie groups, the Poincaré group and a suitable unitary group. The former acts in our physical space while the latter acts on a mathematical, internal space.

Assume now that hadronic mechanics is valid for the interior of hadrons, while conventional quantum mechanics continues to remain valid for the exterior case. This evidently implies a differentiation between the interior and exterior mechanics beginning from the fundamental physical principles (Heisenberg's uncertainty principle, Pauli's exclusion principle, etc.). The possibility of achieving a strict quark confinement is then consequential. For example, it can be achieved via differentiations between the interior and exterior dynamics such to render incoherent the related Hilbert spaces. In turn, this latter aspect can be achieved, for instance, via the realization of hadronic mechanics reached by the Argentinian physicist A. Kalnay [89–91] currently at the I.V.I.C. Institute in Caracas, Venezuela. In fact, Kalnay's mechanics has a phase space structure which is fundamentally different than that for the exterior conditions. A strict quark confinement is then expected.

It should be stressed that these results are conceivable without any alteration of the current quark theories, as far as their physical results are concerned. This is technically due to the fact that, according to hadronic mechanics, quarks would be realizations of suitable, Lie–isotopic generalizations of the Poincaré and unitary symmetries. Now, these generalizations have resulted to be locally isomorphic to the conventional ones (see ref. [32] for the Lorentz case and ref. [67] for the unitary one). In turn, this local isomorphism implies the possibility of preserving all essential quark characteristics under lifting.

I can therefore conclude by saying that a considerable number of seemingly independent aspects suggest the need to construct a generalization of quantum mechanics in the transition from the atomic to the nuclear–hadronic structures, with the understanding that quantitative predictions from quantum mechanics are expected to be minimal in the nuclear structure and higher in the hadronic structure. These elements range from the need to identify the origin of irreversibility, to the need for consistent bound states with very light constituents, to the need for a strict form of quark confinement.

Owing to the direct universality of the Lie–admissible algebras, the hadronic generalization of quantum mechanics is the structurally broader generalization available at this time. In fact, other generalizations proposed in the literature are all parti-

cular cases of hadronic mechanics. I am referring to the so—called supersymmetric, gauge, rigged and other extensions, as well as to nonlocal, nonlinear and discrete generalizations.

What is unknown to this writing is the particular form of realization of hadronic mechanics that actually holds within hadronic matter. This, however, is primarily an experimental problem, as indicated in the next section.

But the need for a generalization of quantum mechanics under strong interactions should be out of the question. After all, quantum mechanics is basically unable to represent the conditions of mutual penetration of wave—packets which are necessary to activate the strong interactions.

As stressed earlier in this section, particle physics is the last branch of science still anchored to Hamiltonian formulations, while all other branches have passed to structurally broader treatments, resulting in the current lack of unity of physical and mathematical thought.

When unity of science will be one day restored, this can only be done by abandoning Hamiltonian theories also in particle physics in favor of broader theories. The validity of hadronic mechanics within hadronic matter will then follow from its direct universality. It is only a matter of time.

I want to leave a record of this prediction in this book.

The incredible academic politics on quarks.

The word "quark" is an ultimate representative of huge, vested, academic—financial—ethnic interests in the entire U.S. physics, including the academic, corporate, and military sectors.

To understand this, the fellow taxpayer must be informed of a number of aspects, all concurring toward the same interests.

First and foremost, quarks are thought to obey Einstein's special relativity, or at least this is the official version imposed by academic barons in the field. The preservation of the relativity therefore puts quark theories aligned with all vested interests on Einstein's ideas.

Second, quarks are thought to obey conventional quantum mechanics or, again, this is the official version imposed by academic barons. As a result, quark theories are aligned with the vast interests surrounding quantum mechanics, including the corporate and military sectors.

Third, quarks are thought to be a manifestation of Lie's theory, or, again, this is the version imposed by academic barons. But Lie's theory is the hearth of contemporary mathematics (see Section 1.8). As a result, quark theories are aligned with the additional (not ignorable), vested interests in mathematics.

The combination of concurring interests in special relativity, quantum mechanics and Lie's theory, is the secret of the success of quark theories.

The achievement of such a vast combination of vested interests is all based on one central conjecture, that the quarks are point–like. In fact, as elaborated in this chapter, the assumption that quarks are point–like implies the validity of special relativity, quantum mechanics, and Lie's theory, beginning with the local–differential character of the underlying geometry, and then passing to the Hamiltonian character of the underlying mechanics.

The fellow taxpayer will remember the litany of inconsistencies of Einstein's gravitation (Section 1.5). The litany of inconsistencies of current quark theories is perhaps longer.

The hypothesis that quarks are point–like is purely political and deprived of true physical content. In fact, any person, to be a physicist, must know that: (a) quarks possess a wave–packet; (b) that wave–packet has the size of a hadron; and, therefore (c) the wave–packets of quarks must be in conditions of deep mutual penetration in the interior of hadrons. This activates directly the invalidation arguments of the locality of the theory (Figure 1.6.1).

As a result, the mathematical foundations of the special relativity, beginning with the local–differential character of the underlying geometry, cannot be exact for quarks.

Stated differently, "point–like wave–packets" may exist as a figment of academic imagination, but not in the real world.

But perhaps more evident is the invalidation due to lack of achievement of a strict form of confinement.

Recall that current quark theories are based on the assumption of quarks as physical constituents of hadrons which obey quantum mechanics, while no quark has ever been observed to date in the spontaneous decays of hadrons or in hadronic collisions up to the highest possible energies in available particle accelerators throughout the world. Now, one of the pillars of quantum mechanics is Heisenberg's uncertainty principle. According to this principle, when a quark is close to a potential barrier, it possesses a finite, non–null probability of being beyond the barrier (tunnel effect), that is, of being free, contrary to experimental evidence. The selection of an appropriate barrier can reduce the probability, but no theory can render it identically null, unless Heisenberg's uncertainty principle and other laws of quantum mechanics are abandoned in favor of suitable generalizations. But this implies abandoning quantum mechanics in favor of hadronic mechanics, as indicated earlier.

A point the fellow taxpayer has the right to know is that any quark model with a finite, non–null, probability of tunnel effect of free quark is intrinsically inconsistent. Period!

Another point the taxpayer must know is that orthodox papers on quark theories do not compute explicitly the probability of tunnel effect, to my best knowledge (evidence of

the erroneous nature of this statement, and the reference to published articles with explicit calculations of the probability would be gratefully appreciated).

Also, the taxpayer should be cautious in accepting claims of "confinement" within the context of a quantum field theoretical description of quarks known under the name of "quantum chromodynamics" (QCD). In fact, the underlying equattions are, in general, nonlinear partial differential equations of unknown solution. In order to separate academic politics from the pursuit of physical knowledge, the achievement of a strict form of confinement must be first achieved at the level of quantum mechanics. Only thereafter the claims of having achieved confinement at the more general QCD level can be accepted by the scientific community at large, that is, including scientists not aligned with vested interests on quarks.

The problems of scientific accountability raised by this issue alone are staggering. Huge amounts of public funds are dispersed every year on quark models by the U.S. National Science Foundation, the Department of Energy, and other governmental agencies. A significant part of these funds have been spent for years, and continue to be spent to this day, on quark models that are intrinsically, demonstrably inconsistent. Yet, they are supported by leading "peers" in leading academic institutions and, as such, funded.

We are facing here tight governmental–academic circles much similar to those in gravitation and irreversibility, that is, without any foreseeable possibility of self–correction. Governmental agencies will continue to submit grant applications on quarks for review to leading experts on quarks at leading academic institutions. In turn, these "peers" will continue to ignore the lack of strict quark confinement. The governmental agencies will therefore continue to fund applications that are intrinsically inconsistent. After all, why should they change a routine happily followed for decades?

An outside intervention by the taxpayer is the only hope for scientific advances and for improvements of the scientific accountability in the sector.

The means are known. The methods to compute the probability of tunnel effect are taught in undergraduate courses in quantum mechanics. Most physics students are therefore able to compute the probability of tunnel effects for free quarks whenever the essential elements are given., that is, whenever the students know the mass of the quark, the explicit form of the "confining potential" and a few other data. If the probability of tunnel effect is "identically null", the model is consistent; otherwise, the model is inconsistent. Silence in the computation of this probability, as fashionable in the current technical literature, can only multiply the problems of accountability and resolve none.

A serious study of this ethical profile is recommended

here. If not conducted in the U.S., it will be likely conducted abroad.

The study should consider papers immediately following the original formulation in 1964 of the quark conjecture by the U.S. physicist M. Gell–Mann [92], and include papers up to the recent ones. All these papers carry their federal research contracts. The administrative profile can therefore be readily retraced, whenever needed. References to primary papers in the field are readily identifiable and need not be quoted here.

We are therefore talking about known papers in quark theories published under governmental support during the past twenty years. All these papers should be subjected to the calculation of the probability of tunnel effects for free quarks. They can be classified into three categories: the first, with a large probability of tunnel effect (this group contains most of the initial papers); the second with a small but non–null probability of tunnel effect; and the third with hopes of achieving a strict form of quark confinement.

The value of a study of this nature for future orientation and funding of research in the sector is evident.

Note that I am not recommending that research projects without strict confinement should remain unfunded. I am only insisting on the need of scientific honesty. Quark models with a "qualitative" confinement, that is, with a finite, non–null, probability of tunnel effect of free quarks contrary to evidence, should state so, clearly, in all printed papers. In turn, the clear identification of the problem is essential for its resolution.

Whether the current governmental funding of research in quark theories warrants or not an outside intervention by the taxpayer, one point should be crystal clear. The opinions by leading quark experts at leading U.S. institutions should remain what they are: opinions expressed by physicists with decades of vested interests in the dismissal of the problem of confinement. As such, the "peers" used by governmental agencies in grant refereeing are the very least qualified to pass judgment on the inconsistencies of their own grants.

The episode of the paper of criticisms on quarks I wrote at Harvard and distributed in 15,000 copies.

In anticipation of the more detailed report of Section 2.1, at the end of Section 1.3, I have presented a preliminary outline of the opposition I have encountered at the Department of Physics of Harvard University in 1977–1978 in the conduction of my research (need for experimental tests on the validity or invalidity of Einstein's special relativity and Pauli's exclusion principle in the interior of hadrons–see the title of memoir [14] written precisely at Harvard's physics department in early 1978).

After passing to the Department of Mathematics in June 1978, while regularly receiving my salary under my own grant

from the Department of Energy (contract number ER–78–S–02–47420.A000 for the period June 1, 1978 until May 31, 1979), I thought that my problems were over for a while. I therefore plunged myself into the drafting and re–drafting of the monograph on the Birkhoffian generalization of Hamiltonian mechanics (subsequently published in 1982, ref. [10]).

But I was wrong.

In early 1979, Harvard filed a formal application to the Department of Energy for the renewal of my contract for one additional year (from June 1, 1979 until May 31, 1980). The application was filed after passing all the various layers of administrative approvals, from my department, to the office of the dean, and to the office of research contracts. In particular, the Department of Mathematics had approved the submission of the application to D.O.E. with my affiliation to the same department for one second year.

The D.O.E. promptly approved the application for funding under the new contract number AS02–78ER4742. The D.O.E. notification arrived jointly to Harvard's administration and to me. I felt reassured. At least I could feed and shelter my children and my wife (then still a graduate student) for one additional year, while doing research in physics. I therefore plunged myself into the studies for monograph [10] with renewed scientific ardor.

This happy status was short lived. One day in early April 1979, the chairman of Harvard's Department of Mathematics for that year, Heisuke Hironaka, came to my office.

Our relationship, at that time, was of utmost mutual respect and cordiality. I therefore invited Hironaka to sit in my sofa, and relax. He had visible difficulties in telling me what was going on. After some gentle pressures on my part, he came to the point, indicating that there were "insurmountable difficulties" for my staying one additional year at Harvard.

I reminded him that his department had formally approved the filing of my application to D.O.E., which had been subsequently approved by Harvard's administration and then funded by the D.O.E. He confirmed the awareness of these facts, but re–stressed the impossibility of my stay at Harvard for one additional year.

At one point, Hironaka stressed emphatically that I had to terminate my stay at Harvard at the end of the D.O.E. contract then in effect, that is, at the end of the following month.

I indicated to him that I had two children to feed and shelter and that, under no circumstances would I be able to find another job in such a short time. I also indicated to Hironaka that the attempt to transfer my contract to another university would raise a host of questions, beginning with the basic question: Why Harvard did not want to administer a contract that had already been formally filed and approved?

I therefore asked Hironaka to disclose the reasons of the "absolute impossibility" for my staying there one additional year with my own money, while giving to Harvard the gift of a significant amount of overheads.

I attempted to bring him to the reality of the inevitable consequences at the various levels, in Cambridge and in Washington, not to exclude evident legal implications. Also, the disclosure of the reasons for the "absolute impossibility" would have been important to attempt a friendly resolution of the case to the benefit of all people involved, including those opposing the continuation of my stay.

At one point, Hironaka finally ceased to resist, and told me what was going on. In essence, to draw my salary under the formally approved grant, I needed the renewal of my appointment there as a member of the Department of Mathematics. In turn, he had encountered "insurmountable difficulties" in reaching such a renewal. The senior high energy physicists at the Department of Physics of Harvard had reiterated (AGAIN!) their judgment of "lack of physical value" of my studies. In turn, this had created an evident, apparently intended deadlock at Hironaka's department. I was a theoretical high energy physicist and not a mathematician. As a result, the members of the mathematics department had to rely on the judgment of the senior high energy physicists at Harvard in order to reappoint me. The negative judgment at the physics department had therefore implied the consequential negative judgment at his department. In particular, the opposition at the physics department was so great to create an "absolute impossibility" for the renewal of my appointment.

I thanked Hironaka sincerely for the information (that I had suspected anyhow), and indicated that I would make one final attempt for an "orderly" solution of the problem within the mathematics department. Nevertheless, before he opened the door, I brought to his attention the extreme gravity of the occurrence.

That same night I initiated the writing of a paper of constructive critical examination of the litany of problematic aspects of the quark conjectures. The paper was subsequently completed in a preliminary form on April 19, 1979, under the initial title: "An intriguing legacy by Albert Einstein: the expected invalidation of quark conjectures". The paper was thereafter printed and distributed in 15,000 samples (as stated in the front page) thanks to funds and logistic assistance provided by the printer of the Hadronic Journal. The paper was subsequently subjected to a number of revisions, and finally printed with an expanded and edited title in *Foundations of Physics* in 1981 (see ref. [49]).

As everybody can see, the paper presents a litany of argumentations dismissing the possibility that quarks exist as con-

ceived at that time at Harvard (as well as throughout the world), that is, as the ultimate, "elementary", and therefore indivisible constituents of hadrons. In particular, the paper re–stressed the final physical value of the theory for the Mendeleyev–type classification of hadrons and restricted the critical analysis only to the structural profile. The inspiration of the paper was constructive, as stated beginning from the abstract. The hope was that of stimulating a consideration of the problems by independent researchers in the field as a prerequisite for their solution.

The argumentations were those presented in this chapter, that is, the various reasons why we expect the lack of exact character of the special relativity in the interior of hadrons. But quarks are manifestations of the special relativity, as recalled earlier. Departures from the special relativity, if experimentally established, would then imply the impossibility for quarks to be elementary.

By April 28, 1979, the paper had been printed, and the distribution of the 15,000 copies had begun. I still remember car loads of boxes of individually addressed copies of the paper being distributed to Harvard University, M.I.T., Tufts University, Boston College, and the other universities of the Boston area, while heavy shipments were mailed to all other high energy research institutions throughout the world.

On April 29, 1979, I wrote a letter to all members of the Department of Mathematics at Harvard for an orderly solution of the case. The letter, written in the most respectful possible style, appealed to the scientific ethics of the addressees, as well as to the need for scientific freedom at Harvard.

At the subsequent faculty meeting, the Department of Mathematics formally approved the renewal of my appointment for one additional, but terminal year.

These are the events that forced me to interrupt the studies for monograph [10] and, against all my plans and wishes, forced me into the writing of a paper of criticisms on quarks.

Besides fulfilling the purpose of a scientific presentation of my views on quarks to the members of Harvard's mathematics department, paper [49] appears to have been totally useless on scientific grounds. In fact, the paper was never quoted by any physicist at Harvard, nor has ever been quoted in any paper on orthodox quark lines (evidence to the contrary would be gratefully appreciated).

To understand this occurrence, the taxpayer should know that: (a) no physicist in quark theories can claim lack of knowledge of the paper, owing to the quite unusual volume of distribution of the preprint, followed by the publication and subsequent mailing of reprints; (b) the idea that quarks cannot be elementary, but must be composite, is routinely accepted these days, as indicated earlier in this section; and (c) paper [49],

even though unquoted, was and remains the first to present comprehensive argumentations on the impossibility for quarks to be elementary.

But, above all, the most distressing aspect is that the call launched by paper [49] (to test the validity of Einstein's ideas in the interior of hadrons) has remained unanswered to this day.

The moratorium of early 1980 in the publication of papers at the Hadronic Journal in non—relativistic quark theories.

Every relativistic model (that is, model verifying the special relativity) must admit, for consistency, a valid nonrelativistic limit (that is, a low speed limit verifying Galilei's relativity). The non—relativistic limit of quark theories (which are generally formulated within a relativistic setting) has therefore been studied since the early stages of the theory.

Severe doubts on excessive inconsistencies of non—relativistic quark theories had crossed my mind for years, and increased in time. One day, the issue exploded in my editorial hands in all its force.

In late 1979, I received a paper in non—relativistic quark theories submitted to the Hadronic Journal. At that time, my editorial office was room 435 of the Department of Mathematics at Harvard University.

I submitted the paper to two referees. The first was a leading expert in quark theory at a leading U.S. institution. The second was an applied mathematician, expert in nonrelativistic quantum mechanics, with a record of independence from vested interests on quark lines. The first referee recommended publication of the paper, while the second rejected the paper quite firmly.

The inability to resolve their differences forced me to implement a moratorium in the publication of papers in non—relativistic quark models. The case was reported in an open letter to editors of other Journals dated January 8, 1980, as well as in a following open letter to mathematicians interested in quantum mechanics dated March 19, 1980 (see Doc. p. I—316).

The main issues are the following. The non—relativistic limit of quark theories generally characterizes a Hamiltonian with a structure of the type: $H = aA(r) + bB(r)p + cC(r)p^2 + dD(r)p^4$ + higher powers in p, where: A, B, C, D are functions of coordinates r; p is the canonical momentum; and a, b, c, d are constants.

These models possess the following inconsistencies (mostly valid to this day).

(1) The models violate Galilei's relativity. Recall that the non—relativistic limit was studied precisely in the hope to reach a consistent Galilean setting as one element needed to

prove the consistency of the original relativistic formulation. Therefore, the violation of Galilei's relativity invalidates the very motivation of the study. The violation was proved beyond any reasonable doubt by the referee in applied mathematics. In essence, one of the necessary conditions for the verification of Galilei's relativity in quantum mechanics is the verification of the so–called Mackey's imprimitivity theorem [93]. In turn, this theorem is manifestly violated by all Hamiltonians with momentum powers higher than two.

(2) The models violate the conservation laws of the total energy, linear momentum, angular momentum and other physical quantities. This second aspect was established, also beyond any reasonable doubt, by the necessary and sufficient conditions for given forces to admit a potential energy [9]. In fact, one theorem of this latter theory implies that the total energy is not conserved for all "potentials" with momentum powers higher than two. A similar situation occurs for all other physical quantities. In short, the models were intended to describe closed–isolated hadrons, but in actuality resulted to violate all total conservation laws. Of course, the Hamiltonian H(r,p) is conserved in time. The point is the H does not represent the total energy under the conditions considered. A similar situation occurred for other physical quantities.

(3) The probability of tunnel effects for free quarks was excessively high. This third point was also proved beyond a reasonable doubt. It merely implied the use of actual physical quantities, rather than the canonical ones (that is, the use of the total nonconserved energy, rather than the conserved Hamiltonian, etc.).

A number of additional inconsistencies and problematic aspects also existed, such as the loss of the equivalence between the quantum mechanical, Hamiltonian and Lagrangian representations, the activation of the theorems preventing a consistent quantization, etc. For a review, the interested reader may consult paper [94].

It is evident that the problematic aspects of the papers were simply too big and too many to be ignored. There must be a limit beyond which leniency in scientific insufficiencies becomes complicity with aligned interests.

This is the reason why I imposed a moratorium in the field at the Hadronic Journal and, in addition, I felt obliged to bring my findings to the attention of the editors of other journals in particle physics. I did this in full knowledge that the information would be damaging to me, as it did! In fact, an anonimous referee subsequently rejected one of my research grant applications by quoting, among other things, precisely my

open letter to the editors on this issue (see Section 2.5). Evidently, this referee was a quarkologist who felt threatened by my desire to do physics, rather than pursuing academic politics.

The reactions of the editorial community resulted to be a perfect image of the academic politics in the field. In essence, the editors of (U.S.) independent journals reacted with interest and cooperation, while those aligned or controlled by quark interests attempted to discredit my efforts, or to ignore them altogether.

For instance, the U.S. physicist David Finkelstein of the Georgia Institute of Technology, and editor of the International Journal of Theoretical Physics, reacted with keen interest. In particular, his constructive comments resulted to be invaluable in improving our understanding of the technical issues, and I shall remain always grateful to him for that.

Par contre, the U.S. physicist George L. Trigg of the American Physical Society, editor of Physical Review Letters (the leading journal of the society), reacted in a rather incredible way. I had mailed him (and to a number of other A.P.S. editors) all possible information, including copies of papers and of proceedings of workshops in related topics. His answer is reproduced below.*

PHYSICAL REVIEW LETTERS, Editorial Office, 1 Research Rd. Ridge, New York, N.Y. 11961, tel. (516) 924 5533

May 22, 1980

Dr. R. M. Santilli
Department of Mathematics
Harvard University
Cambridge, Mass. 02138

Dear Dr. Santilli:

Thank you for lending me the material from the workshop on Lie admissibility. I apologize for having kept it longer than the two weeks or so that you had suggested; I hope that this did not cause you any difficulties.

I find, to my regret, that my familiarity with modern abstract algebra is sufficiently sketchy that I was not really able to appreciate much of the argument. I cannot help feeling, however, that your campaign calls for much more drastic action than is really warranted. As you must be aware, this is not the first instance in which physics theory has made progress on the basis of questionable mathematics, nor is it likely to be the last. I do not mean in any

sense to disparage the work that you and others are doing to try to provide a sounder basis; but I do not feel that a moratorium of any sort would be useful.

I thank you again for lending me the material, and I offer my wishes for success of the forthcoming workshop. I regret that my schedule does not permit me to attend.

Sincerely yours,

George L. Trigg
Editor

GLT/jaw

As one can see, Trigg dismissed the moratorium on grounds that the deficiencies were mainly of "questionable mathematics". Instead, the deficiencies were of purely physical nature and of primary physical relevance at that, such as: the invalidation of Galilei's relativity; the violation of the conservation of the total energy; the excessively high probability of tunnel effects of free quarks; etc.

The taxpayer can therefore draw his/her own conclusion. The fact remains that, at the Journals of the A.P.S., papers in non—relativistic quark theories continued to be printed without any consideration whatsoever or mention of the literature on the problematic aspects considered here. As far as the Journals of the A.P.S. were concerned, my efforts to stimulate a moment of reflection on the excessively big inconsistencies of non—relativistic quark models were a total waste of time.

Note that the scope of my action was not the suppression of research in the field. Not at all. Instead, the objective was the clear identification of open problems as a prerequisite for their solution.

It is hoped that the fellow taxpayer will remember this epidose when reading Section 2.4 on my experience with the journals of the A.P.S. In fact, all rejections of papers submitted to A.P.S. journals should be always compared to the quality and consistency of the papers routinely published, such as precisely the papers on nonrelativistic quark conjectures. I am referring to the rejection of the experimental paper on nuclear irreversibility by Phys. Rev. C, subsequentily published in Europe (ref. [105]), of the theoretical paper on hadronic mechanics and the possible internal irreversibility of strong interactions, rejected for over one year by Phys. Rev. Letters and Phys. Rev. D, subsequently published also in Europe (see ref. [59]), and too numerous other cases. All these rejections of papers not aligned with vested, financial-academic-ethnic interests on Einstein's ideas, should always be compared to the routine

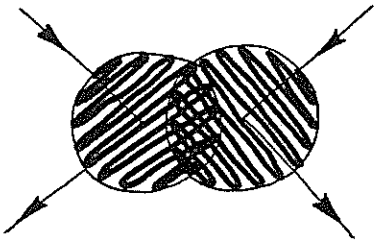
publication of papers aligned with vested interests, irrespective of their inconsistencies and problematic aspects.

My invited talk at the 1980 Conference on Differential Geometric Methods in Mathematical Physics at the University of Clausthal, West Germany.

In early 1980, H. D. Doebner of the Theoretical Physics Department of the University of Clausthal, West Germany, invited me to deliver a talk at the yearly *Conference on Differential Geometric Methods in Mathematical Physics,* to be held at his institute the subsequent July.

The conference is generally attended by the leading experts in applied mathematics and theoretical physics. I saw a unique opportunity to draw attention on the limitations for strong interactions of conventional algebras, geometries and mechanics. My hope was that, in doing so, I could stimulate some of the best minds toward the natural future step: the construction of suitable generalizations specifically conceived for the strong interactions.

I began my talk by projecting on the big screen of the conference room the symbol of this book: extended wave—packets in conditions of mutual penetration and overlapping, as experimentally established for the strong interactions.



As stated during the talk, my task would have been accomplished if the participants had remembered the physical reality of the diagram above, after the conference, when returning to their research activities.

The diagram provides evidence of the lack of exact character of the algebras, geometries and mechanics used for the strong interactions at that time, and continued to be used to this day. As familiar from the preceding review, the diagram identifies the incontrovertible evidence according to which strong interactions are non—local (that is, distributed throughout a finite volume of space), thus implying the insufficiency of all currently preferred geometries such as the symplectic geometry (which are precisely of local—differential character). In turn, this implies the insufficiencies of the Lie algebras, beginning with the Lorentz and Poincaré algebras of the special relativity, because of the insufficiency of the underlying topologies and other

reasons. Finally, the diagram depicts the insufficiencies of currently preferred mechanics, because of the contact/non—Hamiltonian nature of the interactions.

To illustrate the implications to the conference participants, I outlined the status of our knowledge at that time on the expected deformation of the charge distribution of hadrons under external strong interactions, with the consequential mutation of the intrinsic magnetic moments, as reviewed earlier in this section. The quantitative treatment was conducted via the Lie—admissible generalization of the conventional, quantum mechanical, Lie treatment of the rotational symmetry. The embedding of the Lie treatment into a covering Lie—admissible one, was intended to represent the open/non—conservative character of one hadron under external strong interactions.

I concluded my talk with a review of the status of our experimental knowledge on the rotational symmetry which was intriguingly favoring the mutation of the magnetic moments as well as of the spin, although yet inconclusive (see next section).

The transparencies of my talk were subsequently expanded into a paper published in ref. [62].

One can imagine the reaction of the audience to my talk. Mathematicians there were heavily committed to the local—differential character of the geometry, while theoreticians had a known history of vested interests on Einstein's ideas. The very view of the diagram above, despite its incontroverible reality, was anatema for most of them.

I still remember S. Sternberg of the Department of Mathematics of Harvard University leaving the conference room as soon as the diagram above appeared on the big screen, and I began the presentation of the nonlocality of the strong interactions.*

Upon conclusion of my talk, I remember a vociferous intervention by Y. Ne'eman of Tel—Aviv University, Israel, who attacked the very idea of testing the rotational symmetry under strong interactions. My answer was that we had a duty to resolve the issue one way or the other, because of the fundamental character of the rotational symmetry, on one side, when combined with the plausibility of the deformations of extended hadrons, on the other side. At any rate, the idea that extended hadrons are absolutely rigid has no scientific value, while the breaking of the rotational symmetry for deformed charge distributions can be seen by all. But, all my argumentations (later continued in the corridor) were useless. As well known, Y. Ne'eman is a renowned expert in quark theories and Einstein's gravitation. The physical conditions of the diagram above undermine the ultimate mathematical foundations of both quark theories and Einstein's gravitation as elaborated thoughout this chapter. The possibility of establishing a constructive scientific dialogue be-

*When he subsequently delivered his own talk, I evidently made it a point in leaving the conference room soon after its initiation.

tween Ne'eman and myself proved to be nonexistent.

Another criticism that I still remember is that by I. Segal of the Department of Mathematics of the Massachusetts Institute of Technology who, in subsequent conversations, warned me against the study of the conditions of the diagram, because "it would open a Pandora's box." I told Segal that the conditions of the diagram were not of my own invention, and that we had an ethical duty to consider seriously Enrico Fermi and other founding fathers of strong interactions, who had established a record of the non–locality of the theory. Such an historical record could not possibly remain ignored. The sooner we study it, the better.

For fairness, I must report one voice of support during the discussion following my talk, by the Irish physicist C. C. C. He recalled to the audience that, under my assumptions (one hadron in the open/non–conservative conditions due to external strong interactions) "all conventional Lie symmetries are expected to be broken, including the rotational symmetry". But his voice was lost in the sea of oppositions.

More recently, while organizing, in late 1983, a workshop on hadronic mechanics to be held at the beautiful Villa Olmo, on the edge of the Lake of Como in Italy (Center Alessandro Volta) in 1984, I invited K. Bleuler of the University of Bonn, West Germany, to be a member of the Organization Committee jointly with several other distinguished mathematicians and physicists. Bleuler was one of the founders and co–organizers of the Clausthal Conference. He was present at my talk there in 1980 and fully aware of the issues. My invitation was motivated by the fact that hadronic mechanics uses, among other tools, a certain generalization of the inner product of the Hilbert spaces of quantum mechanics that had been identified in the early 50's. Bleuler was the last living physicist of the original group who had identified the generalization [95]. His participation in the Organization Committee of the Como Workshop on Hadronic Mechanics would have been scientifically invaluable, even without physically attending the meeting.

Bleuler never acknowledged my invitation, nor the gentle solicitation by the Workshop secretary. Evidently, a few words of declination of our respectful invitation would have been sufficient. I must denounce Bleuler's silence because strictly anticollegial and antiscientific. In fact, his lack of answer produced considerable delays in the completion of the formal announcement of the meeting, with evident scientific damage.

Nevertheless, I would like to take this opportunity to express my utmost gratitude and respect for H. D. Doebner. By permitting a presentation at the 1980 Clausthal Conference of the ultimate roots of the expected inapplicability of Einstein's ideas under strong interactions, he fulfilled in full his scientific accountability as a scientist and as a conference organizer. What

happened afterward is the sole responsibility of the conference participants.

All in all, the experience of my participation at the Clausthal conference reinforced my conviction that the conduction of research on the expected invalidation of Einstein's ideas in the interior of hadrons is a total waste of time, and will remain a total waste of time until taxpayers intervene to force the implementation of strict scientific accountabilities in the sector.

This is why I halted all research, and considered my time better spent in writing this book.

Interruption due to the death of my mother.

On the afternoon of March 16, 1984, I received a phone call from Italy asking for my leaving immediately for Rome, due to a sudden illness of my mother who was dead at my arrival there the following morning. Work on this book was resumed on the afternoon of April 4, 1984.

She had gently followed and spiritually supported me throughout my life, and, in particular, during my difficult times recalled in Chapter 2. Monograph [10] on the Birkhoffian generalization of Hamiltonian mechanics was dedicated to her.

I wanted to have a record in this book of this unexpected event.

1.7: THE EXPERIMENTAL VERIFICATIONS OF THE VALIDITY OR INVALIDITY OF EINSTEIN'S IDEAS UNDER STRONG INTERACTIONS.

The approaching of the central ethical issues raised by IL GRANDE GRIDO.

The experimental tests on the validity or invalidity of Einstein's ideas under strong interactions (A) are fully within current technological capabilities, (B) are of quite moderate costs, particularly when compared to orthodox particle experiments, and last but not least, (C) the experimental information currently available, even though preliminary and still inconclusive, points quite clearly toward the violation.

Once the taxpayer has reached a sufficient knowledge of these aspects, a number of stormy questions follow quite naturally:

- *Why these fundamental experiments are not done?*
- *Why public money is spent in other experiments whose relevance is dwarfed by that of the tests on Einstein's theories?*
- *Who is behind this?*
- *What is the responsibility of presidents of national laboratories and leading colleges?*
- *Is there an organized conspiracy within the U.S. governmental—academic complex to impose a scientific obscurantism on Einstein's theories?*

and many, many more.

Information on the plausibility of the violation of Einstein's ideas has been provided in the preceding analysis. In this section, I shall provide the taxpayer with a review as simple as possible of the available experimental information.

But, upon achieving these tasks, my job would remain still incomplete. The same information can be reached by all people with scientific curiosity and time, trivially, because the information is available in research libraries.

To complete my job, I must present my experience as an insider. I must tell the episodes I have experienced during my (totally unsuccessful) attempts to have the governmental—academic complex at least consider the tests, let alone actually do them! Only then the taxpayer will have the elements to judge the gravity, depth and diversification of the questions above, and their potential implications for our societies.

Bits of the latter task have been occasionally included in the preceding sections. More detailed information will be presented in the next chapter.

The fellow taxpayer should be aware that the fundamental knowledge is and remains the scientific one. Only after achieving such a knowledge, the issues of scientific ethics and accountability can be truly mastered. As stated earlier, this chapter on the scientific profile is merely a guide throughout (part of) the technical literature. The taxpayer is therefore urged to complement this presentation with the reading of the quoted literature. Except the inevitable technical passages, most of the argumentations and conclusions are understandable by all. The reading of articles NOT authored by me is also essential to understand that, by no means, I am alone. On the contrary, I am only one among

numerous scholars on the limitations of Einstein's theories scattered throughout the world.

The fundamental experiments by the Austrian physicist H. Rauch on the tests of the rotational symmetry under strong interactions.

Recall the prediction of hadronic mechanics, that the charge distribution of hadrons can experience deformations under sufficiently intense external fields, with consequential breaking of the rotational symmetry and, consequently, of the special relativity.

This deformation/rotational–Lorentz–asymmetry can be readily subjected to experimental measures. In fact, it implies a (necessary) alteration of the intrinsic magnetic moments of hadrons, while the intrinsic angular momentum (spin) can remain unchanged for sufficiently low energies.

Experimental measures directly relevant for the above prediction have been conducted by the Austrian physicist H. Rauch (director of the Atominstitute of Wien), and his associates. The measures have been conducted at the Laue–Langevin Laboratory in Grenoble, France, via the so–called neutron interferometers (see Figure 1.7.1 for more details). The experiments tested the rotational symmetry of neutrons under external fields. The first measures were conducted in 1975 [96]. The tests were then repeated in the subsequent years [97,98,99]. The latest available measures are given in ref. [100].

The main ideas of the experiments are so simply, to be understandable by all. The intrinsic magnetic moment of neutrons renders them similar to small magnets. Under an external magnetic field due to an electromagnet, neutrons therefore rotate. The value of the neutron magnetic moments in vacuum is known. Thus, the field of the external electromagnet can be calibrated for one, two, or more "spin flips" or complete rotations.

When a neutron beam propagates in vacuum under the long range action of the electromagnet only, no deformation of the charge distribution and mutation of the magnetic moment is expected. To realize experimentally the physical conditions for activation of hadronic mechanics, the neutrons must be brought within the intense fields in the vicinity of nuclei. In this case, Eder's calculations [65] show about 1% deviation in the intensity modulation, a value well within current experimental capabilities.

Rauch's team reached, rather accidentially, the physical conditions needed for hadronic mechanics. Indeed, they filled up with Mu–metal sheets the electromagnet gap. This was done by the experimenters to reduce the stray fields. In actuality, by

letting the neutron beam to propagate within matter, they automatically reached the joint conditions of long range electromagnetic and short range nuclear interactions.

The first experiments [96] were conducted for neutron propagating in vacuum. Their results, therefore, have no value for hadronic mechanics. More recent experiments, however, have been conducted with the electromagnet gaps filled up with Mu–metal sheets. These are the relevant experiments here.

The best available measurements on the angle for two complete spin flips are the following [100] : 715.87±3.8 deg, that is, the minimal angle is 712.97 deg, while the maximal value is 712.07 deg. As a result, and according to the experimenter's own words, the measures "do not include the expected 720 deg within its simple error limits" (ref. [100], p. 730).

What does this mean? The answer is incontrovertibly clear for all ethically sound scholars: THE CURRENTLY AVAILABLE MEASURES BY RAUCH DO NOT CONFIRM THE PREDICTIONS OF QUANTUM MECHANICS IN THE BEHAVIOR OF THE FUNDAMENTAL ROTATIONAL SYMMETRY. In fact, to confirm orthodox theories, the measures should have been of the type, say, with maximal angle of precession 720.01 deg and minimal angle of precession 718.37 deg, thus including 720 deg. As remarked by the experimenter, the value 720 deg is instead OUT of the simple errors limits. Quantum mechanics is therefore not confirmed by the experiments as they stand now.

It is equally evident to all ethically sound scholars that Rauch's values [100] DO NOT confirm hadronic mechanics either. In fact, such a confirmation can only be claimed after repetition of the experiments in a substantial number of different realizations (see below).

In short, the experiments by H. Rauch and his team on the rotational symmetry of neutrons under strong and electromagnetic interactions, confirm the essentially open character of this fundamental problem of human knowledge. The lack of recovering of the angle of precession predicted by the exact rotational symmetry, confirms the plausibility of the deformation of hadrons with consequential alteration of their magnetic moments.

The need for the repetition of the experiments is then evident to all.

The needed tests are well known (see, for instance, ref.s [62,100]). They are as follows:

1) The first tests suggested are given by the repetition of measures [100] according to exactly the same set up as originally done, (two complete spin flips in both branches of the neutron beams), but with an improved accuracy. Apparently, the use

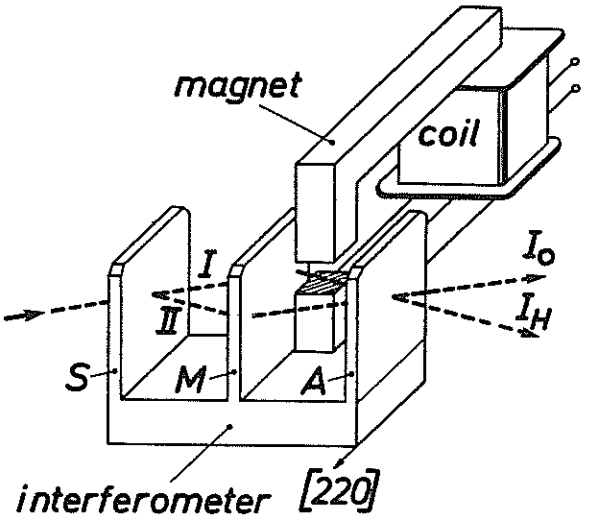


Figure 1.7.1. A schematic view of the neutron interferometers used in the tests [96–100] of the rotational symmetry under short range interactions. A low energy neutron beam originating from a nuclear reactor is subjected to a coherent spitting into two beams via perfect crystal, and then to a coherent recombination. An electromagnet acts on each or both branches of the beam thus inducing a precession in the orientation of spin. Some typical data are the following: beam cross section = 2x1.5 mm^2; crystal characteristic wavelength = 1.83 Å; magnetic induction needed to produce two complete spin flips = 7496 G. The stray fields for electromagnet gaps in air are rather pronounced, thus increasing the errors. The gaps are therefore filled up with Mu–metal sheets. This latter feature renders the experiment of fundamental character because it implies the test of the rotational symmetry under the long range magnetic forces of the electromagnet and the short range, intense fields in the vicinity of nuclei due to penetration of the neutron beams within the Mu–metal sheets. Under these latter conditions, hadronic mechanics predicts a deformation of the charge distribution of the neutron due to the intense nuclear fields. This deformation, in turn, (necessarily) implies an alteration (mutation) of the intrinsic magnetic moments. Still in turn, the alteration of the magnetic moment implies deviations from the angle of spin precessions predicted by the exact rotational symmetry. Explicit calculations conducted by Eder [65] predict about 1% deviations, The measures of the angle of spin precession are done via measures on the so–called intensity and polarization modulations. The experiments have been conducted by Rauch and his associates since 1975 [96–100]. The latest available measures [100] DO NOT contain the angle of the exact rotational symmetry (720 deg) in their simple errors limits. The measures are therefore encouraging in favor of hadronic mechanics, although, and this must be stressed here, they are inconclusive and in need of numerous verifications before reaching any conclusion. The measures, if confirmed by future tests, imply a direct violation of Einstein's special relativity. In fact, as reviewed in Section 1.4, the violation of the rotational symmetry implies the breakdown of the foundations of the special relativity, such as the alteration of the speed of light under a Lorentz

transformation. It should be stressed that the experiments reviewed here are not specialized to maximize the deformation–mutation effects. Rauch's tests can therefore be repeated to maximize the possible deformation–mutations. As final comments it should be indicated that neutron interferometric measures are known to be among the most accurate measurements throughout the entire experimental physics. This accuracy is mostly dependent on the low energy of the beam, which is therefore important for the experimental resolution of the possible mutation of the magnetic moment of hadrons. The tests of other predictions of hadronic mechanics demand sufficiently higher energies. This is the case of the tests for Pauli's exclusion principle (see later on).

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of recent experimental advances could permit a decrease of the error by a facotr of 1/10. An improved accuracy of this type would be per se sufficient to resolve the issue.

2) The tests should be repeated with an increasing number of spin flips, say, 2, 4, 6, 8, 10, and more (apparently, currently technology could permit up to 50 spin flips). The comparative analysis of the various individual tests would then permit the elimination of possible statistical fluctuations, the identification of the linear or nonlinear behaviour of possible deviations with the precession angle, and other important aspects.

3) Each of the tests 2) should be finally repeated with a progressive increase of the width of matter penetrated by the neutron beam, say, 0.5 cm, 1 cm, 1.5 cm, etc. This latter specification is evidently important to maximize the physical conditions needed for a possible mutation of the magnetic moments. Progressive tests of the type suggested here would also provide additional information on the possible nonlinear behaviour of the mutation with the width of matter penetrated by the beam, and others.

A number of additional tests have also been suggested in the literature, such as repeat experiments 1), 2) and 3) with the electromagnet in only one branch of the neutron beam, with particles other than neutrons, etc.

The scientific implications of Rauch's experiments.

The scientific importance of Rauch's experiments is such to dwarf ALL other experiments in particle physics, without exceptions. It is of the essence that the fellow taxpayer understand the ethical implications originating from the suppression or even delays in the repetition of Rauch's experiment.

The rotational symmetry is the true, ultimate pillar of the entirety of our current description of the microscopic world. The central role of the rotational symmetry for the special relativity has been stressed beginning from Section 1.4. But this is only part of it. Each and every aspect of quantum mechanics is either directly or indirectly dependent on the rotational symmetry.

It is important that the taxpayer understands the lack of reciprocity of this occurrence. Take for example the discrete symmetries: space and time reflections. For the case of particles with spin, these symmetries are dependent explicitly on the rotational symmetry. Thus, if the rotational symmetry is broken, the space and time reflection symmetries must be broken too. The opposite situation, however, is not necessarily true, in the sense that the discrete symmetries can be broken, but the rotational symmetry can remain exact (or at least this is the thesis currently preferred in leading U.S. institutions). The reasons are identified in the additional components of discrete symmetries, besides those depending on the rotational symmetry.

A similar situation occurs for virtually all other aspects of nuclear physics, particle physics, statistical mechanics (including the controlled fusion!) and other branches of physics.

It is a truism to say that, if future experiments will confirm the breaking of the rotational symmetry, the virtual entirety of our contemporary description of the microcosm must be suitably generalized.

The low cost of Rauch's experiments on the rotational symmetry when compared to the costs of current particle experiments of lesser relevance.

The neutron interferometric measures on the rotational symmetry [100] can be repeated with expenses ranging from $ 50,000 to $ 100,000. This expenditure includes reactor time, salary for two experimentalists, and all other direct and indirect costs.

This cost takes into account the fact that all basic equipments are already available, such as the reactor to produce the neutron beam and the perfect crystal , while the measures can be reached within a period of time of the order of two months.

To understand these numbers, the fellow taxpayer should compare them with costs of other experiments in particle physics. These latter experiments typically involve teams of several dozen (or even hundreds) of experimentalists, working for extended periods of time (of the order of one year or more). The tests are done in particle accelerators, resulting in costs of the order of millions of dollars and more.

Whenever we shall enter into the problem of ethics and scientific accountability in the U.S. physics, the fellow taxpayer

must remember this comparatively low cost of the test of the rotational symmetry, jointly with their comparatively more fundamental relevance.

In fact, owing to their low costs, financial reasons cannot be claimed in a credible way as the reasons for the lack of repetition of the experiments.

Once the taxpayer sees that, then he/she will be able to see beyond reasonable doubts that the lack of repetition of the experiments is due to mumbo–jumbo academic politics and maneuvring by vested interests.

The impossibility to repeat Rauch's experiments on the rotational symmetry since 1978.

As indicated earlier, the first measures by Rauch's team were conducted in 1975 [96] and then repeated in subsequent years. The last tests occurred in 1978 [99]. In fact, the best available measures [100] are a mere re–elaboration of the measures of 1978 due to the improvements of physical constants and other advances occurred in the meantime.

Since 1978, it has been impossible to repeat the measures, despite numerous attempts in two continents, as we shall review in detail throughout the rest of this presentation.

As a preview, the impossibilities included:

- the prohibition by the Laue–Langevin Laboratory in Grenoble, France, to repeat the measures in conjunction with an international conference in the field;
- the lack of interest and cooperation by the Massachusetts Institute of Technology despite its availability of all basic equipments;
- the rejection by E. T. Ritter, Director of the D.O.E. Division of Nuclear Physics, to fund the repetition of the measures via an Austria–France–U.S.A. collaboration;

and numerous other aspects the taxpayers of the U.S.A. and abroad MUST know.

These difficulties have been one of the ultimate motivations for writing IL GRANDE GRIDO. As evident, if the experiments could have been routinely done, the scientific issues underlying this book would have been resolved one way or the other, by therefore pre–emptying the scientific motivations of this presentation.

The tests of Pauli's exclusion principle under strong interactions.

Recall that the magnetic moments could be altered by short range interactions without affecting the value of the spin [65], resulting in measures [100]. This situation, however, is expected to be only the first stage of a much deeper physical context.

In fact, under sufficiently higher energies and/or collisions, the value of the spin itself is expected to mutate, in which case the mutation of the magnetic moment would be a mere consequence.

The test of the possible mutation of spin can be done via the experimental verification of the validity or invalidity of Pauli's exclusion principle under strong interactions. This is the test submitted in memoir [14] which originated most of the theoretical studies reported in this book.

Quite encouragingly, the test of Pauli's principle is well within current technical feasibility. Also, it is of quite limited cost and of high accuracy inasmuch as it can also be done via neutron interferometers.

To avoid misrepresentations of this presentation, it should be indicated that no direct experimental measure of Pauli's exclusion principle exist to this day, and the information is strictly inconclusive.

Nevertheless, it is encouraging to see that the test has already been studied by experimentalists and considered as feasible via the scattering of neutrons on the nuclei of the tritium.

The main physical ideas are again simple and understandable to all. The core of the tritium is made up of two neutrons in the s–state with antiparallel spin, thus filling up all possible states. According to Pauli's principle, no additional neutron can therefore penetrate within such a core when in the s–wave state, contrary to our intuitions and expectations.

The experiment consists in having a beam of s–wave neutrons collide with the tritium. Pauli's principle can be tested via interferometric measures of the so–called scattering length which is one measure of the mutual penetration of wave–packets.

For sufficiently low energies of the incident beam, the validity of Pauli's principle is unquestionable. In fact, the preservation of the value ½ of the spin of the neutron for low energy nuclear phenomena is out of the question.

With sufficiently high energies, instead, the situation is expected to be different. Spin is nothing but an intrinsic angular momentum. As such, it is expected to alter in value (or fluctuate in Eder's words [65]) under sufficiently intense collisions. If this is indeed the case, neutrons with a value of the spin $\frac{1}{2}+\epsilon$, where ϵ is near zero, are not exact Fermions, and Pauli's exclusion principle is not expected to be exactly valid, as suggested in ref. [14]. Sufficiently small deviations are then conceivable. These deviations result in a proportionately small penetration of the incident neutron within the tritrium core,

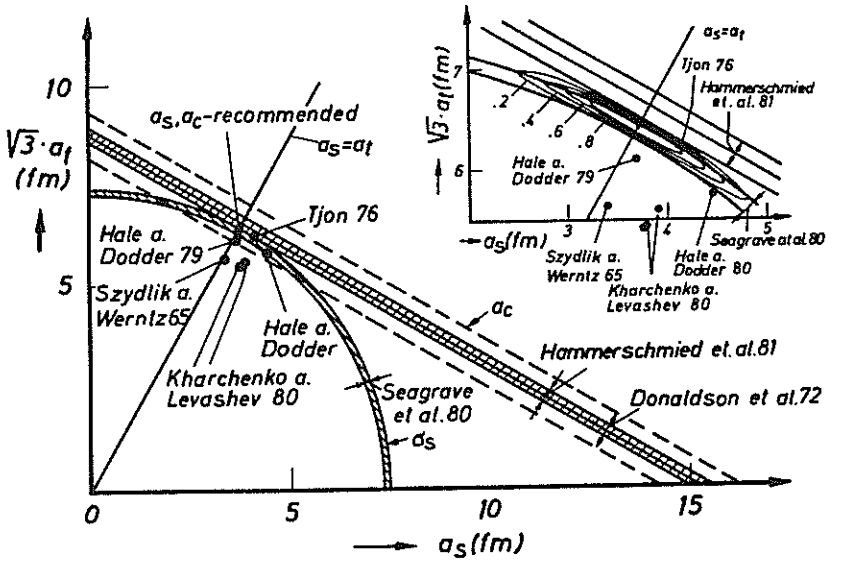


Figure 1.7.2. A reporduction of diagram 3, p. 731 of paper [100] on the experimental elaboration of the test of Pauli's exclusion principle under nuclear interactions done via the neutron–tritium scattering. The diagram summarizes most of the available experimental data (represented via lines) and includes also some theoretical estimate (represented via points). The value of scattering length a_c recommended in paper [100] as plausible under currently available data is indicated in the top–left part of the figure. Of course, there is no experimental evidence at this time favoring deviations from Pauli's principle. Nevertheless, the experimental resolution is well within current technical capabilities and simply requires the repetition of the experiment with neutron beams of sufficiently higher energy (see the test). The most encouraging aspect is the plausibility of the violation. This can be seen in a number of ways. In fact, the wave–packets of the incident neutrons become closer and closer to those of the tritium core with the repetition of the tests (see the insert of the figure). The possibility of overlapping, and thus violation of Pauli's principle, cannot be excluded with further tests specifically conceived for the purpose. The fellow taxpayer, however, can reach a true assessment of the situation via the fact that **all experimental and theoretical data presented in this diagram have been elaborated via the assumption of the exact validity of Pauli's exclusion principle.** Under these conditions, the results simply cannot test the (tacit) assumption in a true way. ALL data presented in the diagram above should therefore be re–elaborated via the use of hadronic mechanics and the assumption of a (small) violation of the principle. The two results should then be compared, and the emerging context be resolved by subsequent tests. Particularly for higher energies of the incident neutron beam, the elaboration of the data of the insert under the assumption of the validity of Pauli's principle has exactly the same credibility than that under the assumption of the violation (which could ALREADY show overlapping). Above all, the fellow taxpayer should keep in mind the religious–type–dogma underlying all this: the absolute constancy of the intrinsic angular

momentum of the incident neutron, irrespective of the impact and collisions with the tritium core. How can physicists believe in such absolute physical conditions and jointly expect no critical examination of their scientific ethics?

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which is prohibited by Pauli's principle, as indicated earlier. This possible penetration can be measured via the scattering length.

In the experimenter's own words (ref. [100], p. 731):

> *"The extracted singlet and triplet scattering lengths (a_s = a_t & 3.70 fm) define a repulsive hard core radius which determines an overlapping region given by the radial mass distribution of the neutrons of the tritium nucleus outside the hard core radius. Within this region a partial violation of Pauli's principle can be assumed."*

Again, these comments are inconclusive. The important point here is the technical feasibility of the experiment as well as the plausibility of deviations from Pauli's principle.

It should be indicated here for clarity that we do not possess at this moment the theoretical prediction of the threshold of energy which could initiate deviations from Pauli's principle. This is due to the fact that we have no direct experimental knowledge of the underlying forces, the contact/non–local/non–Hamiltonian ones. We have some knowledge for their representation (via isotopies and genotopies of conventional formalisms), but the "strength" of the forces for given physical conditions are unknown.

In different terms, the state of our knowledge regarding the contact/non–local/non–Hamiltonian forces is similar to that at the time of the discovery of the law $F = qq' / r^2$ by Charles Augustin de Coulomb in 1785. At that time, there was some idea regarding the physical law. However, quantitative predictions could be made only upon achieving an experimental knowledge of the value of the charges q and q'.

The situation regarding the contact/non–local/non–Hamiltonian forces due to mutual penetration of wave–packets is quite similar to the preceding one. In fact, we need at least some preliminary measures on the strength of the forces in at least one physical situation. Once this is achieved, then we are in a position to make quantitative predictions in different physical situations.

The tests of the mutation of magnetic moments and/or of spin could provide exactly this missing link. In fact, once achieved, the experimental knowledge could be extrapolated via the techniques of hadronic mechanics to other physical conditions, by therefore achieving the capability of quantitative prediction that is typical of physical theories.

The needed tests are evident (see, for instance, ref.s [14, 62, 100]). The interferometric measures of the scattering length of neutrons on tritium should be repeated with a progressive increase of the energy up to the highest possible value achievable with contemporary technology. A comparative analysis of the individual tests could then provide the currently missing link: the possible treshold of deviations from Pauli's principle. Jointly, the accuracy should be improved, as routinely done in each test.

Finally, and most importantly, the available experimental data should be re–elaborated under the assumption of a sufficiently small violation of Pauli's exclusion principle. The results should then be compared with those based on the exact validity of the principle. The need for the alternative elaborations is evident. In fact, it may well be that the experimental results of Figure 1.7.7 (lack of overlapping of the wave–packets of the incident neutron beam with the neutron core) are a mere consequence of the theoretical assumption in the data elaboration (exact validity of Pauli's principle).

The impossibility of conducting the test on Pauli's principle until now.

I published memoir [14] in the hope of stimulating a constructive scientific dialogue on this fundamental open problem of human knowledge. After its overwhelming experimental verification in the atomic structure, Pauli's principle was merely "assumed" as valid in the nuclear structure without any, even minimal, process of critical examination.

But, physics cannot be done on the basis of experimentally unverified assumptions. Owing to its fundamental character, the problem of validity or invalidity of Pauli's principle in the nuclear and hadronic structure must be subjected to suitably exhaustive, theoretical studies and experimental resolutions.

Despite this scientifically democratic but inquisitive attitude of memoir [14], the reaction of the community was generally that of complete ignorance, if not of hysterical opposition, except on rare occasions.

As an example, D. D. D., an internationally renown scientist, following the appearance of memoir [14] wrote me to terminate the scientific association we had at that time on grounds that there was no need to test Pauli's principle.

I accepted the termination of our association with pleasure, but I accused him of scientific corruption.

Memoir [14] did not recommend to verify the violation of Pauli's principle. Instead, it recommended the establishing of physical knowledge via experiments, irrespective of whether in favor or against Pauli's exclusion principle. As a result, the

experimental proposal, when realized, could well CONFIRM the validity of Pauli's principle.

Any person opposing such experimental verification "must" be accused of scientific corruption. Otherwise, why should that person oppose experiments that may eventually confirm his/her views?

Numerous correspondence with experimental nuclear physicists in the U.S.A. and abroad indicated quite clearly that the possibility of testing Pauli's principle under strong interactions along the lines considered here were absolutely null. This correspondence has been lost with the passing of time (and my too numerous changes of office . . .). Lacking the documentation, I shall abstein from reporting it in this book. The illustration will be essentially restricted to a documented report of the reaction by the Massachusetts Institute of Technology (Section 2.2).

Mutatis mutanda, the substance of the matter is that, except the experimental consideration in the European paper [100], it has been impossible to reach even the "consideration" of the test of Pauli's principle under strong interactions in the U.S. physics. The possibility of the actual conduction of the experiment prior to the appearance of this book is absolutely null.

This situation should be compared with the ultimate essence of physics, that of conducting, repeating, and then doing again all necessary experiments to establish and then refine our physical knowledge. For instance, the magnetic moment of the neutron has been measured, remeasured, and then measured again countless times since the discovery of the particle. This is the reason why any physicist opposing the experimental test of Pauli's principle must be accused of scientific corruption.

But, fellow taxpayer, nuclear laboratories in the U.S.A. use hundreds of millions of our dollars in research projects crucially dependent on the exact validity of Pauli's exclusion principle under strong interactions, that is, on a religious dogma currently deprived of a direct experimental support. If the (generally small) deviations theoretically predicted in ref. [14] and experimentally indicated as plausible in ref. [100], are true, a significant portion of our money goes down the drain (that is, in the pockets of academic barons without true scientific output).

Again, as it was the case for governmental funding of manifest inconsistencies in Einstein's gravitation, statistical irreversibility, and quark conjectures, absolutely no self–correcting mechanism by the governmental–academic complex is conceivable without your intervention, fellow taxpayer.

Of course, academic barons have the right to voice their opinions on the lack of needs for the experimental verification of Pauli's exclusion principle under strong interactions. But this,

if and only if they have no scientific accountability toward the taxpayer, that is, if and only if they use their personal money or money belonging to their colleges. Under no circumstances the voicing of such antiscientific opinions should be justified and, most importantly, should be permitted to continue under governmental support.

Experimental data on the mean life of unstable hadrons at different energies conducted in Denmark, Mexico, U.S.A. and other countries.

The experiments immediately following those on the rotational symmetry in the scale of absolute scientific values, are the measures of the mean life of unstable hadrons in flight at different energies which test the Lorentz symmetry (see Figure 1.7.3).

Recall that an unstable hadron, such as a charged pion or kaon, when moving within the high vacuum of a particle accelerator, must verify the special relativity, in the sense that its center–of–mass trajectories must conform to the physical laws of the special relativity, including the increase of mass with speed, the Lorentz contraction, etc.

Pions and kaons, however, are composed of particles with wave–packets in conditions of deep mutual penetration and overlapping, thus resulting into an internal non–local structure with consequential departures from the special relativity.

The problem considered in the preceding sections was that of ascertaining how deviations from the special relativity in the interior dynamics could manifest themselves to the outside world, while the center–of–mass trajectory is strictly conformed to the special relativity.

An answer known at this time is the behaviour of the mean life as a function of the energy of the particle. The reasons are evident. The mean life is directly dependent on the internal dynamics. If such a dynamics violates the special relativity, the behaviour of the mean life must deviate from the predictions of the special relativity.

Very intriguingly, ALL available re–elaborations of the experimental data on the behaviour of the mean life with energy show deviations from Einstein's ideas. The available studies are quite numerous, all concurring toward the same conclusion, and increasing in time.

Here I limit myself to recall the studies by the Danish physicist H. B. Nielson and his associates at the Niels Bohr Institute in Copenhagen [35]. These authors have essentially re–elaborated available experimental data on the charged pions and kaons. The data shows a clear variance in the structure of the space–time underlying the special relativity, the Minkowski space. In fact, the structure $X'mX$ indicated in Section 1.4, is shifted to the new structure $X'gX$, where $g = \text{diag}(1 + 1/3\ a,$

$1 + 1/3\ a,\ 1 + 1/3\ a,\ -1-a)$ and $a = (-3.79 \pm 1.37) \times 10^{-3}$ for charged pions, while $a = (0.61 \pm 0.17) \times 10^{-3}$ for charged kaons [35].

The direct universality of the Lie–isotopic relativity [32] can now be put to work. In fact, whether the parameter a is constant or a local function, the Lorentz–isotopic relativity applies, yielding the generalizations of the Lorentz transformations leaving invariant the quantity X'gX.

Note the differences in values and signs of the Lorentz breaking parameter a in the transition from pions to kaons. This is also fully in line with hadronic mechanics and the Lorentz–isotopic relativity. In fact, the two particles are expected to have basically different structures (in the sense of having different numbers of elementary constituents). In turn, these structural differences result in different Minkowski–isotopic spaces, those with different values of g.

The independent studies conducted by the Mexican physicists R. Huerta–Quintanilla and J. L. Lucio M. [37] have confirmed the above findings, by reaching the value $a = (3.6 \pm 5.2) \times 10^{-3}$ for the case of muons.

Further independent studies have been conducted by the U.S. physicists S. H. Aronson, G. J. Bock, Hai–Yang Cheng and E. Fishback [36] on the behaviour with varying energy of all essential parameters of the neutral kaon including most importantly the mean life. As stated by the authors in the abstract of article [36] "The data suggest that these parameters may have an anomalous energy dependence", where in plain language the term "anomalous" means violation of Einstein's idea.

As a matter of fact, the violation indicated as possible by this latter study is much deeper than that of the preceding studies [35,36], because it predicts an energy–dependence of the mean life of the neutral kaon even for observers at rest with the particle. According to the special relativity, no such a dependence is possible for the rest frame.

The needed experiments are well known and definitely within current technical capabilities. They consist in the measuring of the mean life on unstable hadrons (at least pions and kaons) at a number of values of increasing energies. The comparison of the measures with the predictions of the special relativity will resolve the issue one way or another, at least up to the attained energies (see Section 1.6 on the possible breakdown of the special relativity at the speed of light in vacuum).

The experiments should also be repeated for leptons with the understanding that its composite character is unclear at this writing. In fact, the muons could be excited states of the electrons (as suggested by the Italian physicist Caldirola [58] and others), in which case no anomalous behaviour of the mean life is conceivable, trivially, because of the lack of nonlocal internal effects. Even if muons are indeed composite, they are not

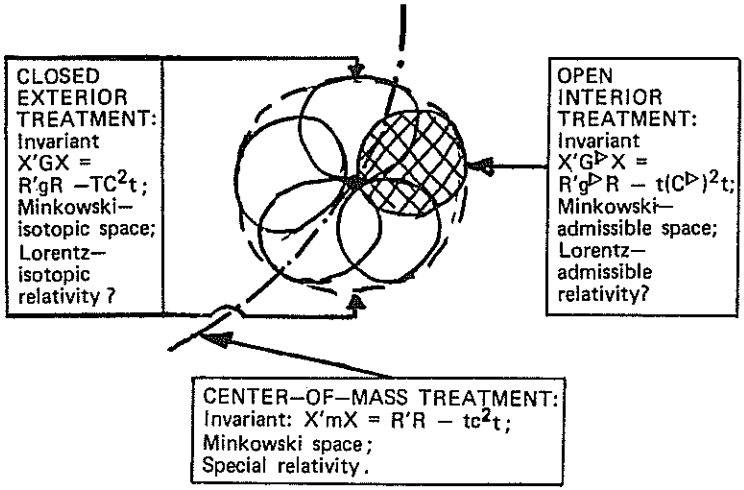


Figure 1.7.3. A schematic view of the currently available experimental information on the apparent validity in the interior of hadrons of suitable generalizations of Einstein's ideas, while the same ideas remain valid for the center–of–mass trajectories in vacuum. The information is based on the behaviour of the mean life of unstable hadrons at different energies [35–37]. The results are apparently in favor of the hadronic generalization of quantum mechanics due to internal, nonlocal/non–Hamiltonian effects originating from deep mutual penetration and overlapping of the constituents' wave–packets. This situation is depicted in the figure above by associating the conventional Minkowski space of the special relativity with the center–of–mass trajectory in vacuum, and the Minkowski–isotopic space [32] with the interior dynamics as suggested from experimental studies [35–37]. The contributions by hadronic mechanics to these latter studies are the following: (1) reconciliation of a generalized interior relativity with the conventional center–of–mass one [31,55]; (2) methods for the explicit construction of the generalized Lorentz transformations leaving invariant the Minkowski–isotopic separation (this is achieved via the methods of ref. [8, 10, 18, 19, 32, 33]); and (3) possibility of achieving a unified formulation of all seemingly different results of ref.s [35–37] as well as of others. But perhaps the most relevant contribution of hadronic mechanics is the possibility of regaining unity of physical and mathematical thought which is inclusive not only of the interior strong problem, but also of other fundamental aspects, such as the irreversibility of the real world, the noncanonical character of classical mechanics, the lack of local Lorentz character of the interior gravitational problem, etc. All these aspects can be unified via the Lie–admissible generalization of quantum mechanics for the open–nonconservative interior problem, with its Lie–isotopic counterpart for the complementary closed–conservative treatment. In fact, the unification is permitted by the abandoning of local/Hamiltonian/Lie formulations in favor of structurally more general formulations. In turn, the physical origin of the generalizations is given precisely by the nonlocal/non–Hamiltonian effects originating from deep overlapping of the wave–packets under strong interactions. The deviations from Einstein's ideas reported in ref.s [35–37] are precisely a manifestation of these effects. The historical roots of the occurrence are intriguing indeed. The

founding fathers of the theory of strong interactions indicated quite clearly the intrinsically nonlocal character of the interactions due to the deep penetration of the wave–packets (which is generally absent under electromagnetic interactions). This legacy has been studiously ignored by vested interests for decades (see the episode of my talk at the Clausthal Conference at the end of Section 1.6). The studies reported in this chapter have taken the legacy seriously and identified preliminary (not necessarily unique), mathematical means for its quantitative treatment. Everything else is a consequence of that, including the Lie–admissible/Lie–isotopic generalizations of quantum mechanics, the identification of the physical and mathematical roots of anomalies [35–37], and the possible regaining of the unity of physical and mathematical thought. The most fascinating aspect is that these anomalies are without any possibility of achieving a credible reconciliation with the special relativity (as it was possible for the case of parity violation). To illustrate this point beyond a reasonable doubt, it is sufficient to note that **all anomalies [35–37] imply the abandonment of the speed of light in vacuum as the limiting speed of the universe, by therefore resulting to be a confirmation of prediction [31] and of the basic assumptions of the generalized relativity submitted in ref. [32, 33]** (see Section 1.5). This is an inevitable consequence of the alteration of the time component of the Minkowski metric which, as well known, characterizes precisely the maximal speed of causal signals. Thus, the experiments under consideration leave no room for manipulatory maneuvring due to academic greed. This ultimate resolutory character of the experiments is, of course, well known to vested interests and constitutes the most plausible reason for the impossibility of their repetition until now.

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strongly interacting. This implies smaller conditions of internal mutation due to wave–overlappings [14] and, therefore, a lesser anomalous behaviour of the mean life. Despite these considerations, the analysis of ref. [36] (on the anomalous behaviour of the mean life of the muons) should be kept in mind.

Preliminary theoretical predictions of deviations have already appeared in the literature. For instance, the Canadian physicist D. Y. Kim [101] predicts a deviation of about 14.3% from the prediction of the special relativity for muons at 400 GeV. The results of the analysis appear to be readily extendable to hadrons. [As an important note, ref. [101] intended to stress the view that the experimentally established violations of discrete symmetries are due to the violation of the special relativity because they all originate from the nonlocality of the interior structure.]

The most important aspect is that **the experiments on the mean life of unstable particles are the most direct possible tests of the Lorentz symmetry for the interior problem, without questionable theoretical elaboration of the data.** In fact, the value of the energy produced by the particle accelerators can be identified in an incontrovertible way. The measures then reduce to those of mean life of the particles, from their production to their spontaneous decays. As one can see, no major theoretical elaboration is used, except those of routine experimental character (such as

for the errors).

To understand the importance of this occurrence, the fellow taxpayer should compare it with that of other experiments in which the law to be tested is often used as a fundamental assumption in the data elaboration (see the case of Pauli's principle of Figure 1.7.2!). The experiments, here considered, therefore leave no room for attempts by vested interests to re–elaborate the data in such a way to reach compatibility with old doctrines.

The impossibilities to repeat experimental measures on the behaviour of the mean life of unstable hadrons at different energies.

All experimental studies [35–37] deal with "re–elaborations" of experimental data intended for different purposes. Differently stated, the experiments were authorized for objectives full aligned with vested interests. At the time of such authorization, it was apparently unknown that the same measures contained information on the apparent invalidation of Einstein's ideas. If this possibility had transpared even minimally, the chances of running the experiments would have been so minute to be ignorable.

This situation is established beyond a reasonable doubt by the fact that ALL APPEALS TO U.S. (AND FOREIGN) LABORATORIES TO REPEAT THE MEASURES OF THE MEAN LIFE OF UNSTABLE HADRONS AT DIFFERENT ENERGIES FILED BY INDEPENDENT SCHOLARS INCLUDING MYSELF, HAVE RESULTED TO BE COMPLETELY USELESS. (See Section 2.3 for details). Incontrovertible evidence proves that, despite these appeals, no experiment on the direct measure under consideration here is currently under way at U.S. National (as well as foreign) laboratories to this writing (April 16, 1984).

Again, the impossibility to repeat these truly fundamental tests has been another pivoting reason for writing IL GRANDE GRIDO. In fact, the experimental resolution of the issues would have voided the very motivation for writing this book.

The experimental tests of the reversible or irreversible character of nuclear interactions.

Additional, fundamental, experiments that must be brought to the attention of the taxpayer are those on the reversible or irreversible character of nuclear interactions.

Recall the predictions of hadronic mechanics indicated in Section 1.6, that: (A) the center–of–mass trajectories of strong systems are generally reversible; (B) the internal open processes are strictly irreversible; and (C) the complementary exterior–closed treatment can restore the time reflection symmetry under isotopy (by incorporating all time–asymmetric terms in the iso-

topic operator g of the abstract product A*B = AgB). In short, the most reliable way to test the reversible or irreversible character of strong interactions is to ensure the achievement of open/nonconservative conditions due to external strong fields. The reversibility of the closed–exterior treatment can be at best misleading (recall the Earth whose center–of–mass trajectory in the solar system is strictly reversible, while its interior trajectories are strictly irreversible).

Experimental studies of the issue conducted by a number of experimentalists have added further conditions for the achievement or meaningful tests, such as the lack of reliability of the so–called cross–sections. In fact, these quantities are averaged out over all possible states. In this way, their experimental information is reliable for other objectives (e.g., of statistical nature), but not for the time–reflection symmetry.

The same studies have identified that the most effective means to test the time–reflection symmetry in nuclear physics is via direct measures of the so–called polarization of the forward reaction and analyzing power of the backward reaction (see the readable review by the Canadian experimentalist R. J. Slobodrian [102]). If these quantities are equal, the time reflection symmetry is exact "under the conditions considered" (e.g., in the center–of–mass frame); otherwise, it is violated.

Note that time cannot be reversed in experiments. Thus, the tests deal with one given nuclear reaction, and its "time reversed image", that is, the reaction in which the original and final products are interchanged with respect to those of the original one.

An experimental collaboration Québec–Berkeley–Bonn reported in 1980 experimental measures of the difference between the polarization and analyzing power thus indicating the existence of irreversibility in nuclear reactions. Their findings were subsequently printed in 1981 (see ref. [103] and quoted papers).

Most importantly for this presentation, experiments [103] identified the origin of the irreversibility in the spin component of the nuclear force, thus indicating a possible direct connection with measures [100] on rotational–asymmetry (recall that the breaking of the rotational symmetry would imply that of the space and time reflection symmetries, although the opposite is not necessarily true).

As a result, measures [103], if confirmed, would have provided full experimental grounds for the regaining of unity of thought in physics, by identifying the origin of irreversibility in the most elementary layer of nature, and by promoting their unified treatment via suitable generalizations of currently relativities.

Measures [103], however, were not confirmed by independent experiments conducted at Los Alamos [104].

At the writing of this section, the experimental situation

is essentially unsettled either way. Following publication [104], the Los Alamos group has not repeated the experiment any more. Other experimentalists have conducted additional measures and dismissed the existence of irreversibility in nuclear physics. However, these latter measures do not appear to deal directly with polarization and analyzing power. As a result, their true implications for experiments [103] are unknown to me.

The Québec–Berkeley–Bonn experimental group has continued to be quite active in the conduction of new experiments, by confirming quite firmly their original findings (see ref.s [106–109] and quoted papers).

A comprehensive theoretical program had been prepared by the Institute for Basic Research in Cambridge for an in depth investigation of the problem by experts in the field. Regrettably, funding of the project was rejected by both the U.S. National Science Foundation and the U.S. Department of Energy. As a result, all research on the problem has been halted. The comments below are merely indicational.

The most unsettled aspect of all experiments [103–109] is the currently lacking identification of their nonconservative character. In fact, all experiments are intrinsically open because they deal with beams of nucleons on fixed "external" targets. It is evident that, under these conditions, the energy is not conserved, and the reactions are open.

The taxpayer should recall that, once this nonconservative character is identified, the experiments can only identify the "amount" of irreversibility. But the "existence" of the irreversibility is out of the question (e.g., because of the nonunitary character of the time evolution). This point is essentially presented in ref. [59], jointly with other aspects reviewed earlier.

Along the same lines, if measures [103–109] do indeed deal with center–of–mass treatments of nuclear reactions considered as closed and isolated, then the lack of irreversibility should be expected.

Note the need for comprehensive theoretical studies both in favor and against irreversibility, to avoid insidious interpretations of experimental results.

My coming of age as a physicist.

Physics advances by conjectures that slowly acquire the flavor of plausible theories, to become later physical truths when verified experimentally in all needed details.

Until a few years ago, when still a naive physicist to a considerable extent, I thought that academic manipulations could occur in physics only during the first stage, that of presentation of theoretical conjectures. But the experimental profile was still sacred to my naive thinking of that time.

I was wrong.

I later came to realize that academic manipulations do occur also in the experimental sector. At first, I thought that this regrettable human aspect occurred only during the process of consideration of the experiments, but not when the machines are eventually running.

I was wrong.

The more I familiarized with the experimental setting, the more I realized how easy it is to manipulate contemporary particle experiments except rare cases. In fact, the final "experimental numbers" are the results of numerous assumptions. Often, a minimal variation of only one of these assumptions is sufficient to product basically different "numerical" results.

I realized this as soon as I started reading experimental papers. But then, one question called for another. For instance, the deeper I read within the lines, the more I realized that, in general, only part of the underlying assumptions are fully reported in the final publications, while other assumptions are either reported in part or not reported at all.

It was only at that point that my childhood as a researcher terminated and I became an adult physicist. Today, I know that the credibility of "experimental numbers" in particle physics is primarily dependent on the ethical record of the experimenters. The experimental aspects appear to be of strictly secondary relevance.

The more fundamental the experiments are (with therefore deeper political implications), the more dominant is the ethical record of the experimenters over the technical stuff.

The apparent commissioning of the disproof of nuclear irreversibility.

The fellow taxpayer must know certain background facts underlying the conduction and publication of the opposing experimental results on irreversibility by the Québec—Berkeley—Bonn group [103] and by the Los Alamos group [104]. The information is mostly available in the papers themselves for everybody to read.

The case is quite intriguing indeed. Papers [103, 104] report measures of the same quantities of the same nuclear reactions, resulting into irreconciliably different results, one in favor and the other against nuclear irreversibility. As such, one of the two papers must be wrong. There simply is no room for compromise.

It should be noted for fairness that the Québec—Berkeley—Bonn group conducted several measures of both polarization of the forward reaction and analyzing power of the backward reaction in two different reactions, while the Los Alamos group repeated only some measures of polarization in only one re-

action, and ignored the repetition of the remaining measures in the same as well as in the second reaction. Numerous additional differences also exist, but they are too technical for review in this general presentation and (regrettably) must be ignored here.

As one can see, paper [103] was submitted to the leading journal of the A.P.S., Phys. Rev. Letters, on August 8, 1980. The paper was published on December 21, 1981, that is, some 1½ years (or some 70 weeks) later.

This extremely long period of consideration is per se sufficient to justify a suspicious attitude toward the editorial board of the journal. In fact, we are talking about a letter journal that is expected to print important results in a matter of weeks.

To have means of comparison, the taxpayer should know that rebuffal [104] was printed in Phys. Rev. C (rather than in the Letters) in only sixteen weeks; or that experimental paper [110] co—authored by one of the editors of Phys. Rev. Letters, R. K. Adair, was printed in the same volume of ref. [103] in about fifteen weeks.

The suspicious attitude stimulated by the excessively long time of publication of ref. [103] is reinforced by a chain of elements the fellow taxpayer has the right to know for whatever their value.

The first idea that comes to mind when facing delays in publication of important results, is that, perhaps, major refinements occurred during the editorial consideration. This possibility is disproved by evidence for paper [103]. In fact, all the papers published by the authors prior to the appearance of ref. [103] or during its submission (see, for instance, ref.s [102, 106]) indicate quite clearly that all the essential results have remained unchanged during the long consideration process of paper [103].

But then, why did the A.P.S. delay a manifestly important paper for such a long period of time without any meaningful improvement occurring in the meantime?

My suspicion was reinforced by the reading of the paper and by the identification of its authors. In fact, one of the authors, H. E. Conzett, is a member of a U.S. National Laboratory, the Lawrence Berkeley Laboratory. I therefore thought that, perhaps, Conzett was a junior member there. To ascertain that, I did some research. It turned out that he was a senior member. I then did further research, by ascertaining that it was common practice by the journals of the A.P.S. to publish experimental papers released by senior members of U.S. national laboratories often without any refereeing at all.

I have no elements to know if and when this practice was halted. But the caliber, ethical record, credibility, and associations of the authors of paper [103] increased my suspicion.

In summary, the following facts are incontrovertible: 1) the A.P.S. kept letter [103] for about seventy weeks; 2) rebuffal [104] was published in sexteen weeks; 3) countermeasures [104] were not running at the time of the submission of paper [103] to Phys. Rev. Letters; 4) paper [103] was published only AFTER contrary measures [104] were available and duly quoted in the paper (see the explicit statement to this effect in page 1806); and 5) immediately after the appearance of rebuffal [104], the official position of the "establishment" in nuclear physics was that nuclear irreversibility had been "disproved" by measures [104] and did not exist!

All these facts created the rumor (I have heard in two continents) that rebuffal [104] had been "commissioned" by vested academic—financial—ethnic interests in the U.S. physics.

Whether this rumor is true or false is immaterial here. The important point is that the A.P.S., by permitting facts 1), 2), 3), 4) and 5) above, has rendered the rumor simply unavoidable.

To my knowledge, this book constitutes the first time the rumor appears in print. Besides the evident need to shed scientific light on the case, the objective of this presentation is to alert the U.S. taxpayer of the occurrence, so that all necessary or otherwise needed actions will be undertaken to prevent its repetition in the future. There is no doubt that the handling of paper [103] has damaged the credibility and ethical standards of the A.P.S. throughout the world.

According to all editorial practices, the Physical Rev. Letter should have: printed immediately paper [103] WITHOUT any reference to opposing data (which at the time of the submission had yet to start!!!), then follow with the publication of measures [104] as soon as available. To put it differently, the function of any journal is that of reporting all relevant results, without any editorial partisanship. Thus, the original measures [103] had exactly the same rights to be printed quickly as the opposing measures [104]. No more, no less. The long delay in the publication of measures [103], compared to the rapidity of publication of rebuffal [104], renders the suspicion of partisanship at the journals of the A.P.S. simply unavoidable. At any rate, a subsequent paper by the Québec—Berkeley—Bonn group confirming the original measures was rejected by Phys. Rev. C, although, it was routinely published by a European journal [105].

The rumors above are quite credible for anyone with a minimum of inside knowledge of the structure, organization and operation of the A.P.S. In fact, as publicly recognized, important papers must pass the approval of leading physicists at leading U.S. Institutions "in good standing with the A.P.S." (see Section 2.4 and related documentation). Translated in plain language, this means that paper [103] had been passed to representatives of the vested interests currently in control.

The halting of its publication for 1½ years was then a quite natural consequence.

Whatever the academic baron (tries to) say in his/her defense, facts persist: the rebuffal [104] was initiated considerably AFTER the submission of paper [103] which was permitted to appear in print only FOLLOWING the NEGATIVE results of the new measures.

But we are still at the beginning of the case. During the conduction of my own investigation of the case out of shear curiosity, I later discovered that E.E.E., a leading representative of vested interests opposing nuclear irreversibility, had left his campus and spent a considerable amount of time at Los Alamos during the running of measures [104]. This fact alone drove my hair into a state of extreme electrostatic stretch. E.E.E. is not an experimentalist (and, indeed, he is not one of the authors of paper [104]). Yet, he has a record of vested interests against nuclear irreversibility on all counts (academic, financial and ethnic). What was he doing at Los Alamos at that time? Was he there on other business, or to supervise measures [104]? Did the experimenters there have meetings with E.E.E.? And if so, what was the impact of E.E.E. in the final results? Also, who paid E.E.E.'s trip there, his college, Los Alamos, or his own government contract? Was he acting alone or was he representing other members of his circle of interests? The number of questions that crossed my mind, all unanswered, were endless.

One thing is sure: the presence of E.E.E. at Los Alamos at the time of measures [104] damaged the credibility of the experimenters.

But, we are still at the beginning of the case. Everything reported so far occurred prior to my direct, personal, contacts with members of both measures [103, 104]. The year 1981 was that of the founding of the I.B.R. (see the appendices). Our institute was interested in both measures. As I.B.R. president, I therefore issued invitations to both groups to deliver joint talks at one of our meetings.

The Québec—Berkeley—Bonn group was quite cooperative, by permitting my visual inspection of their equipment in Québec (a large van der Graph accelerator); by participating in our meetings, and being readily available for all criticism.

On the contrary, the Los Alamos group resulted to be quite distant, to use an euphemism. In fact, my sincere invitation for their sending a representative (under full financial support) to deliver a talk jointly with the opposing group, was rejected (actually it was ignored). At my phone call to ascertain whether the invitation had indeed arrived, I was told that the experimenters were then working on something different and were no longer interested in the problem of nuclear reversibility!

This drove, again, my hair into a stretch. Why were these people uncooperative? How could we possibly reach any genuine

clue of the situation without putting the two experimental teams together and trying to understand their differences with open discussions (rather than papers)? I do not know the answer. But one thing was sure: the lack of participation of the Los Alamos group to our meeting, whether accidental or planned, had the net effect of preventing advances on the problem.

To have a deeper understanding of the situation, the fellow taxpayer must keep in mind the formal position in irreversibility by the "official U.S. physics" immediately following the appearance of paper [104].

At that time, the only direct measures of polarization and analyzing power were those of paper [103] and [104]. How could any physicist claim that any of them is right and the other is wrong? The only ethically sound conclusion was the open nature of the problem (as it remains to this day). Any claim that measures [104] were true and [103] were false was manifestly corrupt. Period!

I then attacked myself to my last hope, that foreign nuclear laboratories had kept independence of thought from their U.S. counterpart. Evidence shattered also this last hope. In fact, a quick scanning of conferences in nuclear physics abroad soon revealed total silence on the issue (a clear sign of dismissal of the very existence of the problem). Verbal communication with colleagues abroad then confirmed the dreadful reality: the official position of foreign laboratories was fully aligned with that in the U.S.A.

Fellow taxpayer, I am confused. I know that the above facts are true. The spider's web behind them is unknown to me. I can only recommend that you conduct a deep, deep, look at the case, if you care for this beautiful Land, for the preservation of its Institutions, and for what they mean to humankind.

Besides that, my best suggestions are those of Section 3.2: to have first the A.P.S. formulate and adopt a CODE OF ETHICS, and then have an appropriate, independent body to strictly enforce it.

Lacking a code of ethics, everything goes!

Of one thing I am sure: the handling of the experimental case of nuclear irreversibility by the journals of the A.P.S. has been questionable. Rushing the repetition of only a few of the measures conducted by the Québec–Berkeley–Bonn group, and then claiming lack of irreversibility has not been dignifying for the A.P.S. The scientific, economic and military implications of irreversibility are simply too big to justify such an insufficient approach to such a fundamental physical problem.

Note that the official position on the lack of nuclear irreversibility will stand forever, and no credibility will be given to research efforts attempting to show the open nature of the problem, . . . unless you, fellow taxpayer, intervene. This reality is well known to all researchers in irreversibility submitting

papers to the journals of the A.P.S. or submitting grant applications to U.S. governmental agencies. I am one of them.

But, above all, one thing should constantly remain in your mind throughout the consideration of each and every aspect related to the case: **the establishing of irreversibility in nuclear reactions would imply the irreconciliable experimental invalidation of Einstein's ideas under strong interactions.** The need for vigilance on ethical issues is then evident to all.

High energy experiments and the nonpotential generalization of the scattering theory by the Italian physicist R. Mignani.

The fifth and last experimental aspect I feel obliged to bring to the attention of the taxpayer is the current situation in conventional high energy scattering experiments, those fully aligned with vested interests, and routinely done at national laboratories.

As an example, take the deep inelastic scattering of leptons on hadrons conducted a few years ago at the Stanford Linear Accelerator Center (SLAC) and then repeated elsewhere.

As it is the case of all experiments without exception, the SLAC experiments produced beautiful physical results. For instance, they provided experimental confirmation of the composite character of hadrons. This physical value is obvious, and it is not an issue here.

The relevant aspect is the objectivity of the "numerical" results. In turn, this objectivity is dependent on the way the data are elaborated.

The first, and most obvious thing is that the special relativity is routinely assumed at the foundation of the theoretical tools elaborating the data. This is perfectly admissible. After all, alternative theoretical tools based on a generalized relativity more suitable for the interior of hadrons are not available to this writing.

The point is that scientific caution should be exercised whenever considering "experimental results" which are directly dependent on the assumed relativity. To be specific, the SLAC experiments under consideration here concluded that hadronic constituents are point—like. The issue is how objective is this "experimental result"? The only possible answer is that caution should be exercised before assuming this result ad litteram. After all, the special relativity is fundamentally dependent on the point—like character of the particles, as stressed throughout this presentation. As a consequence, it is at best unclear whether the experimental result (point—like constituents) is a true experimental information, or it is a mere consequence of the theoretical assumption. One thing is sure: the experimental detection of extended constituents within hadrons would have been incom-

patible with the underlying special relativity.

Most generally, currently available experiments in hadron physics cannot be interpreted as providing "evidence" of the validity of Einstein's special relativity. Such a position has value only for academic politics. The reasons are incontrovertible: the special relativity is assumed as a central tool in the data elaboration of the experiments. The results, therefore, cannot test the assumptions. The experiments considered can, at best, provide elements of plausibility.

This is a case similar to that of Pauli's exclusion principle encountered earlier in this section (see Figure 1.7.2).

Particularly unreassuring is the current way experimental data are elaborated for hadron–hadron scattering, that via a theory known as "potential scattering theory". The very name of the theory implies the underlying central assumption: that the scattering is of potential/action–at–a–distance type. For electromagnetic interactions, the use of the theory is unquestionable, to my knowledge. However, the use of a potential scattering theory to elaborate strong interactions scattering experiments may well result to be insufficient if not inconsistent for the reasons indicated throughout this book.

The unreassuring aspect is that, if the potential scattering theory is insufficient, the numerical results are, at best, qualitative, and possibly wrong.

The construction of a nonpotential generalization of the potential scattering theory for strongly interacting particles with contact/non–Hamiltonian interactions due to mutual wave–overlappings, has been initiated by the Italian physicist R. Mignani in papers [111–113] as an important part of the hadronic generalization of quantum mechanics (Section 1.6). Even though the studies are predictably at the beginning, they have shown that the existence of a non–Hamiltonian component in the strong interactions implies the alteration of the central tool of the theory, the cross section [113].

The scientific and administrative implications of these studies are potentially far reaching. If Mignani's nonpotential scattering theory is correct, it implies the need to review virtually all high energy experiments on strong interactions whose numerical results have been reached via conventional cross sections.

It is hoped that this presentation has provided sufficient elements to illustrate the plausibility of the nonpotential nature of the strong interactions. The fellow taxpayer should then see the administrative implications for future funding of high energy scattering experiments.

That is my last hope.

U.S. governmental agencies do not see this. In fact, both the National Science Foundation and the Department of Energy rejected research grant applications filed by the I.B.R. to hire (U.S.) personnel for the study of Mignani's nonpotential scatter-

ing theory.

The fact that vested interests in the U.S. physics have benefited by the above rejections is beyond any reasonable doubt. In fact, the rejections have achieved in full the apparently intended or evidently consequential result: halt the research in this sensitive field [NOTE: Mignani's scattering theory is incompatible with Einstein's ideas, being based on suitable generalizations].

The issue pertinent to you, fellow taxpayer, is equally clear: has the decision to halt research on Mignani's scattering theory been in your best interest, that is, in the best possible accountability in the future spending of your money in the sector? The answer is equally clear: NO! There is no doubt that the investments of public funds in the use of the potential scattering theory for the data elaboration of strong interaction experiments is and will remain questionable until the studies rejected by N.S.F. and D.O.E. are conducted and the situation resolved either way.

In summary, there is a realistic possibility that hundred of millions of your money may be spent each year in data elaborations of particle experiments that are potentially inconsistent.

In the hope of minimizing misrepresentations, I want to stress that the rejection of the I.B.R. grant application does not create, per se, any ethical problem. After all, grant applications are routinely rejected every day. The ethical issue is created by the rejection of the I.B.R. applications WITHOUT the research being conducted at other institutions. The uniqueness of the I.B.R. applications, their rejection by governmental agencies, and the lack of conduction of the same research elsewhere, have implied the suppression of the investigations in the field. The ethical issue is created precisely by such an implied suppression of research, and not by the rejection of the I.B.R. applications. After all, if studies on Mignani's scattering theory and the possible insufficiencies of current data elaboration of scattering experiments were currently conducted, say, at Harvard University or at the Fermi National Acceleration Laboratory, the issue under consideration here would be nonexistent.

1.8: THE MATHEMATICAL RESEARCH.

The mathematical structure of physical theories.

In the preceding pages, I have attempted to present a known property, that physical theories constitute mere realizations of abstract mathematical structures. As a consequence, a

true generalization of a given physical theory cannot be attempted, unless one identifies first the underlying generalized mathematical theory.

The mathematical structure of Einstein's ideas is the so–called Lie theory (including its diversification into algebras, groups and geometries). As a consequence, no true generalization of Einstein's ideas is conceivable, unless one identifies first at least a conceivable generalization of Lie theory, including its algebraic, group theoretical and geometrical formulations.

Viceversa, mathematical studies on possible generalizations of Lie theory are manifestly important, not only in pure mathematics, but also in theoretical physics. In fact, once a generalization of Lie theory has been identified in the mathematical literature, the construction of the corresponding generalization of Einstein's ideas is only a matter of time.

Lack of sufficient generality of the contemporary mathematical formulation of Lie's theory.

As soon as I was exposed to Lie theory during my graduate studies in theoretical physics, I noted the lack of its sufficiently general formulation. This occurrence is at the basis of the generalized relativities presented in this book and, as such, it deserves a few comments.

Very loosely speaking, Lie theory can be constructed via the so–called enveloping associative algebra [114]. This is an algebra with generic elements A, B, C, . . .and product AB verifying the associative law (AB)C = A(BC). The Lie algebra is characterized by the antisymmetric product attached to AB, the celebrated Lie product AB – BA [74]. Lie groups can be constructed via suitable power series expansions in the associative envelope (the so–called exponentiation) or other means [74]. The notion of the carrier space and field in which the theory is realized, and additional data, permit the identification of the underlying geometry (such as, the symplectic geometry [17]).

Physical applications, for instance, in quantum mechanics occur when interpreting the elements A, B, C, . . .as matrices (or operators). The time evolution of a generic physical quantity A is then given by Heisenberg's law idA/dt = AH – HA, where H is the total energy (Hamiltonian)and AH is the ordinary product of matrices (Section 1.6).

The lack of sufficient generality I noted in the late 60's is due to the fact that the product AB – BA is the simplest conceivable Lie product, because the associative envelope with product AB is the simplest possible envelope. In fact, I could identify nonassociative generalizations of the product AB in such a way that the attached antisymmetric product is still Lie. In this way I reached the existence of a more general formulations of Lie theory, that via nonassociative envelopes.

The Lie–admissible generalization of Lie algebras.

The first paper I wrote (jointly with others related to my Ph. D. thesis) was ref. [115] on the so–called Lie–admissible generalization of Lie algebras.

An algebra with generic elements A, B, C, . . .and abstract product, AxB, is called Lie–admissible when the attached product AxB – BxA is Lie. The important point is that the product AxB is not necessarily associative, that is, (AxB)xC ≠ Ax(BxC). The generalized character of the product AxB – BxA over the conventional form AB – BA, is then evident.

At the time of writing paper [115], the words "Lie–admissible algebras" were unknown in the physical literature. An inspection soon revealed that a nonassociative product AxB whose antisymmetric part AxB – BxA is Lie, was also unknown in all mathematical textbooks of the time I could inspect in research libraries. Owing to this situation, I was forced to spend a number of years of research in specialized mathematical libraries in northern Italy. I finally discovered that the algebras I was interested in had been identified by the U.S. mathematician A. A. Albert in 1948 [116] under the name of "Lie–admissible algebras" and thereafter ignored in mathematical circles to a considerable extent, with the sole exception of ref.s [116–117]. I published paper [115] only upon achieving such knowledge on prior contributions.

Some essential mathematical aspects of the Lie–admissible algebras.

By recalling the fundamental role of Lie algebras throughout mathematics, the mathematical possibilities of the Lie–admissible algebras are evident.

A first possibility is that of generalizing the enveloping associative algebras [75]. In fact, the associative product AB is one of the simplest possible particularizations of the nonassociative Lie–admissible product AxB. A second possibility is that of generalizing the Lie algebras themselves [8]. In fact, the Lie product AB – BA itself is one of the simplest possible particularlizations of the nonassociative Lie–admissible product, that is, we can have AxB = AB – BA. Also, Lie algebras are Lie–admissible, although the opposite property is not necessarily true, while the algebraic axioms of the Lie–admissible algebras (here ignored for simplicity) are a bona fide generalization of those of the Lie algebras. Additional possibilities are offered in other branches of mathematics, such as geometry or topology. More recent studies have indicated the possibility of generalizing the remaining aspects of Lie theory (this is the case of the generalization of Lie groups provided by the so–

called Lie–admissible bi–module [86–88]).

Mathematical studies of the Lie–admissible algebras have been conducted by the following scholars. G. M. Benkart, D. J. Britten, H. C. Myung, R. H. Oehmke, S. Okubo, J. M. Osborn, A. A. Sagle, M. L. Tomber and G. P. Wene from the U.S.A.; by Y. Ilamed from Israel; by S. Gonzales and A. Elduque from Spain; and others. A comprehensive list of mathematical studies on Lie–admissible algebras can be found in the three volumes of Tomber's bibliography and index [118].

Predictably, the physical applications of the Lie–admissible algebras follow as close as possible the above mathematical profile. In fact, the first physical application of the Lie–admissible algebras was their use to treat broken unitary symmetry [119], in which case they were used as generalized envelopes. The immediately next application was their use to characterize the time evolution of Newtonian systems [120], in which case they were used as bona fide generalizations of the Lie algebras themselves.

Additional physical applications followed the mathematical ones. For instance, the hypothesis on the generalization of the special relativity for open/nonconservative systems was submitted in monograph [12] only upon achieving a rudimentary identification of the geometry underlying the Lie–admissible algebras, the symplectic–admissible geometry. The same geometry was subsequently studied by another theoretician [50–51] to formulate a generalization of the available interior gravitational theories for the inclusion of the trajectories of the real world, those of non–Hamiltonian type (Section 1.5).

Further physical advances can be reached only when additional studies are conducted at the pure mathematical level, such as in the representation theory (this is particularly the case for the possible identification of the hadronic and quark constituents with electrons).

The mathematical relevance of the studies is so evident that needs no comments here.

The Lie–isotopic theory.

A "bonus" in the study of the Lie–admissible algebras is the identification of an intermediary generalization of Lie theory which, even though still Lie in character, is nontrivial. It is given by the construction of Lie's theory via an envelope with abstract product $A*B$ which is still associative, yet more general than the conventional one AB. This is the case of the product $A*B = AgB$, g = fixed, characterizing the hadronic generalization of quantum mechanics (Section 1.6). The formulation is called "isotopic" in the (Greek) sense of preserving the basic characteristics of the original formulation. In fact, the original product AB is associative, and so remains the product $A*B$. Similarly,

the original product $AB - BA$ is Lie, and so remains the more general product $A*B - B*A$.

The Lie–isotopic theory emerges quite naturally in the study of the nonassociative Lie–admissible algebras. In fact, under certain conditions, the Lie algebras constructed via nonassociative envelopes with products AxB can be reformulated via associative–isotopic envelopes with products $A*B$, while leaving the Lie product unchanged, and I shall write $AxB - BxA = A*B - B*A$. The point is that this reformulation generally does not regain the simplest possible product $AB - BA$, that is, $A*B - B*A \neq AB - BA$. The need to formulate Lie theory via its most general possible associative envelopes, is then consequential.

It is evident that the isotopic generalization of the envelope of Lie's theory implies a corresponding generalization of the entire theory. The mathematical relevance of the generalization is evident, as illustrated in the preceding sections via the explicit construction of the symmetry transformations (invariance group) of a given n–dimensional metric space with metric g (achieved via the isotopic lifting of the orthogonal group in n–dimension, $O(n)$, and trivial unit $I = diag(1,1,1,...,1)$, into the isotope $\hat{O}(n)$ characterized by the generalized unit $\hat{I} = g^{-1}$; the invariance of g then follows because Lie theory leaves invariant the unit, whether in its conventional or in its isotopic form).

The needed mathematical research.

A comprehensive mathematical study on all possible generalizations of Lie theory is recommended here, under the proviso that the theory admits (a) a consistent generalized algebra; (b) a consistent generalization of the Lie transformation groups; and (c) a consistent generalization of the geometries underlying current Lie–Hamiltonian formulations.

The studies should begin with the Lie–isotopic reformulation of the contemporary Lie theory. This study is needed because several properties and theorems of the conventional formulation are not necessarily true for the Lie–isotopic one (for instance, a Lie algebra which is compact or semisimple when expressed via the conventional Lie product, does not remain necessarily compact or semisimple under Lie–isotopic reformulation).

The mathematical studies should then continue with the more general Lie–admissible theory in its various aspects (generalized algebras, groups and geometries), and then pass to conceivable other generalizations not necessarily of Lie–admissible type.

Also, all theories presented in this book are of local–differential (although non–Hamiltonian) type. Studies should also initiate for their nonlocal generalization.

One point should be clear. The depth and diversification of the physical application of Lie theory have been possible because of the availability of comprehensive mathematical studies in the field (often conducted by theoreticians). The need for similar, comprehensive research in the generalizations of Lie theory, is then evident for further physical advances.

Regrettably, ALL research grant applications filed over a three year period by the I.B.R. to U.S. governmental agencies (both civilian and military) on behalf of distinguished, senior mathematicians, have been systematically rejected, often against the recommendation of the referees, as we shall see in Section 2.5 (and in the Documentation of this book).

The doubt still persists in my mind that a relevant (if not determinant) factor in all these rejections was the knowledge that mathematical studies on the generalization of Lie's theory will inevitably imply a generalization of Einstein's theories.

1.9: IL GRANDE GRIDO.

The organizational efforts underlying the studies reported in this book.

Studies on the limitations and possible generalizations of Einstein's theories are definitely not a one man job. The studies presented in this chapter have been the result of a considerable organizational effort to coordinate the research by distinguished mathematicians, theoreticians and experimentalists.

I initiated these efforts back in 1977 with the founding of the Hadronic Journal (whose first issue was published in April 1978). This demanded first the raising of the necessary funds, and then the setting up of an adequate editorial organization. Today, thanks to all authors, editors, editorial advisors and referees, the Hadronic Journal has acquired a record of seven years of regular and successful publication, in the specialization originally planned: mathematical, theoretical and experimental papers on the limitations and possible generalizations of current relativities, mechanics and related mathematical structures. The understanding is that papers along conventional trends not only are welcome, but are often invited.

Once the Hadronic Journal was under way, I passed to the organization of the yearly *Workshops on Lie–admissible Formulations.* The first meeting was held at Harvard's Department of Mathematics in early August 1978 with three participants (including myself). The meeting resulted in papers [121–123] on mathematical studies of Lie–admissible algebras and their appli-

cation to particle physics (field theory and Pauli's principle). The mathematical and physical foundations of the studies reported in this chapter were established in that year, such as: the direct universality of the Lie–admissible algebras in Newtonian mechanics; the main ideas of possible generalized relativities; the proposal to construct the hadronic generalization of quantum mechanics; etc.

The *Second Workshop on Lie–admissible Formulations* was held in August of 1979 at Harvard's Science Center. The meeting saw a considerable increase of participants, and resulted in the publication of two volumes of proceedings [124], one of review papers and one of research papers. With this second meeting, we succeeded in gathering mathematicians and theoreticians for one full week. Theoreticians would identify open physical problems, while the mathematicians would assist in the identification of applicable mathematical tools. The relaxed, friendly, and mutually respectful atmosphere permitted a number of mathematical advances, such as the identification of one of the most general forms of Lie–admissible algebras (by Y. Ilamed from Israel), or the continuation of the structure theory (by H. C. Myung, R. H. Ohemke, G. P. Wene from the U.S.A. and others). Some of the physical advances achieved at the meeting were: the proof of the direct universality of the Lie–admissible algebras in classical field theory (by J. A. Kobussen from Switzerland) and in statistical mechanics (by J. A. Fronteau and A. Tellez–Arenas from France, and myself); and other advances.

The *Third Workshop on Lie–admissible Formulations* was held in August 1980 at the new Harbor Campus of the University of Massachusetts in Boston. This time we succeeded in putting together in the same room for one full week mathematicians, theoreticians, AND experimentalists. The meeting resulted in the publication of three volumes of proceedings [125], one in pure mathematics, one in theoretical physics, and one in experimental physics and bibliography. The advances achieved at the meeting are too numerous to be outlined here.

The year 1981 saw a major thrust in the organizational efforts. Circumstances reviewed in the next chapter forced the founding of a new, independent, institute of research, the I.B.R. As a result of a considerable financial effort by individuals, a building was purchased in July 1981 within the compound of Harvard University (the "Prescott House") for the housing of the new institute which was formally inaugurated on August 3, 1981. The ceremony was attended by the governors, officers and advisors of the institute, as well as scholars from several countries (see the Appendix on the I.B.R.). Immediately after the inauguration, we had our *Fourth Workshop on Lie–admissible Formulations,* which saw further advances reported throughout this chapter (for instance, the discovery by the Austrian physicist, G. Eder of the possible mutation of magnetic moment while

keeping conventional values of spin, was presented at this meeting for the first time). The meeting resulted in a number of papers published in mathematical and physical journals.

The advances achieved during the preceding years permitted the organization of a new series of meetings, this time of formal character. In this way, we had our *First International Conference on Nonpotential Interactions and their Lie–admissible Treatment,* held in early 1982 at the Universite d'Orléans, France. This meeting saw a considerable increase in the participation (including participants from the U.S.S.R. and the People's Republic of China), and resulted in the publication of four volumes of proceedings [126] for some 1,700 pages of printed research in mathematical, theoretical and experimental aspects reported in this chapter. This new series is scheduled for continuation every few years. (The Second International Conference is scheduled for early 1986 in Europe).

Our First International Conference made us aware of having achieved the essential research objectives in classical mechanics. I therefore released for publication monographs [10, 12] outlining the primary results. This signaled the need for our focusing of the efforts in the hadronic generalization of quantum mechanics. For this purpose, a new series of yearly meetings was organized under the name of *Workshops on Hadrnoic Mechanics.* The first meeting was held at the I.B.R. in Cambridge, U.S.A., in August 1983, and resulted in the publication of proceedings [127]. (The second meeting, scheduled for August 1984, has been moved to Europe, as anticipated in Figure 1.6.2).

A considerable editorial effort was also promoted (despite well known, limited marketing potential*) consisting of the reprinting of collected works in salient segments of particle physics under the editorship of experts in the field [128–133]. More recently, these efforts permitted the funding and organization of a new journal in pure mathematics [134].

The Institute for Basic Research, the Journals, and the various Workshops and Conferences, have proved to be invaluable for advances in the limitations and possible generalizations of current relativities, mechanics and related mathematical structures. In fact, they have permitted the coordination of efforts by independent mathematicians, theoreticians, and experimentalists. Lacking this coordination, the advances would have been improbable. The understanding stressed earlier is that the studies are still at the beginning.

The progressive increase of the opposition.

*To have an idea, in the U.S.A. there are about 130 advanced research libraries interested in high energy physics (those of colleges with graduate schools in physics and of a few national laboratories). These libraries can generally purchase only a fraction of the new titles printed every year.

The existence of opposition, interference or shear suppression of due scientific process on our studies by vested, academic—financial—ethnic interests in the U.S.A., is beyond any reasonable doubt, in my personal view and experience.

The opposition was initiated by senior high energy physics at Harvard University with the prohibition for my drawing my salary from my own grant for one academic year (1977—1978). After my passing to Harvard's Department of Mathematics, the opposition continued with a number of ducumented episodes, such as the written prohibition to hold at Harvard our Third Workshop (which was in fact held elsewhere), despite the fact that it was an important part of my research contract. The opposition then continued with the refusal by Harvard to continue in the administration of my contract (despite the implied, considerable, financial loss of the related overheads). Harvard's refusal evidently propagated to other colleges, leaving no other choice than passing the administration of the contract to a non-academic corporation.

As we shall see, the organization of the I.B.R. was made necessary by the refusal of local colleges to provide even hospitality for me, let alone a regular academic job paid by my own governmental contract.

Opposition, interferences, and shear suppression of due scientific process continued in a variety of ways, such as: the prohibition to list I.B.R. seminars in the Boston Area Physics Calendar; the impossibility to publish papers in journals of the A.P.S.; the open warning to members of our group "to keep a distance from Santilli's studies" or to discourage their visiting our institute; the systematic rejection of all research grant applications filed by the I.B.R.; and other rather incredible (but documented) occurrences.

Admittedly, some of the episodes may have been due to my temperamental character, or to my firm determination NOT to accept gracefully academic manipulations on fundamental physical issues. I admit to these possibilities and assume all possible responsibilities. Nevertheless, the shear volume, number and diversification of the hostilities I have experienced are such to relegate my personality to a secondary role.

As far as the future is concerned, I shall gladly collaborate, most humbly, with the most humble colleague, on all topics reviewed in this chapter. The understanding is that arrogance will be met with magnified arrogance, and manupulatory practices on Einstein's ideas will be openly identified for what they are: scientific crimes.

The risk of turning physics into a farce.

Where ever the responsibilities lies, the end results are incontrovertible. The opposition by vested interests has succeeded

in preventing the conduction of comprehensive research at the I.B.R. on the inconsistencies and/or limitations of Einstein's ideas. The same research, however, is not conducted at other research institutions in the U.S.A. Whether intended or only accidental, the opposition has therefore succeeded in preventing the conduction of comprehensive research in the sector throughout the U.S.A. Any person aware of the international power of U.S. physics, will then see the propagation of the condition abroad.

This book intends to establish a record of the danger of a situation of this type.

A typical illustration may be the available experimental information on Pauli's exclusion principle in nuclear physics reviewed in Section 1.7 (Figure 1.7.2). As well known, the principle is ASSUMED in the data elaboration. The end results are then in agreement with the assumptions (see the lack of mutual overlappings of the wave–packets of the incident neutron on the tritium core in the upper right corner of Figure 1.7.2). It is evident that this situation could repeat itself ad infinitum, in the sense that new experiments could be done and never show an overlapping of the wave–packets because of the underlying assumption of the exact validity of Pauli's principle.

On the other side, one could re–elaborate exactly the same data under the assumption of a (small) violation of Pauli's principle due to the conceivable mutation of spin during the collision of the incident neutrons with the tritium core (Section 1.6). This would evidently result in overlapping wave–packets, that is, in exactly the opposite experimental conclusion of the upper–right corner of Figure 1.7.2.

The danger of suppressing, ignoring or otherwise discrediting dissident views is then evident. In fact, if we ignore the possibilities of sufficiently small deviations from Pauli's principle, we risk turning nuclear physics into a farce.

Along fully similar lines, if we ignore the critical literature of Einstein's gravitation (Section 1.5), we also risk turning gravitation into a farce.

If we ignore the irreconcilable incompatibilities between the established non–Hamiltonian character of our macroscopic world and the presumed Hamiltonian character of the particle descriptions (Figure 1.6.3), we risk turning research on irreversibility also into a farce.

If we ignore the impossibility of achieving an identically null probability of tunnel effects for free quarks under conventional, internal, quantum mechanical laws (Section 1.6), we also risk turning quark theories into a farce.

And so on.

If we do all these things simultaneously, and with one common root, the preservation of Einstein's theories, the risk is compounded. In fact, we risk the implementation of a scientific

obscurantism.

This is, after all, a rather natural consequence of any totalitarial scientific organization, where "physical truths" are imposed via shear academic power, rather than a scientifically democratic consideration of all possibilities, whether aligned or against Einstein's theories.

The financial dimension of the scientific accountability of Einstein's followers.

The continuation or correction of the current scientific scene in U.S. physics is up to you, fellow taxpayer. In fact, the research is conducted with your money. It is therefore time to have an idea of how much public money is involved in the sector.*

★In FY 1983, N.S.F. spent $ 4,900,000 of public funds in gravitation. A major portion of this sum has been spent on Einstein's theory of gravitation, that is, on a theory which is manifestly incompatible with physical reality according to numerous articles published in different refereed journals (Section 1.5). Papers published in the field under N.S.F. contracts have ignored the technical literature on the inconsistencies of Einstein's gravitation. Also, no self–correcting process of the governmental–academic complex is foreseeable, as stressed in Section 1.5. In FY 1984, N.S.F. plans to spend $ 6.1 million of public funds in gravitation and $ 7.9 million in FY 1985. Fellow taxpayers, shall you permit the continuation of N.S.F. dispersing public money on Einstein's gravitation under the ignorance of the technical literature on its inconsistencies?

★In FY 1983, N.S.F. and D.O.E. spent a combined sum in particle physics exceeding $ 100,000,000. A major portion of this sum has been spent in strong interactions under the assumption of the exact validity of Einstein's special relativity. At the same time, papers in the field published under governmental contracts have ignored the now vast literature on the expected approximate character of the special relativity. If this critical literature is correct, a significant portion of the $ 100,000,000 has been wasted. In FY 1984, N.S.F. and D.O.E. plan to spend over $ 110 million in particle physics, and over $ 121 million are scheduled for FY 1985. Fellow taxpayer, shall you permit N.S.F. and D.O.E. to continue in the dispersal of public funds under a totalitarian scientific condition aligned with the exact validity of Einstein's special relativity?

*The financial information below has been derived from *Physics Today*, April 1984, pages 55–60.

★In FY 1983, D.O.E. spent $ 461,300,000 in magnetic fusion. If the magnetic moments of protons and neutrons change under the fusion conditions (Section 1.2 and 1.7), a significant portion of this public sum has been wasted. $ 477.5 million are scheduled for FY 1984 and $ 483.1 for FY 1985. The test of the possible alteration of the magnetic moments under the fusion conditions via neutron interferometers costs less then $ 100,000 (Section 1.7). Fellow taxpayer, shall you permit D.O.E. to continue in the dispersal of public funds in magnetic fusion while ignoring the possible alteration of the magnetic moments?

My list of public expenditures in FY 1983 by Einstein's followers that are rendered questionable at least in part by the inconsistencies and/or limitations of Einstein's ideas could easily pass the mark of one billion dollars in the U.S. alone, particularly when military research is included. But I see no point in entering into such a detailed presentation, because the sole issue of scientific ethics is sufficient here. After all, we are talking about a totalitarian conduction of research in the ultimate foundations of physical knowledge.

IL GRANDE GRIDO

IT IS THE DUTY OF EVERY PERSON TO HONOR THE MEMORY OF ALBERT EINSTEIN AS ONE OF THE SINGLE GREATEST CONTRIBUTORS TO HUMAN KNOWLEDGE.

BUT THE LIFTING OF EINSTEIN'S IDEAS TO THE LEVEL OF RELIGIOUS DOGMA, TO BE PRESERVED INDEFINITELY VIA THE ORGANIZED SUPPRESSION OF POSSIBLE FUNDAMENTAL ADVANCES, WOULD BE A CRIME AGAINST HUMANITY.

CHAPTER 2

THE PERSONAL EXPERIENCE

2.1: HARVARD UNIVERSITY.

I now pass to the presentation of my personal experience beginning with my stay at Harvard University in 1977—1980. The fellow taxpayer should keep in mind that a true understanding of the various episodes reported in this chapter requires a sufficient knowledge of the scientific profiles reviewed in Chapter 1, which are and remain the most important ones. The episodes presented in this chapter will then be used in Chapter 3 for the submission of constructive suggestions to improve the scientific ethics in U.S. physics.

September 1, 1977.

The day started early, with my being in line at the unemployment office of Galen Street, in the town of Newton, Massachusetts. A nationwide search for an academic job in 1976—1977 had turned out to be a complete waste of time and money.* A number of hours passed while waiting, first, for the open-

* According to the guidelines set forth by the American Association of University Professors and other bodies, by 1977, I could not be hired by a U.S. college for a regular teaching job without a joint permanent position (tenure). This is due to the fact that by 1977, I had reached the maxium of seven academic years of teaching functions in U.S. colleges (the year of teaching in Italian colleges prior to leaving for the U.S. and the years of research employment in the U.S. without teaching did not count). This "numerology" evidently created substantial difficulties in my securing an academic job in the U.S. beyond 1977 which still persists to this day. The problem of "numerology" here considered is evidently not restricted to myself. Instead, it has invested and continues to invest so many scholars, to constitute a problem of national proportion. The search for a tenured position during the period 1967—1977 turned out to be fruitless. The best job I could obtain was the sadly known one—academic—year—TERMINAL—appointment, with the customary letter of remainder in mid year of the TERMINAL nature of the employment.

ing of the doors of the unemployment office, and then for the completion of all the formalities. I was told to have 33 weeks of unemployment benefits providing funds essentially sufficient to pay the rent of my two–bedroom apartment. With this I had to support my two children then in tender age and my wife (then a graduate student) while having virtually no savings and no other income.

Soon after completing my unemployment formalities, I went to the Lyman Laboratory of Physics of Harvard University to initiate a visit there under the unsalaried position of "Honorary Research Fellow" for the academic year 1977–1978. Steven Weinberg, then at the Lyman Laboratory, had expressed interest in certain papers of mine (on the conditions of variational self-adjointness in field theory; see ref.s [135]), and kindly offered the opportunity of spending a year at Harvard (Doc., pp. I–3–6).* After presenting myself at the departmental office, I visited Weinberg who received me quite cordially, and indicated that Howard Georgi (then a junior member of the department) would be my reference person. I left Weinberg sincerely pleased.

I therefore visited Howard Georgi, who also was quite cordial with me. In fact, I sensed positive feelings and the anticipation that our acquaintance could lead to a rewarding collaboration (a few months later Georgi and I founded the Hadronic Journal). While conversing on topics of disparate nature, the phone rang. On the other line there was David C. Peaslee of the Energy Research and Development Agency (ERDA), in Germantown, Maryland, near Washington, D.C., which became a few months later the U.S. Department of Energy (DOE). Peaslee was searching for the Harvard officer supervising my visit to invite my application for a research contract with ERDA. Georgi was visibly pleased by the invitation.

My plea to Weinberg.

The following day I phoned Peaslee. I told him that all my preceding applications to ERDA, filed from another college, had been systematically rejected, and that these rejections had been a significant reason for my inability to secure a tenured academic job. I frankly told Peaslee that, as a result of this history, I was not ready to reapply unless I received assurance that, this time, ERDA was seriously interested. Peaslee indicated his awareness of the preceding rejections and stressed the seriousness of ERDA interest at that time.

I had met Peaslee before. I trusted him and initiated all the various steps needed for the new application. First, I revised

* It should be indicated for future needs that, while Weinberg's letters clearly refer to the title of "Honorary Research Fellow", the formal letter of appointment I received from the university secretary refers to "Research Fellow in Physics" (p. I–3).

and updated the scientific part of the application, which essentially consisted of research underlying possible generalizations of available mechanics for contact/nonpotential forces (Section 1.3). The proposal was expected to result in a number of papers, monographs and scientific activities.

On September 5, I wrote to Weinberg a very respectful, hand written letter (p. I–5) in which I asked for his help in filing the research grant application to ERDA. In the same letter, I indicated that I was not aiming to remain at Harvard. Instead, I wrote Weinberg that I was merely interested in having the contract administered by Harvard the first year, and then move it to another college where I had some chance for tenure. The letter concluded by saying: *"I am currently unemployed; I have two children of tender age to feed and shelter; my wife is a graduate student; our savings are non–existent; and the unemployment benefits last only a few weeks."* I personally placed the letter in Weinberg's mailbox.

A few days later, I went to see him. He had seriously considered the case, by verifying the existence of the invitation, (one of the very few he had eyewitnessed, as he jockingly told me), and confirmed his help for the administrative formalities. Weinberg was aware of the topic of the application (which included papers [136]). In particular, he was aware that I had been working at the drafting and re–drafting of monographs [9, 10] which were then under consideration for publication by one of the most prestigious editorial houses in physics, Springer–Verlag of Heidelberg, West Germany.

The administrative difficulties in filing the invited application to ERDA/DOE.

Weinberg showed me Harvard's faculty manual indicating that only full professors qualified as principal investigators of research contracts. Being a research fellow, I could not therefore apply alone, but had to search for a full professor interested in serving as principal investigator of the contract with me as co–investigator.*

Weinberg did a genuine effort for that. In fact, he personally contacted a number of administrators in the department and in the Dean's Office; he introduced me to potentially interested colleagures; and tried other avenues. Regrettably, it was impossible to locate any full professor in physics who could serve as principal investigator. Steven Weinberg, Shelly Glashow and Sidney Coleman were principal investigators of a contract with the National Science Foundation (NSF), and could not serve in the same capacity for a contract with ERDA. Other colleagues

* The manual did allude at the possibility of waiving the restriction and permitting research fellows to be principal investigators, but this possibility was not considered in my case.

we contacted, such as Roy J. Glauber, even though under ERDA support, were not interested or had other reasons to decline.

ERDA independently explored other avenues. A senior Italian experimentalist at Harvard, C. Rubbia, was part of an experimental team operating under ERDA support. To my understanding, Peaslee contacted Rubbia proposing the incorporation of my contract into his via a budgetary increase of the funds, plus other benefits. Rubbia apparently refused the proposal (and the money, including the considerable overheads for Harvard) on grounds unknown to me. I had never met Rubbia, nor I believe that he had ever heard my name before. What struck me was his rejection without even bothering to call and talk to me. After all, my office was not that distant from his. What an ackward behaviour, particularly from a compatriot! What a difference with other ethnic groups!

In the meantime, months were passing by and my financial situation was becoming more critical. Nevertheless, the scientific qualifications for my research activities with or without ERDA support, were increasing. For instance, I delivered at Harvard an informal seminar course in the topic of my monographs, which was attended by a number of graduate students from the local universities (p. I–8). Subsequently, W. Beiglböck, Editor of Springer–Verlag for the series "Textbooks and Monographs in Physics", sent me the formal acceptance of the publication of my volumes (p. I–10). In addition, I had written a paper in "Harvard style"readily accepted by Phys. Rev. D (ref. [136]; see p. I–55 for the front page of the Lyman preprint) and was working at several other projects.

By October, 1977, I had exhausted all possible avenues for filing the invited application with a principal investigator from the Department of Physics (Georgi was not qualified because not a full professor at that time).

I therefore attempted to file the application under the administration of Boston University, where I would have no difficulty to be principal investigator under my title of Associate Professor of Physics. Boston University readily accepted the proposal, which was prepared and signed by the necessary administrative officers (p. I–15). Unfortunately, this change of administration was not well received by ERDA, and that application was never filed in Washington. In fact, all the preceding rejections I had received from ERDA regarded applications filed precisely under the administration of Boston University.

After this last episode, my personal situation deteriorated considerably. I was left with a few additional weeks of unemployment benefits to pay the rent, while the lack of savings began to affect visibly my family. I had no other alternative but initiate suitable scientific actions. That meant to put in black and white the insufficiencies and limitations of Einstein's theories.

In my "last progress report" to S. Weinberg, M. Tinkham (the departmental chairman of that year), and H. Georgi of December 4, 1977 (p. I–16), I disclosed my second series of monograph [11, 12], with copies of the statements by colleagues released by a new publisher (p. I–18). To be as clear as possible, I entitled the first volume "Nonapplicability of the Galilei and Einstein Relativities?" and the second volume "Coverings of the Galilei and Einstein Relativities?"

I had crossed my scientific Rubicon for the first time. At any rate, I had no other alternative. The monographs were my only hope for some income.

The filing of the invited application to ERDA with S. Sternberg as principal investigator.

In mid December, 1977, an unexpected event occurred. Shlomo Sternberg, a professor of mathematics at Harvard (and that year chairman of the department), was aware of my papers on the topic of the invited application to ERDA and indicated interest in being the principal investigator. Sternberg is a renown geometer. As such, he qualified in full for the position.

Sternberg and I had a brief meeting on the matter in which we readily reached a full agreement on all aspects. After that, everything moved quickly. My part of the application had been written and rewritten countless times and was ready. It took a few hours for Sternberg to prepare his own part, its enclosures and the front page. After that, I asked authorization from Tinkham, in his capacity as chairman of the physics department, to file the application with Sternberg as principal investigator and with me as co–investigator, UNDER MY AFFILIATION WITH LYMAN LABORATORY OF PHYSICS. I emphasized this last point because, since I am a theoretical physicist and not a mathematician, I was not expecting to qualify for an association with the mathematics department. Independently from that, Sternberg contacted the senior members of the Lyman Laboratory to have the go ahead under the same terms, which was readily given (see copy of the front page of the application on p. I–45). In this way, it took very few days to complete the application; to have it signed by the various administrative officers; and to have it shipped by Harvard's Office of Research Contracts (ORC) to ERDA. In turn, it took only a few weeks for the scientific office of ERDA in Germantown to approve the application and send it to ERDA's administrative office in Argonne, Illinois, for funding. Each and every one of Peaslee's words turned out to be correct, as expected.

The impossibility of receiving a salary under my own grant.

Always alert for possible things that could go wrong, and with deteriorating family conditions, I kept checking on the progress of the contract. In early April, 1978, I discovered that I COULD NOT DRAW MY SALARY FROM MY OWN GRANT because, according to university regulations, I had an appointment as "Honorary Research Fellow", that is, an appointment without compensation, while I needed an appointment at least as "Research Fellow" to draw a salary.

On April 6, 1978, I therefore wrote a formal application to Tinkham asking for the removal of the word "Honorary" in my title, so that I could draw a salary under my contract (p. I—24). That application signaled the initiation of a crisis that, a number of years later, rendered unavoidable the writing of this book.

On personal grounds, my unemployment compensation would end in April, 1978. In turn, this raised the spectrum of: possible eviction of my family from our apartment because of lack of payment of rent; lack of money to buy food; etc.

On administrative grounds, the remaining formalities had been completed by ERDA and Harvard's ORC; the contract was operative under number ER—78—S—02—4742; and the money was sitting in a bank somewhere, including the money for my own salary.

On scientific grounds, my research on the limitations and possible generalizations of Einstein's theories had become better known to the members of the Lyman Laboratory (see Figure 2.1.1 and, later on, Coleman's case).

The chain of repetitious rejections by Coleman, Glashow, Weinberg and possibly other senior physicists at Harvard to prevent my drawing a salary from my own grant.

The months of April, May and June, 1978, saw repetitious rejections of my appeals to senior physicists at Harvard with a predictable deterioration of the relationship.

The affair evolved like this. By the end of the week, I would phone the chairman of the physics department to inquire about the status of my application for the removal of the term "Honorary" from my title. Tinkham would generally tell me that the case would be considered at the senior faculty meeting of the following week. The day after the meeting, Tinkham would usually call me to indicate that the senior faculty had voted against my appointment as "Research Fellow", that is, against the removal of the word "Honorary" from my title, which implied my inability to draw a salary from my grant*.

* The fact that my official appointment had the title "Research Fellow in Physics" (Doc. p. I—3) remains a mystery to me to this day. In fact, I have been unable to figure out why with this title I was prohibited to draw a salary from my grant.

At the beginning, I was as courteous as permitted by the circumstances. It should not be forgotten that Harvard had formally approved and filed a governmental contract with my affiliation to the physics department (p. I—45). Now that the grant had been funded by the U.S. government, the physics department was preventing, opposing or otherwise jeopardizing its actuation.

By May, 1978, my unemployment benefits had expired; my family was truly risking eviction and lack of money to buy food; while the senior physicists at Harvard were still preventing my drawing a salary from my own grant. This situation should be kept in mind while passing judgment on anything I did during (and after) that period, such as the letters I wrote to directors of National Laboratories (p. I—360 and ff.), or my exchanges with officers of the American Physical Society (APS), notoriously aligned with vested interests at Lyman.

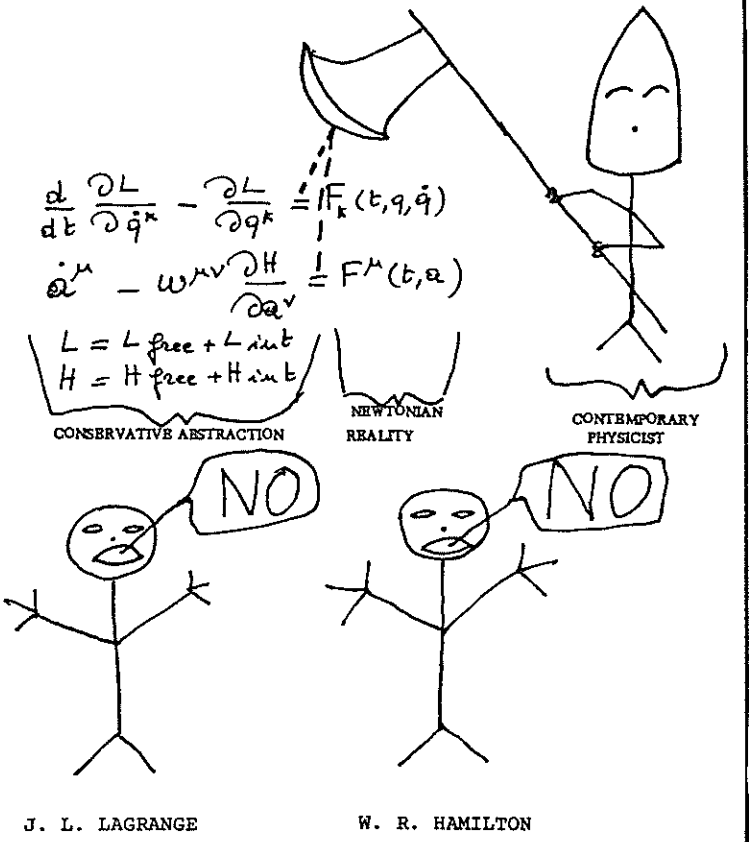
Some of the dates of the repetitious rejections have been documented in the front pages of ref.s [8] and [14]. I had planned to release these memoirs several years later, under the evident assumption of having my salary supported by the DOE contract. The prohibition to draw my salary compelled me to anticipate their publication. Thus, every time that Tinkham would call me to report the negative decision of the senior faculty, I would improve ref. [8] and [14] and resubmit them to the Journal, thus resulting in the indicated partial record of negative decisions (see the dates of pp. I—56 and I—57).

On May 10, 1977, Tinkham wrote me a letter (p. I—43) communicating the final negative decision by the physics department.* In that letter, he expressed the view of his department according to which, since the principal investigator of my contract was a member of the department of mathematics, I should seek an affiliation with that department.

There is little point in indicating my surprise. In fact: (a) I had asked and obtained authorization to file the grant application with my affiliation to Lyman and the same result had been independently reached by Sternberg (p. I—45); (b) copy of the research grant application had been passed to the Physics Department in January, 1978; and, last but not least, (c) I had expressed to Tinkham my impossibility to apply for a position at Harvard's mathematics department simply because I am not a mathematician.

Why S. R. Coleman, S. L. Glashow, S. Weinberg and other senior physicists at Harvard had collegially changed their commitment with the U.S. Government? Why had they waited so many months to tell me to apply for a position at the mathe-

* Howard Georgi was not part of this decision, to my knowledge, because a junior faculty at that time, while the various meetings on my case had been restricted to the senior faculty.

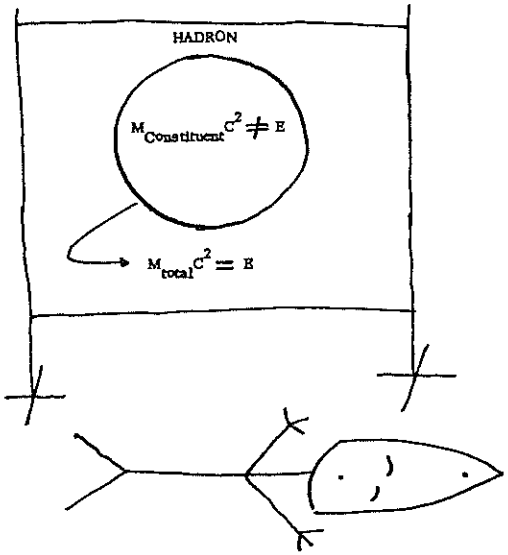


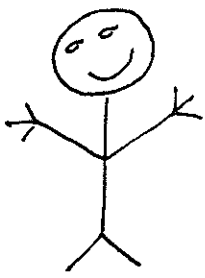
FIRST VIGNETTA:

THE TRUNCATION OF

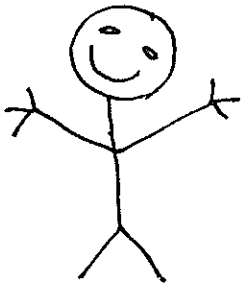
LAGRANGE'S AND

HAMILTON'S EQUATIONS





G. GALILEI



A. EINSTEIN

SECOND VIGNETTA:

RELATIVISTIC

FAINTING

SPELLS.....

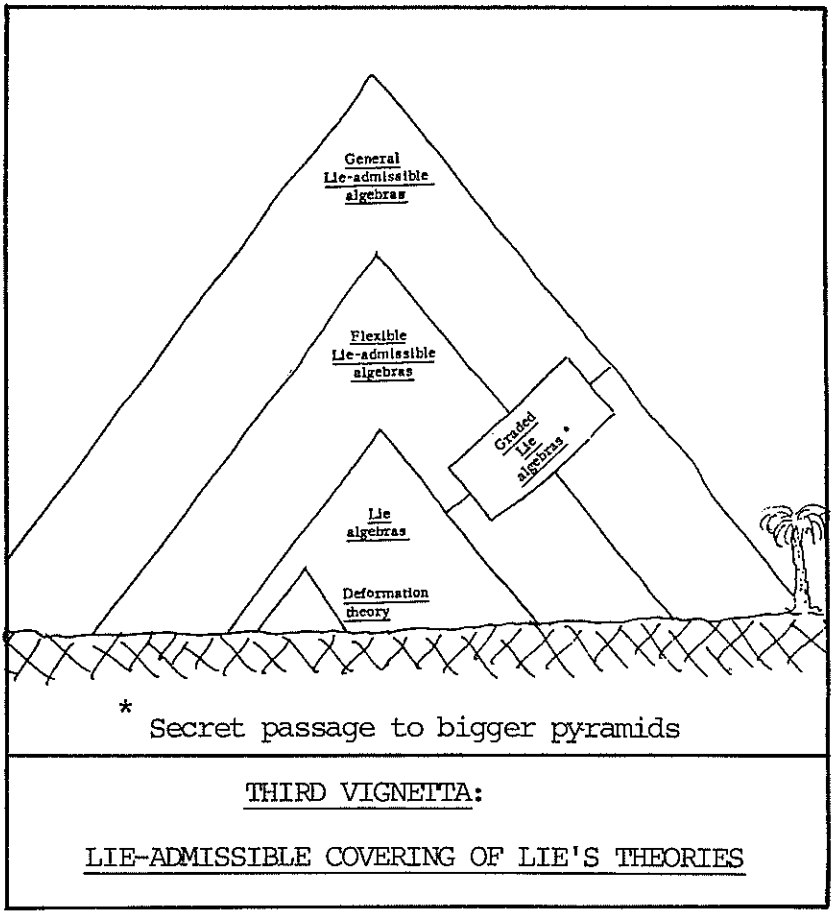


Figure 2.1.1. The three "vignette" appended to a presentation dated April 26, 1978 I submitted to the senior members of the Lyman Laboratory of Physics of Harvard University (p. I–26–32), following a request of additional information of my research by the departmental chairman M. Tinkham. The information was needed for action on my application for the removal of the term "Honorary" from my title, so that I could draw a salary from my own contract then in full administrative standing (DOE contract number ER–78–S–02–4742). The senior physicists at the Lyman Laboratory were aware of the topic of my monographs with Springer–Verlag (the first volume was in print at that time) and related papers, but they had no specific idea how the underlying techniques would be used in particle physics. My presentation to the Lyman Laboratory of April 26, disclosed the intended use of the techniques: to conduct a study of the limitations and possible generalizations of Einstein's theories in the interior of nuclei, or of strongly interacting particles (hadrons) or of stars along the lines essentially reviewed in Chapter 1. The three vignette were appended in the hope of toning down the topic and stimulating a friendly atmosphere. The first vignetta depicts the historical roots of the contact/nonpotential forces among extended particles. The founders of contemporary analytic mechan-

ics, Lagrange and Hamilton, had formulated their celebrated equations with external terms representing precisely the forces considered. These external terms had then been "truncated" since the beginning of this century because not needed in the description of planetary trajectories or of the evolution of electrons in the atomic clouds. The same external terms, however, had to be added for more complex trajectories of non–perpetual–motion–type, such as the motion of a proton within the core of a star. The resurrection of the historical external terms in Lagrange's and Hamilton's equations then implied the irreconcilable abandonment of Einstein's relativities for a number of technical reasons reviewed in Chapter 1 (such as the breakdown of the Lie character of the underlying algebraic structure). The second vignetta depicts a rather heated discussion I had sometime in early 1978 at the Lyman Laboratory with F.F.F., a firm believer of the unlimited applicability of Einstein's theories. The third vignetta presents a schematic view of the mathematical tools I was using for the construction of possible generalizations (the Lie–admissible algebras). The presentation stressed the scientific iterim which, as the reader can see, has been strictly implemented in the outline of the scientific case of Chapter 1, and which consists of

1– Identification of an arena of unequivocal applicability of Einstein's special relativity (point–like particles, such as electrons, moving under electromagnetic interactions, as originally conceived by Einstein);
2– Identification of broader physical conditions implying doubts on the exact validity of the special relativity (extended/deformable particles such as protons and neutrons under the conditions of mutual overlapping of the strong interactions, which were unknown at the time of formulation of the special relativity, and which imply the presence of contact/nonlocal/non–Hamiltonian forces);
3– Identification of mathematical tools (such as the Lie–isotopic and Lie–admissible algebras) which are broader than those underlying the special relativity (Lie algebras) and capable of incorporating non–Hamiltonian forces at least in local approximation;
4– Attempts to construct a generalization of the special relativity for the broader physical conditions considered via the use of the broader mathematical tools, under the conditions that the new relativity contains the old as a particular case (see ref. [8] for the Galilean case; ref.s [12, 33] for the special relativistic case and ref.s [50, 51] for the gravitational case).
5– Formulation of experiments for the resolution of the problem of exact or only approximate character (or, strictly speaking, the validity or invalidity) of the special relativity under the broader conditions considered.

This scientific iterim was submitted to the senior physicists at Harvard not only with the presentation of April 26, 1978, but also in a variety of other ways, such as: the submission of a draft of memoir [8] to S. Coleman for review (see below in the main text); the presentation to departmental members of the subsequent memoir [14] on the need to test the special relativity under strong interactions; and other ways. Despite the friendly and respectful tone, the presentation of April 26, 1978, did not achieve the in-

tended objectives. In fact, I never received any scientific assistance and/or comment whatsoever from Harvard's physicists on my efforts, while the primary reason for my going to Harvard was precisely that of receiving a minimal, but scientifically professional assistance on such a manifestly difficult problem. Second, the senior physicists of the Lyman Laboratory of Physics voted against the removal of the term "Honorary" from my title, or, equivalently, against my appointment as "Research Fellow", by therefore preventing in this way that I draw a salary from my own grant.

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matics department? Why had they done these things in full awareness of the consequential hardship on my children?

The most probable answer is evident: they opposed the actuation of my DOE contract at their department, that is, they opposed studies on the limitations and possible generalizations of Einstein's ideas in the interior of strongly interacting systems.

Needless to say, my personal opinion is insignificant. What is important is the opinion of the fellow taxpayer who has provided large financial support to Weinberg, Glashow, Coleman and other members of the Lyman Laboratory on research in particle physics under the (tacit) assumption of the exact validity of Einstein's theories under unlimited physical conditions.

More on Sidney Coleman.

In late 1977, Howard Georgi and I founded the Hadronic Journal. The first issue was scheduled for printing at the end of April, 1978. In early 1978, we were carefully selecting the papers for the first issue (mainly by invitation). Also, as editors, we had decided to print in the first issue one paper each. By April, Georgi had completed his paper [137] (on soft CP violation), while I was working at the drafting and redrafting of a memoir on a conceivable Lie–admissible generalization of Galilei's relativity [8].

However, as indicated earlier, my plans were to work at that memoir for a number of additional years before releasing it for printing, in case my salary had been finally authorized. In place of ref. [8], I could have readily prepared for the first issue of the journal another paper written in "Harvard style", such as ref [136]. In short, I was waiting for the physics department to resolve the issue of my salary, so that, in turn, I could decide whether or not to publish memoir [8] in the first issue of the Hadronic Journal. I had submitted several drafts and redraftings to colleagues, experts in the essential topics (mechanics, algebras and geometries). But I still lacked a critical inspection of the memoir from a competent fellow at Harvard.

For these reasons, in early April, 1978, I visited Sidney Coleman, indicating the case, and asking for the courtesy of a critical review of the manuscript. Coleman indicated interest, and actually stressed that I should give him a copy, but he could

look at it only after filing his tax returns.

On April 15, 1978, I therefore wrote a very courteous note to Coleman asking for a critical examination of the manuscript and for counsel (p. I–25). I had selected Coleman because he was one of the few physicists at Harvard with the necessary mathematical knowledge to understand, first, the proposed generalized algebras and geometries, see how corresponding generalized mechanics follow, and finally, see how a generalization of Galilei's relativity was inevitable within such a setting.

Regrettably, I never heard or saw Coleman again after my petition of April 15, despite a number of solicitations such as those of April 27 (p. I–33) and May 5 (p. I–38). Nevertheless, I was told that Coleman, while being totally silent with me, had been quite generous of criticisms on my memoir at the senior faculty meetings on my case.

Subsequently, in a letter to Tinkham of July 19, 1978, (p. I–47), I expressed my "extreme disappointment" for Coleman's behaviour "because contrary to centuries of scientific traditions to which I have been educated, and contrary to the confidentiality of the formal referee process". In fact, the memoir had been clearly submitted to Coleman for refereeing, with a clear mark on the front page indicating "Rudimentary draft for confidential communication" (p. I–26). As chairman, Tinkham treated the case with manifest disinterest.

Centuries of traditions in scientific ethics should have definitely prevented Coleman from expressing his criticism to others while keeping silence with me.

But, again, my personal opinion is immaterial. The important opinion is that by the fellow taxpayer who has financed Coleman's research for years.

The appointment at Harvard's mathematics department.

In this way, I was left with no other choice than apply for a position at the Department of Mathematics, which I did on May 16, 1978, (p. I–45). The mathematical content of my monograph [9] was considered sufficient for a position; my application was accepted in a matter of a few weeks; and, FINALLY, in June, 1978, I drew the first salary from my DOE grant.

The entire affair at Lyman remained, for me, substantially beyond a rational explanation, as it remains today. During the entire period of the affair, I was indeed a formally appointed member of the laboratory and, as such, I was regularly publishing articles and books with my affiliation to the Lyman Laboratory. Under these circumstances, which was the rational explanation underlying the decision by the senior faculty there to prevent my drawing a salary under my own grant, while jointly preventing Harvard from cashing the related, considerable, overheads? How could such a behaviour under said circumstances be

rationally explained, if one keeps in mind the fact that the case had been pushed to such extremes, to be very close to the filing of multimillion dollar law suits?

The most plausible explanation I could find is that the senior faculty at Lyman apparently intended to use the hardship on my children and my wife as a possible means of bending my complete independence of scientific thought into a form compatible with their research lines. If that was the case, Coleman, Glashow, Weinberg and the other senior faculty there incurred into in a major misperception. I am a committed free person, humanly and scientifically. My complete independence of scientific thought simply has no price.

Judging in retrospect, I am happy to see that the episode was one of the most instructive of my life. For instance, I learned the way to conduct an intense financial activity while owning nothing, in such a way to be able to inflict the maximal possible damages permitted by law, while suffering the minimal conceivable damages. Also, in the long run, the episode turned out to be most productive for me, in the sense that it forced my undertaking of a number of scientific initiatives that otherwise would not have seen the light. In fact, I am happy to admit that I own a number of my achievements to the obstructions I experienced from Coleman, Glashow and Weinberg.

Final report to the Lyman Laboratory.

At the time of expiration of my honorary appointment at the Lyman Laboratory on June 30, 1978, I presented my final report according to customary departmental practice. The report summarized my scientific activities for the past academic year which include (pp. I–49–61):

a– The reception of a DOE research contract;
b– The funding of the Hadronic Journal;
c– The publication of two monographs [9, 11] and the preliminary drafting of additional ones;
d– The writing of a number of articles and memoirs in Physical Review D [136] and in the Hadronic Journal [8a, b; 14];
e– The delivery of an informal seminar course on the Inverse Problem at Lyman;
f– The delivery of a number of formal or informal seminars (at: the International Center for Theoretical Physics, in Trieste, Italy; the Institut voor Theoretische Mechanica of the Rijksuniversiteit, Gent, Belgium; the Institut für Theoretische Physik der Universität, Zürich, Switzerland; the Department of Physics of Northeastern University, Boston; and the Department of Physics of Queens College, New York); and,
g– The conduction of referee work for a number of

journals, besides the Hadronic Jouranl, such as: Physical Review Letters; Physical Review D; Annals of Physics; and others.

All this was achieved while being unemployed.

The first comprehensive report to Derek C. Bok, President of Harvard University, on December 27, 1978.

After leaving Lyman for the mathematics department, I though that my problems were over, and that I would have been left in peace to conduct research under the DOE contract.

I was wrong.

The opposition by Coleman, Glashow, Weinberg and possibly others against the conduction of studies on the limitations of Einstein's theories continued, propagated outside the university; and eventually rendered the writing of this book unavoidable.

The first, outside, negative, intervention of which I am aware,* occurred when senior physicists from Lyman indicated to senior mathematicians that "Santilli's studies have no physical value". In turn, this created evident, apparently intended problems for my appointment there, clearly, because I was a physicist. Mathematicians had to consider the judgment of their physical colleagues to appoint me. It was only thanks to the mathematical content of my research that this additional problem was by—passed.

The situation deteriorated substantially in December, 1978. In essence, Sternberg was interested in continuing the contract. As a result, I was not in a position to move it to another college, as originally planned. My only possibility to keep the contract was therefore that of remaining at Harvard. At that time, Sternberg and I had a sincere, scientifically and humanly rewarding relationship.☆ He had no personal objection on my continuing under our DOE contract for one additional (although terminal) year.

By December, 1978, the application for the renewal of the

*The episode of the denial of hospitality under contract with the U.S. Department of Energy by the European Organization for Nuclear Research (CERN) of Geneva, Switzerland (Appendix A), should be kept in mind. In fact, it is evidently unlike that CERN reached a negative decision on an application for hospitality originated at the Lyman Laboratory without first consulting senior members there.

☆See my letter to Sternberg of p. I—66 while he was at Tel—Aviv. It concerns the sudden death of one of my best personal friends, the Jewish musician, John Boros of Brandeis University, and his Italian wife Emy. We had joined forces here, organized a fund raising, and succeeded in doing a record of John's (beautiful) musics. I asked Sternberg to donate one sample of the record to any public collection in Israel preferably, that of Tel—Aviv University.

contract for one second year had to be filed. Its renewal from the part of the DOE was expected to present no problem. I had contacted David C. Peaslee at the DOE in that respect, and he had explained to me that the second year renewal was normally done without external refereeing. All the books and papers Sternberg and I had published during the first year were more than sufficient, in Peaslee's view, to warrant the renewal of the contract for one additional year.

The problems for the renewal were at Harvard, that is, they were at the Lyman Laboratory of Physics. In fact, one day in the second half of December, 1978, Sternberg came to me saying that he was experiencing extreme difficulties in securing the renewal of my appointment at the department of mathematics under the DOE contract, because of the insistence on the "lack of physical value" of my research from the senior members of the physics department. As a result of that, Sternberg was proceeding alone with the renewal of the contract without my participation. This meant for me, again, unemployment a few months later on.

Two things then happened, almost simultaneously. On December 27, 1978, I wrote my first, comprehensive, ten—page report to Derek Bok, in his capacity as President of Harvard University, with copy to Richard G. Leahy, in his capacity as Associate Dean in the Faculty of Arts and Sciences. The report (pp. I—72—81) was studiously written in a language as candid as possible for the intent of identifying the implications and potential danger for Harvard of the posture by Coleman, Glashow, Weinberg and possibly others. The objective was to prevent that the apparent opposition against the study and experimental resolution of the validity or invalidity of Einstein's ideas in the interior of hadrons would propagate from individual faculty to the entire university. Stated differently, my objective was to prevent that the personal problems of scientific accountability vis—a—vis the U.S. taxpayers by Coleman, Glashow and Weinberg extend to the entire university.

This time, I intentionally became repetitious by conveying and reconveying again the same message to Bok a number of additional times, such as those of January 11, 1979, (p. I—82), May 6, 1979, (p. I—100), September 23, 1979, (p. I—127), May 1, 1980, (I—172), May 8, 1980, (p. I-175), and even telegrams just a few days before leaving Harvard (see below). The clear objective of all these letters was to make absolutely sure that Harvard's administration knew in all the necessary details the ethical implications for the suppression of studies on the verification of Einstein's theories.

One thing I studiously attempted to convey to Bok with this correspondence,is that I was not a "Harvard man" as customarily intended in the Yard. In fact, I studiously avoided the use of "Harvard's language" (a concoption of allusory remarks

which: avoid the direct consideration of the case at hand; are formulated in the most concise possible terms; and are expressed only in case of extreme necessity — ignorance being the most dominant "language" in the college). Instead, I consider it a question of principle to be as specific as conceivably possible, owing to the gravity of the case and of its implications.

At any rate, it was clear that I was at Harvard to attempt the free pursuit of novel physical knowledge and NOT a career in the University, with full knowledge that these two pursuits, in my case, were irreconcilably incompatible.

I believe that I did succeed in conveying the necessary information. However, Derek Bok turned out to be substantially uninterested, to use an euphemism, as we shall see. Back to my first report of December 27, 1978, it remained unacknowledged.

Independently from this report, Sternberg had contacted the DOE office indicating his decision to submit the renewal application for one second year without my name. Peaslee discouraged quite firmly such a renewal, indicating that the likelyhood of its funding would have been very small. I still remember when Sternberg came to my office reporting this phone conversation and indicating his embarassment.

In this way, we reached the decision to apply for the renewal of the DOE contract with my affiliation this time to the Department of Mathematics. Sternberg evidently followed the administrative iterim with all due care, beginning with the formal approval by the mathematics department, and then passing to the approval by the appropriate administrative bodies, and finally releasing the contract to the ORC.

I thought that my problems were over for at least one more year. They were not. The DOE contract was soon renewed. However, when time came for the renewal of my appointment, the senior physicists created additional difficulties at the mathematics department. The case has been reported in Section 1.6, pages 132—136 (of this volume), and resulted in a paper of criticisms on quarks I wrote and distributed worldwide in 15,000 copies (see Doc. p. I—97 for copy of the front page).

The subsequent moratorium at the Hadronic Journal for the publication of papers on nonrelativistic quark conjectures because of excessive inconsistencies (Section 1.6, pages 136—140), also belongs to that period.

The proposal to President Bok to organize a new center of research within the university.

The scientific initiatives of 1977 and 1978 had created a considerable interest in the physical and mathematical communities. By late 1978, an increasing number of scholars were becoming interested in the Lie—isotopic and Lie—admissible generalizations of Lie theory, and their applications to classical

mechanics, statistical mechanics, particle physics and other disciplines.

This information originated not only from the papers routinely arriving at my editorial desk, but also by the ongoing organization of our *Second Workshop on Lie–admissible Formulations,* as well as from the requests of scholars to visit me at Harvard.

It was clear that I could not effectively relate to such a growing activity while being a member of the department of mathematics. The most effective way would have been to organize a new center of research, for the conduction and coordination of research on generalizations of Lie theory and their applications (including possible military applications; see Section 1.6, pp. 120–123).

In early January, 1979, I therefore proposed to President Bok the consideration of the possible founding of a new branch of the university under the name of "Center for Hadron Physics" or any other more preferable name, such as "Center for Applied Mathematics" (pp. I–82–83). As an incidental note, I made it clear that I was not a candidate for an executive position. I was merely interested in being a member.

The proposal soon received encouraging, although informal, support from mathematicians at Harvard, such as Sternberg and the new chairman for that year, Heisuke Hironaka. The proposal was also informally communicated to DOE in Germantown. Pleaslee had a meeting with Hironaka on the project, confirming the best possible consideration of possible research proposals. To stress the feasibility of funding this possible new center, Peaslee indicated that, in case needed, it could get started with my existing contract (which would have implied no financial disbursement from the University, but actually the acquisition of new overheads). Everything looked quite promising at that time, until . . . .the proposal reached the senior physicists at Lyman. In fact, Hironaka subsequently communicated to me the existence of an "extreme opposition" conveyed through Dean Paul Martin from Pierce Hall. Associate Dean Leahy subsequently indicated in a letter of January 24, (p. I–85), that the proposal was solely in the hands of the faculty, who had to approve it, formally endorse it, and then submit it collegially to the administration. By late January, the proposal was evidently dead.

I still wander how much America has lost with the suppression at birth of this new center of research in pure and applied mathematics, and what scientific (as well as military) contributions the center would have achieved in case truly permitted to pursue novel advances in disrespect of vested, academic–financial–ethnic interests.

The unsuccessful attempt to interest Harvard's Center

for Astrophysics.

I had promised to Sternberg first, and then later to Hironaka NOT TO APPLY to the department of mathematics for a third year and I kept my promise.

Sternberg still wanted to continue the grant and, therefore, I could not move it elsewhere. I was then left with no other choice than attempting to interest Harvard's Center for Astrophysics. My research had in fact direct gravitational implications (Section 1.5). A possible research position at the Center for Astrophysics would have been fully sufficient for the continuation of the DOE contract with Sternberg.

I therefore contacted Fred L. Whipple first, then Director of the Center (p. I—107), his successor G. B. Field (p. I—111), and R. Giacconi (p. I—144), one of its members, by conveying the main scientific aspects of the program. I received from all of them courteous acknowledgments, but no true interest materialized.

For me, this meant to leave Harvard.

For the Center for Astrophysics, it meant the continuation of a considerable problem of scientific accountability vis—a—vis the taxpayer. In fact, to my best knowledge, research at that Center has been continuing on conventional, Einsteinian, gravitational theories, without any consideration and/or quotation of the literature on their manifest inconsistencies or disproof of dissident views (see Section 1.5 for scientific details and Section 3.3 for suggestions to the taxpayer).

Harvard's refusal to house on campus the Third Workshop on Lie—admissible Formulations under governmental support.

As indicated in Section 1.9, we held, under DOE support, our *First Workshop on Lie—admissible Formulations* in early August, 1978, in a very informal way, at the office kindly provided to the (three) participants by G. Birkhoff (the mathematician, son of the mechanicist to whom I named the "Birkhoffian mechanics" [8, 10]).

The *Second Workshop* was held, under DOE support, at the Science Center of Harvard in early August, 1979. The participation this time was considerably greater. The meeting resulted in two volumes of proceedings (see ref.s [124] or pp. I—118—122 for reproductions of their Table of Contents).

Throughout the last year at Harvard, I worked at the organization of the *Third Workshop on Lie—admissible Formulations.* The meeting had to be scheduled in early August, 1980, because of the inability of the participants to attend at an earlier date.

But . . . ., my contract at Harvard expired on May 31, 1980. I therefore wrote the following letter (p. I—156):

Professor H. HIRONAKA *April 25, 1980*
Chairman
Department of Mathematics *UNIVERSITY MAIL*
Dear Professor Hironaka,
I acknowledge receipt of your recent note confirming the termination of my appointment on June 1, 1980, and indicating the possibility of my continuing to use the current office for a limited additional period of time (and definitely not beyond August 15, 1980).
For your information, and as a rather important part of my current research under DOE support, the THIRD WORKSHOP IN LIE–ADMISSIBLE FORMULATIONS was tentatively scheduled in Cambridge (from August 4 to 9, 1980) several months ago.
The organization of this workshop is now close to completion. A list of participants is enclosed. In addition, we contemplate to have a number of distinguished guests (such as editors of physics Journals).
I assume you have no objection for having this scientific event at Harvard, and I am continuing the organization under this assumption. *RMS/ml*
Very Truly Yours, *ecls.*
Ruggero Maria Santilli *cc: Ass. Dean Leahy*

The list of participants indicated in the letter included a considerable number of distinguished, senior, mathematicians, theoreticians, and experimentalists from the U.S.A. and abroad, including "corresponding participants" from Eastern Countries (for specific names and addresses, see the three volumes of proceedings [125] or the Table of Contents reproduced on (p. I–176–184).

On May 2, 1980, I received the following answer (p. I–174).

Dear Dr. Santilli, *May 2, 1980*
According to my letter of February 12, 1980, which you clearly received and acknowledged in your letter of April 25, 1980, your status at Harvard is to be totally ceased on May 31, 1980.
Therefore you have no right whatsoever to call for a meeting or conference, academic or otherwise, to be held on the premises of Harvard University after the date of the termination of your appointment, unless you were to obtain special permission from the appropriate administrative board of Harvard University. In any event, you have no authorization and no recommendation from our Mathematics Department for the Hadron Workshop to be held at the Science Center during the summer after May 31.
Sincerely yours, *HH/mjm*
Heisuke Hironaka *cc: Dean Richard G. Leahy*
Chairman *Enclosures*

As one can see, my status had "to be totally ceased on May 31, 1980", and this included all scholars who had been contacted to be hosted by Harvard as part of research under a contract with the U.S. Government!

Evidently, the case was too serious to leave it to Hironaka and Leahy alone. I therefore reported the case to President Bok with a letter of May 8 (p. I–175).

Subsequently, during the last days of my stay I sent to Bok two telegrams soliciting his intervention for the holding of the meeting as originally scheduled at Harvard.

Bok did not acknowledge these last communications.

At 11 p.m. of the night of May 31, 1980, I dismantled my office and left Harvard.

The *Third Workshop* was held at the New Harbour Campus of the University of Massachusetts in Boston. Copy of Hironaka's letter was evidently circulated at the meeting when the participants asked me the reasons why the workshop had not occurred at Harvard as scheduled one year earlier (virtually all participants had their Hotel reservations near Harvard in Cambridge and rather far from the U–Mass campus in Boston).

The opposition by the Lyman Laboratory of Physics at Harvard to list seminars by the Institute for Basic Research in the Boston Area Physics Calendar.

After leaving Harvard and founding our independent Institute for Basic Research (I.B.R.–see next section for details), I thought that FINALLY, I would be left in peace to conduct my research. AGAIN I WAS WRONG! In actuality we were only at THE BEGINNING OF THE PROBLEMS. I shall report below only one case, and present others in the remaining parts of this presentation.

In April, 1982, G.G.G., a distinguished, senior, U.S. mathematician, co–author of a famous book in Lie theory among numerous other works, and member of the Division of Mathematics of the I.B.R., came to visit his "second scientific house" in Cambridge. He wanted to deliver a seminar on certain applications of the Lie–admissible generalization of Lie theory.

The Boston Area Physics Calendar (see Section 1.5, page 74 of this book, for a description) was run that year by the Department of Physics of Tufts University. I therefore wrote a letter to the Editor of the Calendar, Celia Mess at Tufts, on April 19, 1982, (p. I–189), well in advance for the listing of G.G.G.'s seminar scheduled for April 30, under the (studiously innocuous) title of "Algebraic identities, vector fields, and coordinate changes".

TO MY ENOURMOUS SURPRISE, TUFTS UNIVERSITY REFUSED TO LIST G.G.G.'S SEMINAR! I heard this first from Celia Mess when phoning on April 20 to verify that

everything was in order. It was not. I was told to contact the chairman of Tufts' physics department, Jack Schneps, which I did immediately. Schneps openly told me that:

- the prohibition to list G.G.G.'s seminar had been specifically voiced by the chairman of the Lyman Laboratory of Physics, Karl Strauch, and other senior physicists there (S. R. Coleman, S. L. Glashow and apparently others);*

- the prohibition would persist for all other seminars of our Institute, irrespective of their authors and irrespective of the wording of the announcement; and,

- the prohibition would persist until lifted by the Lyman Laboratory of Physics.

Numerous things happened after that. First, the fellow taxpayer can understand G.G.G.'s rage. I do not know what he did, nor did I ask to know, but we can expect that he did not remain inactive. Second, I immediately submitted a second request to list in the Calendar an I.B.R. seminar. The request was mailed this time via certified letter, return receipt requested. I was the speaker now for a talk under the title "Experimental and theoretical reasons why I do not believe in quarks".☆ I was evidently expecting the rejection of the listing. In fact, Tufts University rejected this second listing too. I gained, in this way, an unequivocal confirmation of the refusal to list I.B.R. seminars even when of strictly theoretical character. Thirdly, I wrote a confidential memo to selected members of the I.B.R. Evidently, I had to inform them of the "iron curtain" the Lyman Laboratory was apparently committed to build around its neighboring, independent, much younger, institution.

A number of possible actions were considered to bring the physicists at Lyman to scientific reason, ranging from the disclosure of the occurrences to the international press, to the filing of (duly publicized) law suits. Nevertheless, the I.B.R. decided to do nothing in the hope that time would bring to reason the senior physicists at Harvard.

*Weinberg at that time had left Harvard for the University of Texas at Austin.

☆For physicists who are aware of my research, this title is referred to quarks conceived as elementary particles, as conjectured at Lyman during that period. The paper underlying the proposed talk is that distributed in 15,000 copies, and subsequently published in Found. of Phys., ref. [49]. As indicated in Section 1.6, the conjecture that quarks are truly elementary has been lately abandoned, and it is not considered viable any more, although ref. [49] has never been quoted in the orthodox literature on quarks at Lyman and elsewhere (see Section 1.6, pp. 132—140 for details).

The recent rejection by the Boston College to list an I.B.R. seminar by H. Yilmaz on the inconsistencies of Einstein's general theory of relativity (Section 1.5, pp. 74—77) confirmed the continuation of the problem in 1984.

The writing of IL GRANDE GRIDO was then unavoidable.

Epilogue

I must express my gratitude to Harvard University for the hospitality that, despite all, was provided to me in 1977—1980. In fact, a number of scientific initiatives I undertook during that period could materialize because I was at Harvard.

I would like also to express my respect and consideration for Harvard University which is and remains one of the most prestigious academic institutions throughout the World.

Nevertheless, my dedication and commitment to America are much bigger than my sentiments toward Harvard. I therefore feel obliged to express my disagreement with Derek C. Bok, President of Harvard University, on grounds of scientific ethics.

During the last decades, Harvard University has used large amounts of public money in mathematical, theoretical and experimental research in particle physics under the assumption of the exact validity of Einstein's special relativity. Once doubts on such exact validity under specific physical conditions are voiced in refereed journals, as they have been, and brought to the direct attention of the university administrators, as done repeatedly, those administrators have the ethical duty to promote active research on campus on the resolutions of the doubts either in favor or against established Einsteinian doctrines, the understanding being that such resolutions must also occur via articles published in refereed journals (rather than talks in university corridors).

The existence of such an ethical duty for Harvard is manifest and incontrovertible. In fact, to this day (June 18, 1984), Harvard could be continuing research under governmental contracts for which Einstein's special relativity is violated, with consequential risk of misusing public funds. Until Harvard uses university money ONLY, outsiders do not necessarily have the right to pass judgment on university decisions. However, the moment Harvard uses one penny of public money, outside taxpayers such as myself or my neighbor, have the right to pass judgment on the ethical soundness of university decisions, and voice their concern as effectively as possible.

S. R. Coleman, G. B. Field, R. Giacconi, S. L. Glashow, P. Martin, C. Rubbia, K. Strauch, M. Tinkham, S. Weinberg, F. L. Whipple and other physicists and astrophysicists at Harvard University have accumulated throughout the years a sizable PERSONAL problem of scientific accountability vis—a—vis the U.S. taxpayer, for conducting or otherwise supporting research

under Governmental contracts crucially dependent on the exact validity of Einstein's special and general relativities, or part of them, under physical conditions for which numerous, at times historical doubts have been voiced and published in the technical literature, and without the appropriate quotation of the dissident views.

Again, as stressed earlier, physicists and astrophysicists at Harvard have the right to believe in the exact validity of Einstein's theories under unlimited physical conditions, but they have the ethical duty, first, to quote dissident views, and, second, to support the resolution of the problem, whether in favor or against their personal opinions and interests, whenever operating under support from the U.S. taxpayer. The numerous episodes reported in this book and in the related documentation, indicate beyond a reasonable doubt the opposition by senior members of Harvard University against such resolution, while the lack of quotation of dissident views on Einstenian ideas by Harvard's papers can be readily verified in research libraries.

Furthermore, the backing provided by Derek C. Bok, President of Harvard University, and/or his administration, to the senior physicists and astrophysicists, or the mere lack of interest on the issue, has propagated the ethical problems, from individuals, to Harvard University AS AN INSTITUTION. The size of the public funds involved, the duration in time of the episodes, the international academic weight of the campus, and other factors indicate beyond a reasonable doubt that Harvard University cannot suppress research on the insufficiency and possible invalidation of Einstein's theories without infriging fundamental codes of scientific ethics, and, at the extreme, without putting the premises for a potential, future, threat to National Security, particularly in case the action is done in support of vested, academic–financial–ethnic interests of individuals or of organized groups of individuals at Harvard, in disrespect of the interests of America.

It should be stressed that my personal contributions are insignificant here. There are many physicists more qualified than myself to conduct a better job on dissident research on Einstein's theories. The point is that by backing the senior physicists at Lyman, and by permitting the suppression of my feeble voice, Bok has endorsed the suppression of dissident research at Harvard thus creating the university problem of scientific ethics indicated earlier. In fact, after I left that campus, no paper explicitly treating the possible invalidation of Einstein's theories has been published under Harvard's affiliation (evidence to the contrary would be appreciated).

But there is more. The international academic power of Harvard University is well known to outsiders and certainly well known to its president. By merely tolerating the actions

perpetrated by Coleman, Glashow, Weinberg and other physicists against myself and my associates during our efforts to identify the limits of applicability of Einstein's theories, Derek C. Bok has created the potential prerequisites for a scientific obscurantism in physics, based on the suppression of dissident views on Einstein's theories via academic power, rather than papers in technical journals.

In fact, the mere tolerance of the actions by the university president and/or his administration following my detailed reports, rather than containing has multiplied the confidence and impunity in questionable behaviour, by reaching extremes such as the direct interventions to suppress the listings throughout the years of dissident I.B.R. seminars on the (seemingly democratic) Boston Area Physics Calendar. The possible premises for a scientific obscurantism then become plausible for anybody who is really aware of the international academic power of senior faculty at Harvard.

This is a true, ultimate reason for my writing this book. In fact, until the opposition by Coleman, Glashow, Weinberg and others against my dissident research remained contained at Harvard, I did carefully avoid any release of the information outside the Yard. The propagation of the opposition to outside peers in the U.S.A. and abroad (see the remaining presentation) indicated to me the possible initiation of a scientific obscurantism on Einstein's ideas. The writing of this book was then rendered absolutely unavoidable.

Even ignoring the evident, fundamental character of the scientific issues, there are military aspects (touched in Section 1.6 evidently without any detail) that simply cannot be treated too lightly. **Hadrons are the biggest energy reservoir known to mankind. The possible invalidation and generalization of Einstein's ideas in their interior may permit the conception of new weapons which are simply unthinkable under Einstenian laws. The risk that such weapons might be conceived first by enemies of America must be prevented. This should indicate the reasons why the backing of vested, academic—financial—ethnic interests at Harvard University on Einstein's theories not only would be antiscientific and in violation of scientific ethics, but could constitute a potential threat to the free world.**

But . . . . my personal opinion on these matters is insignificant. Equally insignificant is the personal opinion by Derek C. Bok and other members of Harvard University. The only important opinion is that by the taxpayer who supports the research at Harvard.

Fellow taxpayer, the passing of judgment on the matters is therefore released to you. For that, I beg you not to be blinded by the notorious brillance of Harvard's parlance. As recalled in Section 1.4, physics is a science that will never admit terminal theories. No matter how good Einstein's theories are

today, one day they will be replaced by more general and more accurate descriptions. The sooner these generalized theories are achieved, the better it is for America and mankind.

2.2: MASSACHUSETTS INSTITUTE OF TECHNOLOGY

The primary reason of scientific dispute with colleagues at Harvard was the exact or approximate character of Einstein's special relativity in the interior of hadrons. The primary reason of scientific dispute with colleagues at the Massachusetts Institute of Technology (MIT) was the exact or only approximate character of a central part of the special relativity: the symmetry under rotations.

For a better understanding of this section, it is useful to review the following scientific aspects considered in Chapter 1.

1.) Victor F. Weisskopf, a senior physicist at MIT, was one of the first scholars to acknowledge in his book [2] of 1952 the hypothesis formulated in the early stages of nuclear physics according to which the intrinsic magnetic moments of protons and neutrons could experience a deviation from their conventional values, when the particles are within a nuclear structure.

2.) After being ignored for decades, studies of the hypothesis were resumed in 1978. It was then understood that the alteration (called "mutation") of the intrinsic magnetic moments of protons, neutrons and all hadrons under strong interactions is expected to be a consequence of the deformation of the extended charge distributions of the particles. In turn, such deformation implies a breaking of the (conventional) rotational symmetry (one can think of a sphere which, because of collisions or external forces, is no longer spherical and, therefore, no longer rotationally invariant; see Figure 2.2.1). It was furthermore understood that the maximal conceivable conditions of mutation of intrinsic magnetic moments (rotational asymmetry) were expected to be due to the alteration of the intrinsic angular momentum (spin) in the conditions considered (sufficiently energetic hadrons under EXTERNAL STRONG interactions). In turn, the alteration of spin under these extreme conditions would imply the alteration of the statistical character of the particles. Thus, Bosons or Fermions were not expected to remain exact Bosons or Fermions, respectively, under the extreme physical conditions considered, and Pauli's exclusion principle (a pillar of quantum mechanics) was not expected to be exactly valid [14].

3.) In summer 1981, it became known that, for sufficiently low energies, the alteration of the magnetic moments could occur under deformation of shape/rotational asymmetry, but in such a way to preserve the conventional values of spin and, therefore, of Pauli's exclusion principle [65]. These were evidently some intermediary conditions prior to the more general deformation/rotational–asymmetry AND mutation of spin of point 2.)

4.) The Austrian experimentalist H. Rauch and his collaborators had been conducting, since 1975, direct experimental tests of the intrinsic magnetic moment/rotational symmetry of (low energy) thermal neutron [96–99]. In 1981, Rauch announced re–elaborations of preceding tests indicating a possible 1% mutation/rotational–asymmetry exactly along points 1.) and 3.) (but not necessarily 2.). Rauch announced his measures at an international conference in Orléans, France, of 1981 [100], and subsequently confirmed the same measures at an international workshop in Tokyo, Japan, in 1983 [139]. To this writing, these measures remain the ONLY available DIRECT measures on the rotational symmetry.

5.) In the same contribution [100], Rauch indicated the experimental plausibility of sufficiently small deviations from Pauli's exclusion principle for sufficiently energetic neutrons colliding with the tritium core.

To this writing (June 19, 1984), the problem of the rotational symmetry is still fundamentally open on theoretical and experimental grounds. In fact, the resolution of the problem needs considerable, additional, theoretical study, as well as a sufficient number of diversified experiments, such as those identified in Section 1.7. Most importantly, the fellow taxpayer should keep in mind the current orthodox position according to which Pauli's principle is exact under strong interactions. This conclusion, however, is supported by data elaborations of experiments which are based on the assumption of the exact validity of the principle. To prevent turning nuclear physics into a farce (Section 1.9, pp. 178–180), current experiments on neutron–tritium scattering should also be re–elaborated under the assumption of a (generally small) violation of the principle. The two different elaborations should then be confronted, and the differences resolved via specific experiments. Most of all, to understand the content of this section, the taxpayer should keep in mind that the possible extablishing of the breaking of the rotational symmetry in physics (whether only for conditions 3.) above or for the full conditions 2.), would imply the irreconcilable invalidation of Einstein's special relativity (Section 1.4).

The beginning of my contacts at MIT.

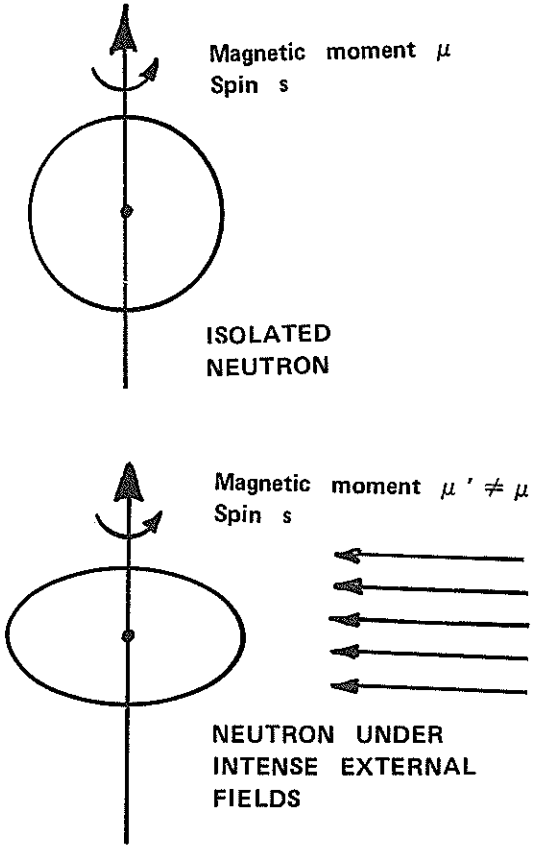


Figure 2.2.1. A schematic view of the primary reasons of dispute with senior physicists at MIT. Protons and neutrons are not point–like particles, but possess an extended charge distribution with a radius of about 10^{-13}cm. Assume for implicity that such distribution is perfectly spherical and therefore rotationally invariant (an assumption that is already debatable to begin with). Under sufficiently intense external forces and/or collisions, protons and neutrons are then expected to experience a deformation of shape, trivially, because perfectly rigid objects do not exist in the physical reality. The amount of deformation for given external conditions is unknown at this time. But the existence of the deformation itself is out of the question on strict physical grounds (although not on grounds of academic politics!). The deformation of shape has a number of scientific, economic and military implications. First, it implies an alteration of the intrinsic magnetic moments of the particles, as it can be inferred from mere classical considerations. In turn, the alteration of the magnetic moments has important implications for controlled fusion (e.g., for magnetic confinement) trivially, because the value of the intrinsic magnetic moments of the particles to be confined magnetically may change pre-

cisely at the time of the initiation of the fusion process. Second, the deformation of shape of protons, neutrons and all hadrons implies a breaking of the symmetry under rotations, trivially, because the particles are no longer rotationally invariant. In turn, such breaking implies that of Einstein's special relativity (see Sections 1.4 and 1.6). The political implications for vested, academic–financial–ethnic interests on Einstein's theories at MIT and other campuses are simply unavoidable for the problem under consideration. The possible military implications cannot evidently be detailed here. The fellow taxpayer should however know that, if the intrinsic characteristics of protons and neutrons change when the particles are in the interior of nuclei, improvements of existing weapons or even new weapons could become conceivable. At any rate, these possibilities simply cannot be dismissed too lightly. Despite: the manifest plausibility of the deformation, the availability at MIT of all equipment for speedy experimental resolutions (see below), and the scientific–economic–military implications, senior MIT physicists showed no interest in the problem. In fact, this section is a report of my repetitious attempts to suggest an active involvement by MIT, which were followed by equally repetitious dismissals over several years. The fellow taxpayer should be aware of the "rebuffal" often voiced by academicians in the hope of by–passing the deformation/rotational–asymmetry/violation–of–the–special–relativity depicted in this figure. The argument goes by saying that protons, neutrons, and all hadrons are made of quarks which are point–like and therefore fully invariant under rotations. Stated in different terms, the argument attempts to recover the exact rotational symmetry and the exact validity of the special relativity, by performing the transition from a proton as a whole, to its constituents. The theoretical plausibility of the argument cannot be denied by a true physicist. Nevertheless, the use of the argument for the purpose of suppressing the need for the experimental resolution of the problems considered, is so questionable, to raise a host of issues of scientific ethics. Quark theories are still conjectural to this writing for a variety of reasons, such as the fact that the quarks themselves have never been isolated and physically detected in a direct way; the achievement of a model of true confinement of quarks is still lacking; etc. (see the end of Section 1.7). **The physical phenomena under consideration here (deformation of shape; alteration of magnetic moment; breaking of the rotational symmetry; etc.) are referred to and must be referred to one proton or one hadron as a whole, irrespective of what the constituents are. It is then the task of any structure model to recover these data on the particle. For these and other reasons, the mere mention of the word "quarks" can be potentially unethical, particularly when used for the intent of voiding the experimental resolution of the deformation of shape of hadrons, with related breaking of Einstein's special relativity.**

As predictable, my contacts at MIT initiated under the best possible auspices and mutual respect. I had been an (unsalaried) personal guest of Francis E. Low at the MIT Center for Theoretical Physics from January, 1976, until August, 1977 (while jointly holding a salaried faculty position elsewhere). During that period, I wrote the preliminary drafts of monographs [9, 10] papers [125], and the preliminary versions of a number of other works. To have an idea of how smooth my con-

tacts at MIT were at that time, I reproduce below the referee report of papers [135] published in Annals of Physics (which is a journal edited by MIT faculty) (see Doc. p. I—680)

> *"Santilli has performed a real service in reviewing beautiful old ideas and extending them to field theories. Such scholarly virture is rare these days and is very important".*

At the termination of my stay, I left MIT for Harvard sincerely grateful to Francis E. Low, then Director of the Center for Theoretical Physics, Herman Feshback, then Chairman of the Physics Department, and several other colleagues.

But . . . . . in all my scientific activities at MIT of that time, I had carefully avoided the mentioning of doubts on the possible invalidation of the rotational symmetry and Einstein's special relativity in particle physics.

The founding of the Hadronic Journal.

On October 20, 1977, I submitted to Annals of Physics five papers on the need to test rotational symmetry and Pauli's exclusion principle under strong interactions (p. I—681). In the subsequent correspondence with H. Feshback, as Chief Editor of the journal, I pointed out the immaturity of the papers and my need for help. Unfortunately, months and months passed without any editorial decision. In fact, the papers were formally rejected only on May 22, 1978 (p. I—685), and it was only after several subsequent requests, that I finally succeeded in having copy of at least part of one referee report (p. I—687—688).

Verbal communications in the meantime gave me the clear impression that senior physicists at MIT were not interested in the experimental verification of Pauli's exclusion principle in nuclear physics, despite its evident fundamental character, not only for basic knowledge, but also for energy related issues (see the implications for controlled fusion of the possible alteration of the magenetic moments of hadrons of Section 1.1, pp. 8—10), not to ignore for military profiles.

The delay at MIT in the consideration of the papers was determinant in my decision to found a new journal with a specific emphasis on the publication of plausible conjectures expressed in a theoretically and mathematically mature way, irrespective of their implications for academic politics. In fact, my search for funds to initiate production of the Hadronic Journal began exactly at that time. The submission of the papers on the tests of Pauli's principle to Physical Review D (Particles and Fields) had to be excluded owing to the notorious attitude of that journal against the publication of speculative ideas (see Section 2.4).

My plea to H. Feshback, F. E. Low, P. Morrison, V. F. Weisskopf, and other senior MIT physicists to conduct the tests at MIT on Pauli's exlusion principle under strong interactions.

I spent the entire day of October 10, 1979, at my typewriter writing individualized letters to Feshback, Low, Morrison, Weisskopf and other senior physicists at MIT, each letter being several pages long (pp. I–213–243). As one can see, the letters pointed right to the heart of the scientific issue. For instance, after seven pages of presentation, the letter to Weisskopf concluded by saying (p. I–232)

> *"I am appealing to you for support in my proposal to Philip Morrison and other friends at MIT to initiate studies at MIT in the experimental verification of Pauli's principle in nuclear physics".*

As recalled earlier, Weisskopf had been among the first to acknowledge the hypothesis of the possible alteration of the intrinsic magnetic moments. I therefore thought that he would be interested in the experimental resolution of this historical open problem. Also, I thought that everybody could see evident physical aspects such as: (a) the plausibility of the deformation of the extended charge distributions of protons and neutrons under sufficiently intense external forces and/or collisions (recall that absolutely rigid objects do not exist in the universe!); (b) the consequential alteration of the intrinsic magnetic moments exactly as predicted by the historical hypothesis; and (c) the equally evident breaking of the rotational symmetry. I thought that Victor Weisskopf and the other senior physicists at MIT would see these things, and initiate an active scientific role or at least be receptive.

But I was wrong.

No acknowledgment of my proposal of October 10, 1979, was ever voiced to me verbally or in writing by any of the senior members I had contacted. The only comment that unidentified MIT physicists made later on to DOE was that "Santilli writes long letters".

The availability at MIT of the equipment for a speedy experimental resolution of the issue.

The fellow taxpayer should know the background reasons for my writing several "long letters" to MIT physicists. In 1979, MIT possessed capabilities to conduct the suggested tests in house. By 1979, I had become acquainted with the experiments conducted by H. Rauch and his team on the rotational symmetry of neutrons via interferometric techniques [95–99]. I had also

become aware of the fact that all the interferometric equipments (perfect crystals, detectors, etc.) used by Rauch were already available at the MIT nuclear physics laboratories. MIT therefore had the capability to repeat Rauch's tests on the rotational symmetry under strong nuclear interactions in about two months running time; all this, if and only if desired or otherwise permitted by the senior physicists there.

But . . . . . , an MIT acknowledgment of the need to test the rotational symmetry would have implied the official acknowledgment of the existence of authoritative doubts on Einstein's special relativity. In turn, the mere acknowledgment of doubts would have been manifestly damaging to the large interests surrounding Einsteinian theories at MIT, throughout the U.S.A. and abroad.

The MIT declination of my proposal was therefore consequential, no matter how plausible the violation is, and no matter how important the implications are.

The visit at MIT to inspect the equipment.

On March 19, 1980, I visited the neutron interferometry facilities at MIT with H.H.H., a European scholar then visiting me at Harvard. The head of the neutron interferometric experiments, Clifford G. Shull, was in Europe. His junior collaborators, J. Arthur, D. K. Atwood, and M. A. Horne were there. They received us quite cordially, by showing the experimental facilities; by providing a detailed presentation of the experimental set ups; and by outlining experiments running there at that time.

After completing the tour of the facilities, we had a meeting in which H.H.H. and I proposed to Arthur, Atwood and Horne the conduction of the experimental test of Pauli's exclusion principle. The subsequent day, I summarized the proposal in a letter (p. I–251) also including a list of references on the proposal (pp. I–252–253).

H.H.H. and I came out of this visit with the confirmation of the conviction that MIT had already in house all that was needed to resolve experimentally the historical hypothesis of the possible alteration of the intrinsic magnetic moments of protons and neutrons under nuclear conditions. H.H.H. was, of course, aware of the solicitations I had made to senior physicists at MIT to conduct these evidently fundamental tests. He was also fully aware of the implications for controlled fusion. I still remember H.H.H.'s surprise to see that so eminent physicists were not interested in testing the rotational symmetry despite all these aspects. It was in this way that H.H.H. reached, in his own independent way, the conclusion that the lack of interest at MIT in the tests was due to academic politics. I still remember my uneasiness with H.H.H., and my worrying of the

comments that this fellow scholar would have expressed on MIT when back to Europe.

The appeal to C. G. Shull.

In my view, the implications of the case were too serious to be left only at the level of junior experimentalists at MIT. On August 27, 1980, I therefore wrote a personal appeal to the senior physicist in charge of the neutron interferometric experiments, Clifford G. Shull (p. I–259–260). For clarity, the full letter is reproduced below.

Dear Professor Shull,
On March 19, 1980, during your leave, I visited your associates M. A. HORNE, D. K. ATWOOD, and J. ARTHUR for the purpose of indicating that your neutron interferometer equipment appears to be particularly suited for the experimental verification of the SU(2)–spin symmetry as well as of Pauli's exclusion principle under strong interactions. Copy of the correspondence with Mike Horne is enclosed.
I am referring, for instance, to suitable modifications and/or improvements of the initial tests on the 4π spinor symmetry already done by the European experimental group headed by Professor RAUCH (a copy of his last paper on the subject is enclosed).
On experimental grounds, the need for additional measurements are numerous. For instance, (1) the exact symmetry value of 720° barely makes it within experimental data (716.8 ± 3.8 deg); (2) the median angle in the latest as well as in the preceding experiments has a tendency to be below 720 deg; and (3) the best fit does not appear to be provided by a sinusoidal curve, as necessary for the exact symmetry (see the diagram of fig. 3 of Rauch's paper, p. 284).
On theoretical grounds, the need for additional measurements are equally numerous, and they have been discussed in detail in the specialized literature on the topic (see the enclosed list of references, copies of which were released to your associates). In its most rudimentary form a primary argument is as follows. For the case of the electromagnetic interactions, the exact validity of the SU(2)–spin symmetry is incontrovertible, as established (for instance) by the property that the angular momentum of a charged particle under an external elm field is conserved. For the case of the strong interactions the situation does not appear to be necessarily the same. As clearly indicated by available experimental data, strongly interacting particles are actually constituted by wave packets in condition of mutual penetration or overlapping (which is absent for the elm case, in general). This confirms the rather old expectation that one component of the strong interactions is constituted by a nonlocal, nonpotential (non–Hamiltonian) force. In turn, this

is expected to imply the lack of applicability in an exact form of the entire Lie's theory, let alone that of the SU(2)–spin case. Irrespective from this aspect (or as a complement to it), the angular momentum of a particle under strong interactions is not expected to be conserved (to avoid the perpetual–motion–type of approximation that, say, a proton orbits inside a star with a conserved angular momentum. . . .). In turn, this is expected to imply a form of breaking of the SU(2) symmetry. Needless to say, such a possible breaking can be only an internal effect of closed strong systems and, as such, not observable via external elm interactions. Also, for the case of the nuclear forces the effect can at most be quite small.

These ideas have been subjected to a quantitative study by a number of mathematicians and physicists via the so–called Lie–admissible generalization of Lie's theory. In essence, the approach studies the generalization of the Lie algebra/enveloping algebra/Lie group in such a way to permit the representation of nonpotential forces.

Also, the approach is applicable to the quantitative treatment of a broken Lie symmetry, and admits the conventional Lie theory as a particular case. The application of these new mathematical tools to the case of a strongly interacting particle under condition of penetration with other particles and expected nonlocal forces has provided: (A) the prediction of a conceivable deviation from the exact SU(2) symmetry of the order of at least 5×10^{-4} for the case of low energy nuclear processes; (B) the apparent interpretation of the "slow down effect" of the median angle; and (C) the apparent improvement of the fit of the experimental data by Rauch and his collaborators.

In conclusion, and to our best understanding at this time, the current experimental data appear to be compatible with both the exact and the broken SU(2) spin symmetry. The fundamental character of the symmetry for theoretical as well as applied physics (e.g., the problem of the controlled fusion) then warrants, in my view, additional experiments.

Since the time of my visit to your laboratory, several developments have occurred, such as

- *a number of experimentalists have answered my call for the initiation of a feasibility study for more refined experiments;*
- *I have delivered an invited talk at the recent Conference in Differential Geometry and Applied Mathematics held from July 23 to 25 at Clausthal-Zellerfeld with encouraging results; and,*
- *we recently had our Third Workshop in Lie–admissible Formulations here in the Boston area from August 4 to 9 with the participation of some 30 scientists, including mathematicians, theoretical and experimental physicsts. The workshop was virtually devoted to the study of the*

problem.
In case you are interested in more detailed information, I would be happy to visit you either for an informal meeting or for delivering a seminar on the subject (I could essentially repeat my presentation at Clausthal–Zellerfeld). I can be reached more readily at my home address given below.
Best Personal Regards, *cc: Professor FRANCIS E. LOW, MIT*
Ruggero Maria Santilli *encls.*
RMS/ml

Shull never acknowledged this appeal, by remaining totally silent with me, despite the explicitly stated offer to meet "informally", that is, to avoid any official announcement by MIT of our possible meeting.

What a difference between the real MIT, and the MIT I had imagined as the temple of pursuit of novel scientific knowledge, while being a high school student thousands of miles away!

The firm continuation of support by DOE after my leaving Harvard University.

The fellow taxpayer will recall that my status at Harvard had to be "totally ceased" (In Hironaka's words) on the night of May 31, 1980. I knew this end well in advance and, therefore, I initiated, in time, all the necessary action.

It was at this point that the Division of High Energy Physics of the U.S. Department of Energy gave concrete proof of determination (at that time) to continue the support of my research irrespective of academic dances that might occur at local institutions. Also, the way DOE conducted the case, and the informal support I received were such that I felt proud of being the father of American children.

In short, by late 1979, I knew that the opposition at Harvard against my research would readily propagate to other campuses, by therefore preventing any realistic possibility of continuing the administration of my DOE contract by an academic institution. I therefore contacted the DOE in Washington asking for the administration by a non–academic corporation. This proposal was accepted by DOE upon due consideration, scrutiny and qualification of the corporation as the administrative conduit of federal contracts.

It is regrettable that such a beautiful independence of the DOE Division of High Energy Physics from high ranking U.S. physicists was short lived. In fact, the DOE subsequently had to succumb to the mounting of pressures intended to suppress the funding of my research. Ironically, this subsequent truncation of support occurred exactly at the time of conclusion of the classical research and initiation of specific studies in particle physics, not excluding military profiles.

The offer of guest status by Gian–Carlo Rota at the MIT Center for Applied Mathematics.

Once I had achieved the removal of the administration of my DOE contract from the academic world, I thought that my problems were indeed finally over, and that I could finally plunge myself into the study of basic experiments without wasting unnecessary human energies in mumbo–jumbo academic dances.

BUT, AGAIN, I WAS WRONG!

One technical aspect of my new DOE application, I knew well since late 1979, was that, even though the administration was of non–academic type, I still needed an academic institution to conduct my work because of the need of library and other research facilities.

For this reason, on January 9, 1980, I wrote to Gian–Carlo Rota, a senior mathematician at MIT, asking for hospitality under my own, independently administered, DOE contract (p. I–248). I specifically indicated in this letter that any possible visiting status would be formally included in my grant application to the DOE (see the last lines of p. I–248).

On January 18, 1980, Rota kindly answered with a formal offer of a guest status for the academic year 1980/1981. In this way, the DOE approved a new research contract (DE–AC02–80ER10651) under a number of provisions, including the administration by the corporate, non–academic, conduit AND my guest status at the Center for Applied Mathematics at MIT.

The printing of the cover of the Hadronic Journal of June, 1980, with my MIT affiliation.

Journals must meet certain production deadlines. To do so, it is a rather frequent practice to print in advance the cover, and then the contents itself. The Hadronic Journal is a bimonthly journal and, as editor, I must confirm or otherwise modify my affiliation and full address for the cover of the journal at least every two months. The last issue with my Harvard affiliation was that of April, 1980. The subsequent issue of June, 1980, had to carry a different affiliation owing to the termination of my status there on the night of May 31.

In early May, 1980, the printer contacted me requesting the affiliation and address for the cover of the June issue. Always suspicious of political maneuvrings, and despite having a written authorization, I phoned Louis Howard, in his capacity of Director of the Center for Applied Mathematics at MIT. G. –C. Rota had previously informed him of all details. He therefore was fully aware of my imminent guest status. I explained to Howard the advance printing of the cover of the Hadronic Journal, and asked for the confirmation of the authorization to disclose the MIT affiliation in my editorial address, which

he gladly did.

There were considerable financial matters involved in the printing of the cover. I was not satisfied with the additional phone authorization I received from Howard. I therefore wrote him a detailed letter summarizing our phone conversation (p. I–254) and again asking for an immediate communication in case of any objection. No objection was raised. On May 18, I therefore authorized the printing of the cover of the Hadronic Journal of the June, 1980, issue and of the additional issues of the academic year 1980/1981.

The revocation of the guest status by the MIT Center of Applied Mathematics on the day of initiation of the visit.

TO MY ENOURMOUS SURPRISE, ON JUNE 1, 1980, JUST AFTER HAVING LEFT HARVARD AND WHILE PREPARING TO GO TO MIT, I RECEIVED A LETTER FROM L. N. HOWARD REVOCATING MY GUEST STATUS AND PROHIBITING THE INDICATION OF ANY MIT AFFILIATION IN MY EDITORIAL ADDRESS (p. I–255)!!!

The letter is evidently the result of what is sadly known as "MIT politics". It uses academic parlance deprived of any contents, while avoiding the disclosure of the real issues. For instance, Howard cites the lack of office space as a reason for the decision, while I had stated, restated, and repeated again that I did not need an office. I only needed the use of the libraries and an academic address.

Why this sudden change? Why had MIT done this in full knowledge that the guest status was part of an official document with the U.S. Government? Why had MIT done this despite the full awareness of the fact that the June, 1980, issue of the Hadronic Journal had already been printed with my MIT affiliation? Which was the force behind the decision? Was it due to isolated individuals or to organized academic–financial–ethnic interests in the Cambridge area?

The most plausible answer is rather simple. I had kept silence on my guest status at MIT; I had asked DOE to keep the information as confidential as possible (by going as far as asking for the courtesy of NOT submitting my application for review in the Boston area), and I have reason to believe that the confidentiality was indeed kept by DOE; Rota, apparently, also kept the information to himself; and Howard did not apparently inform his colleagues of the occurrence. When the time of the initiation of my visit arrived, the information had to be communicated to MIT mathematicians. We must then expect that the information propagated rapidly to the physics department at MIT and/or to Harvard's mathematics and physics departments. Under these circumstances, the gathering of vested,

academic–financial–ethnic interests in the Cantabridgian academic community to suppress my guest status at MIT would have been an extremely easy task covered by total impunity.

Whatever the truth, the fact remains that an incontrovertible, drastic change occurred in a matter of days, from a very nice, friendly and cordial attitude by L. Howard toward me up to the end of May, to the suddenly rigid position of suppressing the visit at whatever cost. It is evident that: (a) Howard did not revoke the guest status by acting alone; (b) the decision must have been the result of a sufficient quorum at MIT; and (c) the diversification and amount of pressures on Howard to suppress my visit must have been proportional to the implications.

I then visited Howard in his office for the purpose of identifying as clearly as possible the financial implications of the revocation. I told Howard that, not only the corporation producing the Hadronic Journal had to destroy the covers of the journal, but my DOE application, even though approved, might well be revoked because based on the assumption of MIT providing the needed use of research facilities. I furthermore indicated the rapidly increasing interest in the studies of the Lie–admissible generalization of Lie theory, by pointing out the gain for his center in adding this line of inquiry. I finally asked him authorization to stay there at least a minimum time for my securing another guest status elsewhere. As a gesture of courtesy, I gave Howard a complimentary copy of my monograph with Springer–Verlag with a dedication.

Howard kept mostly silent during my presentation; he accepted the gift of my monograph; and answered my last question with the confirmation that I was absolutely prohibited to initiate my visit there.

My plea to Francis E. Low, then Provost of MIT.

I could readily foresee the subsequent events. In fact, under the circumstances, the corporation producing the Hadronic Journal would have been forced to file a law suit for damages against the Massachusetts Institute of Technology. Additional law suits against MIT could also be anticipated in case anything would have goon wrong with the DOE contract.

At that time, I was still sincerely interested in avoiding gestures that could damage local institutions. I therefore called Francis E. Low, then MIT Provost, by reporting to him the case at least in a summary way (as I attempted to enter into details, Low would remind me that he was very busy). I then asked Low to intervene, in order to prevent a completely unnecessary crisis.

Apparently, Low did intervene in this particular instance. On June 13, 1980, L. N. Howard wrote me a letter confirming

the original authorization to print the June issue of the Hadronic Journal with my MIT affiliation, but he kept silent on the guest status, thus implying that his preceding letter on the matter was still standing, that is, I would be prevented from being formally authorirized to use the MIT libraries and other facilities essential for the actuation of my research under the DOE contract.

The founding of the Institute for Basic Research.

After the episode of the guest status at MIT, I resolved myself to organize a new research center under the name of THE INSTITUTE FOR BASIC RESEARCH. In fact, while waiting for the initiation of the new DOE contract (which occurred in the subsequent month of September, 1980), I worked virtually full time on the organizational preliminaries (raising of the necessary seed money; charter; operations; etc.). The Institute was incorporated on March 2, 1981, as an academic non—profit institution; a building adjacent to Harvard University, the Prescott House, was purchased on July 29, 1981, to provide permanent housing for the Institute in the heart of the Cambridge academic community; and the official ceremony of inauguration occurred on August 3, 1981 (see Appendix B).

To understand the decision, the fellow taxpayer must know that MIT was not the only U.S. institution to have rejected hospitality to me. In fact, several other colleges had formally declined a temporary guest status with all expenses supported by my DOE contract. This is the case, for instance, of the Department of Physics of Tufts University (p. I—188), the University of Rochester,* and others.

In addition, a number of colleges had rejected my request of administration of the DOE contract. This is the case, for instance, of the Department of Physics of Virginia Polytechnic Institute & State University (p. I—302). As an incidental note, a detailed letter written to R. E. Marshak at the physics department there (to inform him of the status of the studies on the fundamental tests) remained completely unacknowledged, without even a word of thanks for the gift of my monographs accompanying the letter.☆

But the Virginia Polytechnic Institute at least had acknowledged my application and indicated the negative decision!

*Regrettably, the documentation of the Rochester case was misplaced and could not be found at the time of the release of this book for printing. Note that I am referring to declination of guest status made following the declination of an academic position by both Tufts and Rochester.

☆R. E. Marshak subsequently became the President of the American Physical Society for 1982-1983. I then absteined from communicating to him, in his capacity as APS president, additional evidence on the need to verify Einstein's special relativity in the interior of hadrons, because an expected, total waste of time without scientific feedback.

Other U.S. institutions did not even bother to communicate the negative decision. This is the case of the Department of Physics of the University of California at Berkeley, which was formally considering me for a faculty position, but which never acknowledged its evident negative outcome (p. I–310–332); or the Institute for Theoretical Physics of the University of California at Santa Barbara (p. I–303–309) where I was formally considered for a position, and which had received a rather considerable amount of (free) scientific material, including volumes of proceedings of our conferences!

Still other U.S. institutions did not even bother to acknowledge my application, despite the amount of appended material. This is the case, for instance, of the Nuclear Science Division of the Lawrence Berkeley Laboratory, in Berkeley, California. In fact: a formal letter of application for a position there to its Director, Bernard G. Harvey, dated October 10, 1979, (I–334); a subsequent letter to the Director of the Physics Division of the same laboratory, Robert W. Birge, dated October 22, 1979, (I–339); and subsequent letters of January 9 and 30, 1979, (I–346–348); they ALL remained totally unacknowledged! It is impossible for me not to think that the reason for this rather unusual and uncollegial behaviour was due to the fact that I had applied, specifically, to study the test of Pauli's principle under strong interactions, as clearly stated beginning from the very first pages of my application. Yet, while I was predicting opposition by members of the laboratory against the experimental verification of Pauli's principle, I still cannot figure out how so many individual letters and thousands of pages of scientific material could remain totally unacknowledged!*

*This lack of acknowledgment of my job applications propagated to other academic activities, including my formal invitations to U.S. physicists for a variety of functions. As a result of this experience, I now issue invitations to U.S. physicists only under truly exceptional circumstances for the simple reason that the greatest majority of the invitations remain unacknowledged. I see no point to present here a list of documented cases. The following one, however, is particular, and must be brought to the attention of the taxpayer as an example of current professional custom in U.S. physics. In mid 1981, Howard Georgi had to leave the post of editor of the Hadronic Journal for a number of reasons, including the fact that he had been promoted to a tenured position at Harvard University. I therefore initiated the search for a colleague from the U.S.A. sufficiently qualified to substitute Georgi as editor of the Journal. After due search, and a number of consultations with physicists from different ethnic groups, I issued a formal invitation to Sidney Meshkov of the National Bureau of Standards in Washington, D.C. The letter, dated July 4, 1981 (pp. I–416–418), invited Meshkov to consider the post of editor of a journal whose Editorial Council comprised distinguished scientists (including two Nobel Laureates). As one can see, the invitation was written in a most respectful form. Time passed and Meshkov did not acknowledge the invitation. We subsequently reached the time of the inauguration of our new Institute in Cambridge (which would have housed part of the editorial activities of the journal). I therefore mailed

I hope the fellow taxpayer understands why, whether right or wrong, I had the feeling that the opposition against the experimental verification of Einstein's special relativity in the interior of hadrons I experienced at Harvard University, after having been backed up by MIT members, had propagated throughout the U.S.A.

The founding of a new, INDEPENDENT, institute of research was then the only possibility left for the continuation of the studies in the U.S.A. by our group.

The MIT refusal to participate in the experimental test of the rotational symmetry via a joint Austria–France–U.S.A. collaboration.

When MIT turned down my appeal to repeat Rauch's tests on the rotational symmetry under external nuclear interactions, I was evidently left with no other choice than contact Rauch himself. I thought that MIT was not interested in doing the experiment in house, but would have no objections in others doing the

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an additional invitation to Meshkov for participation at the inauguration ceremony of the I.B.R. (p. I–419). But . . ., months and months went by, the I.B.R. was inaugurated, and no acknowledgment whatsoever was received by Meshkov. I therefore attempted to contact common friends in the hope of soliciting any resolution. The fellow taxpayer should know the common practice of scientific ethics according to which, when one individual physicist is invited to become the editor of a scientific journal, no additional invitation must be issued to other physicists for the same post. Ethical standards demand that you simply wait for that physicist to consider the invitation and communicate his/her decision. Additional invitations should then be issued only after declination of the original invitation. Sidney Meshkov, being a senior physicist at a U.S. National Laboratory, knows these things well or, at any rate, he must be expected to know them well because of his post. According to established ethical standards, Meshkov should have communicated his lack of interest with a simple note of declination, thus permitting the continuation of the search with other physicists. In fact, because of the lack of answer by Meshkov, the search for the editor of the Hadronic Journal had to be delayed for over half a year, thus creating predictable scientific damages. The lack of acknowledgment by Meshkov evidently created a host of unanswered questions. After all, invitations for an editorial post of the type I issued in writing (with total and independent editorial authority) are not received every day. But then, why did Meshkov have to damage the Journal? Was he acting for himself, or was he acting on behalf of his peer group? Was the unusual uncollegiality of Meshkov's behaviour due to personal reasons, or was it due to the primary objectives of the journal explicitly recalled in my letter (THE PROMOTION OF THE EXPERIMENTAL RESOLUTION OF THE VALIDITY OR INVALIDITY OF EINSTEIN'S SPECIAL RELATIVITY UNDER STRONG INTERACTIONS)? Nobody will ever know the TRUE answers to these and many more questions. One visible consequence however occurred. The Meshkov case occurred after a number of similar ones in the U.S. physics community. Therefore, subsequent invitations had to be issued to foreign physicists.

experiment elsewhere.

Again, I was wrong! The story of Rauch's experiment is reviewed in detail in Section 2.5 because of its rather crucial scientific, economic and military implications. In this section, I want to report only the following episode.

As a true scientist and a gentleman, Rauch accepted immediately my appeal for the continuation of the experiments, and offered a mutual collaboration between his Atominstitut and the IBR. I therefore proposed to Rauch to apply for partial support at the Division of Nuclear Physics of the National Science Foundation and the U.S. Department of Energy. He stressed the need of minimal funds because the experimental apparatus had already been constructed, while the essential personnel was under employment either of the Atominstitut, in Wien, Austria, or of the Institute Laue—Langevin in Grenoble, France (which provided the nuclear reactor). Nevertheless, he gladly accepted my recommendation. We therefore prepared a proposal for a joint Austria—France—USA collaboration to be submitted to NSF and DOE for partial funding.

To my extreme dismay, I subsequently learned that A. Zeilinger, one of Rauch's collaborators for the experiments on the rotational symmetry, and a proposed co—investigator of the grant application to U.S. Governmental Agencies, HAD LEFT WIEN TO SPEND ONE YEAR AT THE MIT NUCLEAR PHYSICS DIVISION, AND, IN PARTICULAR, TO WORK WITH SHULL'S INTERFEROMETRIC GROUP!!! As soon as I was informed of this, I called Rauch and attempted to convey the idea that it would be better to remove Zeilinger's name as a co—investigator of the application, because, in my expectation, his MIT affiliation could create unnecessary problems. I stressed that this administrative change would leave the scientific profile completely unaltered, including Zeilinger's participation in the new tests. But Rauch, in his kindness and unawareness (at that time) of the Cantabridgean academic politics, dismissed my view as excessively pessimistic, and insisted that Zeilinger should deserve a chance. Evidently, I could not insist. As IBR president, I therefore provided my full services to the experimental team for the completion of the application.

By mid 1981, the application had been completed under the title, "Experimental verification of the SU(2)—spin symmetry under strong and electromagnetic interactions by a joint Austria—France—U.S.A. collaboration". The application was signed in two continents, including administrative formalities in three Countries, and mailed to Zeilinger at MIT for the last missing signature, his.

By keeping in mind all the preceding episodes, the fellow taxpayer can now predict what happened. Nothing happened. That is, MIT did nothing, and released no information whatsoever, whether or not Zeilinger would be permitted to sign the front page of the application under his MIT affiliation, despite

the numerous signatures already there (p. I–263)! Months passed by and no information could be obtained from MIT, whether verbal or in writing. I had a meeting with Zeilinger at the IBR on the matter, which resulted to be fruitless. A subsequent formal letter I wrote to Zeilinger at MIT on October 29, 1981, with copy to C. G. Shull and H. Feshback (p. I–268–269) soliciting "any" decision, whether favorable or unfavorable, was left unacknowledged.

In this way, several months passed by with the application sitting on my desk, without being able to submit it to NSF and DOE because of the lack of Zeilinger's signature. It was only ONE YEAR LATER that Rauch finally acknowledged my original prediction to be verified by the reality of the events. We then prepared a new application by repeating again the entire administrative iterim in two continents, but this time WITHOUT Zeilinger as co–investigator. In this way, the application was finally submitted in Washington with over one year of delay.

But . . . ., as the fellow taxpayer can readily anticipate, the application was rejected (Section 2.5).

Zeilinger's seminar at MIT on the experimental tests of the rotational symmetry and other laws.

In the third week of November, 1981, the Boston Area Physics Calendar brought the information that A. Zeilinger would deliver a seminar at MIT on neutron interferometry experiments (which, as the fellow taxpayer will remember from Section 1.7, are precisely the experiments used by Rauch, Zeilinger himself, and others to test the rotational symmetry [96–99] and other basic, quantum mechanical laws).

At that time, I had already made a formal commitment with myself NOT TO ATTEND ANY SEMINAR AT ACADEMIC INSTITUTIONS OF THE BOSTON AREA, evidently because of the formal prohibition by these institutions to list IBR seminars. In the case of Zeilinger's seminar, the need for my absteining was even more compelling. In fact:

- on one side, I expected Zeilinger to be silent on the recent experimental data [100] and theoretical studies [65] indicating the plausibility of about 1% breakdown of the rotational symmetry; and,

- on the other side, under these premises, it would have been necessary for me to disrupt the seminar in a way as forceful as possible.

For these reasons, I asked the courtesy of a number of other physicists attending the seminar (and familiar with the scientific issues) to report to me the essential elements of Zeilinger's presentation. This was indeed done by a number of friends, in-

cluding members of the IBR, such as I.I.I., a European scholar.

The reports I received in the evening of the seminar (November 18, 1981) confirmed the most pessimistic of my predictions. In fact, Zeilinger had essentially told a rather numerous audience (for which the use of a larger lecture hall had been necessary) that everything was fine with the rotational symmetry, as well as with other quantum mechanical laws. In particular, Zeilinger had absteined from quoting the new experimental data [100] from his boss at the Atominstitut in Wien, and the theoretical studies [65] from his senior colleague at the same institution, not even as a marginal, incidental, curiosity! Note that Zeilinger's awareness of these publications at the time of his seminar was absolutely unquestionable, not only because the papers had been mailed to him from Wien, but also because they were an essential part of the research grant application he had not signed.

I hope the fellow taxpayer begins to consider a bit more seriously my fear of a scientific obscurantism potentially on the way in U.S. physics due to vested, academic—financial—ethnic interests. In fact, what Zeilinger had done is a genuine act of scientific obscurantism under the formal backing of the Massachusetts Institute of Technology!*

The additional seminar at MIT on the rotational symmetry by L. Grodzins.

The Boston Area Physics Calendar of the same week had also announced a seminar by the senior MIT physicist L. Grodzins on the "Measurement of magnetic moments of high spin rotational state". Again, I could not attend any seminar at MIT because of my selfcommitment. Nevertheless, I was interested in listening to the impression by friends and IBR members who attended the meeting.

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*The climax of the irony was subsequently reached in 1983 when A. Zeilinger, C. G. Shull et al. from MIT presented a paper at the *International Symposium on the Foundations of Quantum Mechanics* held in Tokyo, Japan, under the seemingly illuminated title of "Search for unorthodox phenomena by neutron interference experiments" (see p. 289 of the Proceedings edited by S. Kamefuchi and printed by the Japanese Physical Society). Despite the illusory title, the paper carefully avoids the problem of the fundamental test of the rotational symmetry. The fellow taxpayer should keep in mind that the crucial measures (715.87 ± 3.8 deg) on the LACK of achievement of the 720 deg needed for the establishing of the rotational symmetry via neutron interferometry, originally presented by H. Rauch at our First International Conference in Orléans, France, of 1981 [100, 126], had been represented by Rauch at the same Symposium in Tokyo a short time before the exploit by Zeilinger, Shull et al. And in fact, one can read in the proceedings of the same symposium the value 715.87 ± 3.8 deg on page 281, only eight pages before the Zeilinger—Shull contribution.

I still remember I.I.I. returning from this seminar full of scientific excitement because the experimental results presented by Grodzins appeared to be out of the predictions of conventional quantum mechanics and, in particular, of the rotational symmetry. I.I.I. had warned me that, under questions posed by MIT colleagues, Grodzins had made all conceivable efforts to indicate the possibility of reconciling his measurements with the exact rotational symmetry.

Nevertheless, the data persisted. Evidently, Grodzins views on the compatibility of his measures with orthodox doctrines could not be dismissed. The point is that, on similar scientific gounds, one could not dismiss the interpretation of the same data via the VIOLATION of the rotational symmetry (Section 1.6 and 1.7). Furthermore, Grodzins tests could shed light on the historical hypothesis of the possible alteration of the magnetic moments embraced by V. F. Weisskopf at MIT in 1952.

I.I.I. and other colleagues therefore urged me to contact Grodzins. Even though highly skeptical on any scientific outcome because of the evident affiliations, I did contact Grodzins via a letter dated November 30, 1981, (p. I–272), indicating the great similarities of his experiments with Rauch's measures [100]. In fact, both Grodzins and Rauch had performed measures directly related to magnetic moments although for different cases (one for high and the other for low spin values). Also, in both cases the agreement with orthodox predictions was too dubious to be fully convincing. Thus, Grodzins' measures could be a back–up of Rauch's measures and vice versa.

L. Grodzins answered on December 4, 1981, with a few, dry, scientifically uncooperative lines, indicating that *"there is no connection between these studies [his] and those by Professor Rauch on the test of the spinor symmetry of neutrons via neutron interferometers. I regret that members of your institute who heard my talk came away with the wrong impression."* (p. I–273).

On December 9, 1981, I answered Grodzins with one of the scientifically most dissonant letters I have ever written (p. I–272).

The last little academic dance.

Despite everything that had happened, in early 1983 I was still willing to keep some form of contact with the Cantabridgean academic community. After all, I was the president of a growing institute of research, as well as the editor of a scientific journal, and an active researcher.

On February 5, 1983, a paper authored by two scholars from a far away Country was submitted to me for publication in the Hadronic Journal. The paper developed research originally conducted by V. F. Weisskopf and his associates at MIT,

which were indeed quoted first. I therefore submitted the paper to Weissopf for refereeing, with a respectful letter (p. I–275) recommending him to provide a "generous refereeing", of course, not in the sense of scientific leniency but in scientific help and assistance, as requested by our journal. After all, the authors belonged to an important foreign Country; they had worked hard on the subject; and, in the final analysis, the research was on Weisskopf's own topics.

On February 23, 1983, I received the following answer (p. I–276)

Dear Professor Santilli:

Professor Weisskopf asked me to look at the manuscript you recently asked him to referee. It appears to me, from the cover letter accompanying the manuscript, that the authors have not submitted the paper for publication but merely sent your institute a copy of one of their preprints.

Sincerely,

Rober L. Jaffe

To understand this letter, the fellow taxpayer must know that IT IS ABSOLUTELY UNCOSTUMARY TO PROVIDE REFEREES WITH COPIES OF THE LETTERS OF SUBMISSION unless containing useful technical information. The editor merely mails to the selected referee one copy of the paper and the request for refereeing. I know this practice well. Jaffe, being a senior MIT physicist, must also know this practice well. At any rate, only the paper and the letter of request of review had been mailed to Weisskopf. But then, how could Jaffe possibly conclude that the paper had not been submitted to the Hadronic Journal?*

The most plausible answer is therefore that Jaffe's letter was an "MIT parlance" to indicate lack of willingness to review the paper, even though the paper was in their own area of vested interests (imagine what would have been the case if I had mailed a paper to MIT for review on the possible violation of Pauli's principle . . .).

In this way I reached the conclusion that CONTACTS WITH LEADING PHYSICISTS AT LEADING U.S. INSTITUTIONS ARE NOWADAY GENERALLY DAMAGING, UNLESS ONE HAS A HISTORY OF SUBSERVIENCE TO THE CURRENT, VESTED, ACADEMIC–FINANCIAL–ETHNIC INTERESTS, IN WHICH CASE CONTACTS CAN BE AT BEST HOPED TO BE INNOCUOUS.

This admittedly sad conclusion is reached not only for individual physicists scattered throughout the world, but primarily for officers of the American Physical Society, members of U.S. Governmental Agencies and U.S. politicians (see Chapter 3),

*I personally did not even bother to answer, but simply asked the staff of the Hadronic Journal to mail Jaffe a copy of the formal letter of submission (p. I–277). I have erased the names of the authors in the Documentation to avoid a political and scientific incident.

in the latter case, the manifestation of the expected damage can be less obvious and may take considerable more time.

The hope of this book is that of promoting the return to the only way of doing physics, the traditional way of free, dispassionate communications and contacts among free physicists. But this can only be hoped following a public denounciation of the current situation and its independent appraisal by the taxpayer.

Epilogue.

I would like to express my gratitude to the Massachusetts Institute of Technology for the hospitality granted me from January, 1976, until August, 1977, which was one of the most enjoyable academic periods of my life.

I would like also to confirm my respect and consideration for MIT which is and remains one of the most prestigious academic institutions throughout the world.

Nevertheless, as it was the case for Harvard, my commitment and decication to America and to the advancement of physical knowledge are or otherwise must be greater than my sentiments toward MIT. I therefore feel obliged to express my disagreement on grounds of scientific ethnics with Francis E. Low, Herman Feshback, Victor F. Weisskopf, Philip Morrison, Arthur K. Kerman, Clifford G. Shull, Lee Grodzins, and other MIT physicists.

The Massachusetts Institute of Technology is one of the largest, private, nuclear physics laboratories in the U.S.A. and, as such, it has used during the last decades large amounts of public funds in nuclear research, estimated in the range of billions of dollars.

These large public funds have been spent and are continued to be spent under the assumption of basic quantum mechanical laws which have been experimentally established under electromagnetic interactions, but whose validity in the interior of nuclei is only conjectural at this time.

As a result of this situation, the Massachusetts Institute of Technology has an unquestionable ethical duty to conduct an active role in the direct experimental verification of the validity or invalidity of conventional quantum mechanical laws AND relativities under open—external, strong, nuclear, interactions.

In the final analysis, as stressed in my correspondence with individual MIT physicists, the objective IS NOT that of verifying the violation of the laws. Not at all. The objective is that of establishing the laws in a quantitative experimental way, irrespective of whether valid or invalid. As a consequence, the recommended experiments may well confirm the validity of orthodox laws. Oppositions to this type of experiments cannot, therefore, avoid the raising of ethical issues.

Physics is a science with an absolute standard of values:

the experimental verification. Until physical laws are established beyond any reasonable doubt by direct experiments (rather than indirect information only), those laws are and must be of conjectural value, no matter how important they are. The Massachusetts Institute of Technology simply cannot continue, nor can be permitted to continue in the use of large public funds whenever dependent on the exact validity of physical laws in the interior of nuclei that are merely conjectural at this time.

Also, experiments themselves have an absolute standard of values: the more fundamental the tests are, the higher their priority. This is due to the fact that basic experiments have much bigger scientific, administrative and ethical implications when compared to lesser relevant tests. By keeping these known values in mind, the fellow taxpayer is then recommended to tour the Massachusetts Institute of Technology. He will see a feverish experimental activity in a considerable variety of branches of physics. At times, the experiments attempt the achievement of new knowledge, but in the greatest majority of the cases, the experiments deal with refinements of existing knowledge. The value of experiments done or currently under way at MIT is unquestionable, and not the issue here. The fellow taxpayer is instead suggested to compare the experiments running at MIT and those on the basic physical laws, such as the test on the rotational symmetry or on Pauli's exclusion principle or on Einstein's special relativity (Section 1.7). Under all standards of true science (that is, excluding academic politics), it is evident that the importance of the tests of the basic physical laws is such to dwarf all other conventional experiments that can possibly be on at MIT. But then, this situation cannot but raise issues of scientific ethics.*

When, in addition to all that, fundamental tests create a manifestly large problem of scientific accountability vis–a–vis the taxpayer, and have potentially important scientific, economic and military implications, then one cannot but raise severe reservations on the vested, academic–financial–ethnic interests at MIT that have prevented the conduction of the tests until now.

No American resident or citizen can consider him/herself a truly free and responsible member of this society, unless he/she has the courage to denounce publicly the situation, once aware of it, and participate in its public scrutiny.

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* Academicians are known to be capable of masterpieces in the adulteration of facts. I would like here to recall a crucial scientific profile, from Sections 1.6 and 1.7, according to which the true experimental tests of basic laws demands open strong conditions, such as measures on ONE hadron under EXTERNAL strong interactions, exactly as it was done to establish the same laws under electromagnetic interactions. Thus, if the fellow taxpayer is approached by an academician with a river of evidence on the validity of conventional laws for a closed–isolated strong system, academic mumbo–jumbo or scientific corruption should be suspected.

Unfortunately, MIT has inflicted on itself, as well as on the U.S. physics community, considerable scientific damage. All the various episodes reported in this book (and more) are well known to several academic circles in the U.S.A. and abroad. They were known long before the appearance of this book, which has merely brought the episodes to the attention of the taxpayer. As a consequence of this situation, I do not know whether it is appropriate for MIT to initiate, at this time, tests of basic physical laws and relativities. As an internationally known physicist told me: *"I will not believe in possible experiments at MIT on the rotational and other basic symmetries* **even if claiming violation"** [emphasis mine].

What is therefore needed for MIT and other important academic institutions and national laboratories in the U.S. is, first of all, to regain the confidence by independent observers on the implementation of strict codes of scientific ethics via concrete, visible, public actions (such as the firing of members, irrespective of their seniority, rank and ethnic affiliations, in case caught with scientifically unethical behaviour). Only then, after regaining the ethical credibility, the tests of fundamental physical laws can be effectively conducted, and their results accepted by the national and international scientific community.

But . . . ., my personal opinion on the matters is insignificant. Equally insignificant is the personal opinion by F. E. Low, H. Feshback, V. F. Weisskopf and other physicists at MIT. The only important opinion is that of the taxpayer supporting MIT research.

The passing of judgment on the matters is therefore released to you, fellow taxpayer. In this function, I beg you not to be blinded by the renouned MIT authority. Perfectly rigid objects can only exist as a figment of the academic imagination, but not in the physical reality. Once you see this, the violation of the rotational symmetry for protons, neutrons and other hadrons deformed by sufficiently intense external forces and/or collisions is incontrovertible. The amount of violation for given physical conditions and the appropriate generalized theory are evidently debatable at this time. But the existence in nuclear physics of the violation of the rotational symmetry is absolutely out of the question, no matter what MIT physicists may say. At any rate, the only available direct measures are those by Rauch (715.87 ± 3.8 deg) and they DO NOT recover the angle (720 deg) needed for the exact rotational symmetry [100, 137]. This is the physical reality as it stands now, fellow taxpayer. The rest is nothing but MIT politics.

2.3: U. S. NATIONAL LABORATORIES.

When the opposition and/or lack of interest on fundamental tests at Harvard and MIT became clear, I had no other alternative but to contact U.S. National Laboratories. The objective was to solicit the initiation of experimental studies on the exact or approximate character (validity or invalidity) of Einstein's special relativity and other physical laws in the interior of strongly interacting particles, or under other suitable conditions.

This action was reason for considerable, additional disappointment to me. I thought that, because of their evident need for clear accountability vis—a—vis the taxpayer, U.S. National Laboratories would be more receptive than private colleges.

Again, I was wrong.

National Laboratories emerged from these contacts, at least in my eyes, as being without proper scientific light, and being instead subservient to vested, academic—financial—ethnic interests at Harvard University, the Massachusetts Institute of Technology, Yale University, and other leading colleges in the U.S.A.

The gravity of the scientific scene at U. S. National Laboratories.

Numerous experimental verifications of fundamental laws are possible today at National Laboratories (Section 1.7). For the sake of this section, it is sufficient to recall only one case, that of the measures of the mean life of unstable hadrons (pions, kaons, etc.) at different energies (Section 1.4, 1.6 and 1.7). If Einstein's special relativity is exactly valid in the interior of these particles, their mean life should behave with energy as predicted by Einsteinian laws. On the contrary, if internal deviations from the special relativity exist, they are expected to manifest themselves via deviations from Einsteinian laws on the behaviour of the mean life.

A number of historical, authoritative voices of doubts have been voiced throughout this century on the plausibility of internal deviations. The argument is based on the expected nonlocality of the strong forces due to mutual wave overlappings of the particle constituents (Section 1.7).

After a number of attempts, initial, quantitative predictions of violation began to appear in the 70's. Even though not necessarily correct, the predictions were nevertheless specific, quantitative and numerical. As an example, paper [101] by the Canadian physicist D. Y. Kim predicted 14.3% deviation from the Einsteinian law for composite particles at 400 GeV. These

predictions have been lately superseded by more accurate predictions, such as those of ref.s [35, 36].*

On experimental grounds, measures of the mean life of unstable hadrons were conducted soon after the discovery of the particles. These measures, however, generally refer to the particles at rest, or at one value of the energy. To reach the experimental information useful for the problem considered, we need the measure of the mean life of at least one hadron (and not a lepton such as the muon) for at least two different values of energies. The understanding is that the experimental resolution of the issue one way or the other will demand measures conducted for a comprehensive range of energies and for a variety of particles.

Now:

- despite the existence of historical voices of doubts;
- despite the availability of specific predictions of violation;
- despite solicitations independently made by a number of scholars;
- despite the feasibility of the experiments;
- despite the ready availability of all the necessary equipment;
- despite their low cost when compared to less relevant experiments done and/or currently under way;

the needed measures of the mean life of unstable hadrons at different energies HAVE NOT BEEN DONE IN U.S. (AND FOREIGN☆) LABORATORIES TO THIS WRITING (June 20, 1984).

The task of passing judgment by the fellow taxpayer now becomes much more complex. In fact, from the judgment of potential insufficiencies in scientific accountabilities by individual U.S. physicists and/or institutions, the task is now shifted to a much more serious subject: the conceivable existence of a conspiracy in U.S. physics perpetrated by vested, academic—financial—ethnic interests to prevent the experimental resolution of

*The fellow taxpayer should recall from Chapter 1 that the possible internal deviations have been proved to be compatible with the exact validity of the special relativity for the dynamical evolution of the center of mass of the particles. Stated differently, the well known exact validity of the special relativity for the motion, say, of a pion in a particle accelerator constitutes no evidence whatsoever, not even indirect, on the validity of the same relativity for the interior dynamics, which could therefore follow structurally more general laws. This occurrence can be inferred from a mere observation of our physical reality. For instance, the validity of Galilei's relativity for the dynamical evolution of the center—of—mass of our Earth in the solar system is fully compatible with the manifest violation of the same relativity for interior trajectories, such as satellites during re—entry, damped spinning tops, etc. (Sections 1.3 and 1.4).

☆See Appendix A for the situation at CERN, Geneva, Switzerland.

the validity or invalidity of Einstein's special relativity in the physical reality.

I present below my case with the understanding that it is not unique.

My first appeal to Wolfgang K. H. Panofsky, then Director of the Stanford Linear Accelerator Center (SLAC).

When I arrived at Harvard in September, 1977, one of the first preprints that caught my eyes was paper [101] by Kim. The preprint had been written at SLAC, while Kim was spending a leave from Canada. When, in 1978, I realized the opposition and/or lack of interest in the Cantabridgean physics community on the tests of the special relativity, I wrote a long, passionate appeal to W. K. H. Panofsky to initiate active experimental studies of the problem at SLAC. The letter (seven pages long with numerous scientific enclosures) was mailed on July 19, 1978, (p. I–360).

On July 27, 1978, I received a letter from Panofsky (p. I–373) which, even though courteous, was scientifically vacuous in my view. Panofsky essentially qualified my recommendations to conduct basic experiments with the judgment that I *"profoundly misinterpreted both the experimental status of elementary particle physics and the methods of conducting experimental investigations".* My specific reference to Kim's paper written at his laboratory; my insistence on the evident, primary relevance of these tests over the experiments then going on at SLAC; etc.; all these appeals resulted to be useless. THE NEEDED EXPERIMENTS WERE NOT CONSIDERED THEN, THE SUBSEQUENT YEAR, AND THE YEAR AFTER THAT, NOR ARE THEY GOING ON THERE NOW.

The true understanding of the passionate character of my letter to Panofsky demands the knowledge of the fact that, at the time of drafting and re–drafting the letter in early July, 1978, I was unemployed since the preceding month of September, 1977, while being the recipient of a DOE contract, and while being prohibited to draw my salary from my own grant by senior Harvard physicists.

My first appeal to R. R. Wilson, then Director of the Fermi National Acceleration Laboratory (FERMILAB).

Essentially the same letter and enclosures mailed to Panofsky on July 19, 1978, were also mailed to Wilson at FERMILAB jointly with additional material and letters to the theoretical division of the laboratory.

Wilson answered on September 27, 1978, (p. I–382) informing me that he was no longer the director of FERMILAB (a position assumed by L. M. Lederman), and that *"there seems*

to be little point in trying". . ."to answer the questions you have raised". The remaining part of Wilson's letter dealt with the following, admittedly harsh criticisms of FERMILAB'S theoretical division I had candidly voiced to U. D. I. Abarbanel there, in a letter of July 19, 1978, (p. I—371)

"I do feel obliged to clearly and openly express my utmost concern on the current conduction, operation and policy of the Theoretical Division of FERMILAB. I believe that this division is:

- *monopolistic, in the sense that it has only conducted research based on the conjecture that quarks are the constituents of hadrons;*
- *unbalanced, because of the literal lack of diversification of studies on the fundamental problem of contemporary physics; and,*
- *of marginal effectiveness, in the sense that the virtual entire theoretical production on the problem of hadron structure conducted in this division in recent times is devoted to minute aspects along mere opinions by groups of physicists, without any direct consideration of truly fundamental physical problems.*

For more details on my view, you may consult my recent letter to Professor WILSON, copy of which is enclosed."

To understand these words, one must keep in mind that FERMILAB carries the name of Enrico Fermi. The lab therefore had (and still has) a truly special meaning for me. I was sincerely interested in seeing FERMILAB remain as the forerunner of novel physical knowledge. My language was therefore studiously challenging and provocative in the hope of stimulating some suitable action, by therefore preventing the occurrences I was experiencing at Harvard at that time.

The best way for FERMILAB to remain the leading experimental laboratory in particle physics was given, in my view, by the inclusion of truly fundamental experimental tests, those of basic physical laws. I reasoned that, at that time (mid 1978), we had already discovered what is often called a "zoo" of particles (over one hundred of them). Besides the discovery of a few additional ones (such as the so—called W's and the Z^0), the push toward the discovery of new particles was loosing scientific interest . Whether sooner or later, the search for new particles had to leave the way for more fundamental inquiries. The test of basic quantum mechanical laws and relativities is of evident, much bigger scientific interest than the search of new particles, besides being of comparatively much less expensive.

At any rate, long before 1978, FERMILAB possessed in house all the necessary equipment for the resolution of the existence or lack of existence of deviations from the special relativity in the behaviour of the mean life of unstable mesons at different energy. How could FERMILAB possibly remain in-

sensitive to the experiment, particularly when taking into account the large scientific accountability vis—a—vis the taxpayer?

Above all, I was concerned for the freedom of scientific inquiry at FERMILAB and for its independence of scientific thought from vested interests at outside colleges. I saw such freedom and independence as prerequisites for genuinely novel achievements.

Wilson commented on my letter to Abarbanel by saying that (p. I—382)

"You do make some pretty harsh charges regarding our Theory Department. Generally speaking, we have tried to hire the best people available based on the advice of the best theorists in the country. A broad range of theorists come to visit Fermilab for various periods to supplement the efforts of the Fermilab theorists. Having done that, as Director, it would never occur to me to try to influence or restrict their work. Although the tragic death of Ben Lee set us back, I have been satisfied with and proud of our theoretical department."

This answer confirmed the worst of my fears. In fact, it confirmed that the hiring at FERMILAB was done on the advice of the "best theorists in the country", which is an euphemsim for leading representatives of current, vested, interests in physics. The lack of independence of thought and the subservience to said interests, was then a natural consequence, in my view. Needless to say, I did share in full, Wilson's reason of being proud for past achievements. But the reason for my concern was the future. There was no doubt in my mind that, if the control of FERMILAB by vested interests in primary academic institutions was permitted to propagate to the level of jobs, programming and scientific output, the laboratory would decay with the inevitable decay of the vested interested controlling it or not keep up with the pace of advances, IRRESPECTIVE OF THE AMOUNT OF PUBLIC FUNDS POURED INTO IT. To my sincere disappointment, time is apparently proving me right.

I feel obliged to present my apologies here to Wilson, Abarbanel, and other colleagues at FERMILAB. I would like to appeal to their understanding of the harshness that senior physicists at Harvard were forcing upon my children and my wife at the time of our correspondence. I also want to admit the insufficiencies and decifiencies of my presentation.

Nevertheless, Wilson, Abarbanel, and others at FERMILAB have apparently failed to understand my concern and, at any rate, they made no effort in trying to understand it.

One thing is certain. Exactly as it had occurred at SLAC, FERMILAB DID NOT INITIATE ACTIVE STUDIES OF THE EXPERIMENTAL VERIFICATION OF EINSTEIN'S SPECIAL RELATIVITY AND OTHER BASIC LAWS FOLLOWING MY APPEAL OF 1978, NOR DID THEY FOLLOWING THE SUBSEQUENT APPEALS BY MYSELF AND OTHERS.

The rather perfect alignment between SLAC and FERMILAB on one side, and the opposition I was experiencing at private colleges on the other side, creates the difficult task for the taxpayer indicated earlier: to ascertain whether or not we have been facing a conspiracy by vested interests to prevent the experimental verification of the special relativity in particle physics.

My first appeal to G. H. Vineyard, then Director of the Brookhaven National Laboratory.

The same letter of July 19, 1978, mailed to Panofsky at SLAC and Wilson at FERMILAB was mailed also to Vineyard at Brookhaven with additional material. I thought that most of the argumentations, particularly the moderate costs for fundamental tests could re—propel Brookhaven to the frontier of advances.

My appeal to Vineyard was perhaps even more pertinent than those to Wilson and Panofsky. In fact, Brookhaven was suffering from a comparative decay in scientific output and relevance, not only with respect to comparable foreign laboratories, but also with respect to other U.S. laboratories. Also, while SLAC and FERMILAB had been equipped with advanced machines, Brookhaven had been somewhat left behind in technological refurbishing.

As a result, SLAC and FERMILAB had, in 1978, a realistic possibility of remaining at the forefront of advances in particle physics via conventional tests (this possibility more lately proved to be erroneous). Brookhaven, however, was lacking even such a possibility evidently because of lack of the machines.

As a result of this situation, the ONLY possible rebirth I foresaw for Brookhaven National Laboratory was the return to the true values of physics: test the fundamental physical laws. In fact, the cost of basic experiments was minute when compared to those of others, while the scientific output could have been potentially substantial. It is appropriate to bring again to the taxpayer's attention the following facts regarding the test of the rotational symmetry via neutron interferometry [100, 139]. The experiment can be done with an amount of money of the order of $ 100,000, which is a fraction of the cost of experiments generally conducted in particle physics. On the other side, a confirmation of measures [100, 139] regarding the breaking of the rotational symmetry (with consequential breaking of the special relativity) would have scientific implications so vast to promote quite likely a new scientific renaissance (recall that a generalization of the rotational symmetry demands a corresponding generalization of the virtual entirety of contemporary physics).

Owing to these evident possibilities, and sincerely committed to provide my contribution for the future well being of the laboratory, I approached Vineyard with a scientific fervor even greater than that I felt for Wilson and Panofsky.

But . . . ., Vineyard did not acknowledge my appeal of 1978. The appeals I submitted the subsequent years, not only to Vineyard, but to each member of the executive staff of the laboratory also remained totally unacknowledged. In my eyes, this indicated only one thing: the lack of scientific courage to conduct fundamental experimental tests even if opposed by senior physicists at Harvard, MIT, and at other leading colleges. My dream of contributing to the initiation at Brookhaven of a scientific renaissance without large budgetary increases was doomed.

The second appeal to Panofsky at SLAC, Wilson at FERMILAB and Vineyard at BROOKHAVEN.

On May 7, 1979, I made a second appeal to Panofsky, Wilson and Vineyard in their capacity of directors of national laboratories, with particular reference to the following passage (pp. I–391–394)

"I would like to take the liberty of warmly encouraging again the initiation [at your laboratory] of studies on the experimental verification of the basic physical laws currently used in strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle. Even the activation of an initial feasibility study at your laboratory would be invaluable, provided that its conduction is not restricted to quark supporters only.

I am confident that you will see that the protraction of the current situation in hadron physics may invite a crisis. I am referring here to the current investments of truly large amounts of money on strong interactions, all based on the mere belief of the validity of the basic laws, without jointly conducting their experimental verification. Quite frankly, I am seriously concerned that the protraction of such a situation may imply a process to our scientific accountability.

I think that we still have time to prevent further deteriorations. But we simply cannot continue to effectively conduct studies in hadron physics on the basis of mere beliefs by individual physicists on fundamental issues. The return to the traditional conduction of physics, that via experiments, is, in my humble view, much needed and needed soon."

No acknowledgment was ever received from any of them. Sometime later, J. Ballam of SLAC resigned as a member of the Editorial Council of the Hadronic Journal (p. I–395).

The last appeal in 1981 to all officers of all U. S. National Laboratories.

On July 2, 1984, I mailed an additional, final appeal to all officers of SLAC, FERMILAB and BROOKHAVEN, as well as of: the Oak Ridge National Laboratory; the Lawrence Berkeley

National Laboratory; and the Los Alamos National Laboratories. The appeal (essentially the same with the same enclosures for all) included the passage (pp. I—398—415)

During the past years, I have contacted you at the rate of less than once per year to solicite the initiation at your laboratory of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

This is my letter of soliciation for 1981.

The appeal passed to a number of elaborations and information pertinent to the problem, and added:

"I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well known to be continued to be ignored by experimentalists in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences."

Panofsky answered on July 13, 1981, with the following letter (p. I—420)

Dear Professor Santilli:

Thank you very much for your letter of July 2 which you describe as the annual letter "to solicit the initiation at SLAC of experimental studies on the validity or invalidity for the strong interactions of . . ."

You correctly refer to the fact that the experimental information is still preliminary; in fact all experimental information is preliminary in the sense that it can and will be superceded by newer results. You also say "All data could be manipulated to force compatibility with conventional laws." Your principal proposal is that I should convene a meeting of leaders of our laboratory and in the field to consider experiments to specifically test your hypotheses.

Experiments are not conceived or designed in committee; rather, individual initiative arises from the scientific community and from that initiative results a proposal for a specific undertaking which appears technically feasible to the laboratory. The laboratory directors have little and should have little influence over this process. Therefore the only recourse you have is to

disseminate your theoretical deliberations to as wide an audience of experimentalists as possible in a manner such that they can extricate easily the experimental implications of the theory.
With best personal regards,
Wolfgang K. H. Panofsky

I answered by recalling that a number of specific, and clearly identified proposals were available in the literature and had been in fact brought to his attention before, such as the measures of: the mean life of mesons at different energies; the neutron–tritium scattering length; etc., (p. I–421). But my reply was evidently useless.

Leon M. Lederman, the new director of FERMILAB answered on July 28, 1981, with the following letter (p. I–422)
Dear Dr. Santilli:

Your letter of 2 July has raised procedural problems we have no way of addressing. This Laboratory provides facilities for carrying out experiments in High Energy Physics – orthodox or not – as long as the Physics Advisory Committee deems the proposal of sufficient scientific merit.

The main point is that this Laboratory does not do experiments. These are proposed to us by users groups at Harvard, Caltech, and some 100 institutions in the U.S. and abroad. We would be happy to receive unorthodox proposals for research to which we can react. We do not have any mechanism to set up committees to address the kind of tasks you outline. This would have to be done at your initiative outside of the activities of Fermilab.
Sincerely,
Leon M. Lederman

I answered on August 12, 1981, with the following comments (p. I–423)
Dear Dr. Lederman,

I would like to express my appreciation for your kind letter of July 28, 1981. However, permit me the liberty of expressing concern for its content.

Truly large financial and human resources have been spent through the years and are currently spent at FERMILAB in strong interactions, all under the assumption of the validity of conventinal laws, and despite the knowledge, repeated through the years, that possible modifications of the basic laws imply such technical consequences to result in different numbers for the same experiments. The seriousness of the problem is then self–evident.

On my part, I have simply accomplished the scientific duty of bringing to the attention of Fermilab (to Dr. Wilson first, and now to you) the existence of a rapidly growing community of scientists and observers calling for the experimental verification of the basic laws, irrespective of its result (whether in favor or against), as well as, perhaps equally importantly, the

achievement of a more balanced use of public funds.

My concern for FERMILAB has been increased considerably by your letter because Harvard, Caltech, and all the other academic Institutions you mention are not responsible for the situation. In fact, these institutions have good reasons to resist any intrusion in their own internal decisional processes. As a result, the entirety of the responsibility of the situation is viewed to rest on you, as well as all the other executives at FERMILAB and other national laboratories. The fact that, according to your letter, FERMILAB does not have mechanisms to set up committees of study, can aggravate the situation, but cannot eliminate your responsibility. To be specific, if fifty colleges propose independently exactly the same experiment, they infringe no rule. It is the responsibility of bodies such as FERMILAB to prevent that public funds are wasted by unnecessarily repeating the same experiment fifty times. If all the colleges affilated with FERMILAB abstein from proposing a needed experiment, they also violate no rule. In fact, if the experiment is needed to provide credibility to others, or for any other scientific reason, its promotion is expected from laboratories such as FERMILAB.

It is usually difficult to predict the future, and it is more so in this case. This means that everything may continue to function smoothly and orderly for years, or a serious crisis may be triggered a few months from now by malcontent or other unforeseeable reasons, particularly in this delicate moment of considerable scrutiny on the use of public funds.

The following point may serve as partial illustration of the interest at FERMILAB in the basic experiments. As everybody knows, FERMILAB is famous for the vastity of its research libraries, including subscriptions to all possible research journals in physics, whether from the U.S.A. or far away places. Despite that, FERMILAB has apparently avoided, for years, the subscription to journals known for their commitment to the promotion of fundamental tests and continues to do so to this day (see p. I–431).

Vineyard and all his executives at Brookhaven totally ignored my last appeal. There is no point therefore in adding further comment on that laboratory.

One point is crystal clear: my appeal resulted to be useless. The tests on Einstein's special relativity, Pauli's principle and other fundamental physical laws were not considered then, were not considered thereafter, and, to my best knowledge, are not running there now.

The appeal of 1981 to National Laboratories was my last. It had been mailed to over eighty officers of the indicated laboratories (their names are provided at the end of each letter on pp. I–398–415). The enclosures were more than sufficient to present the scientific case. I saw further appeals as merely

a waste of time and money. No additional appeal has therefore been submitted ever since.

Only a few, marginal episodes occurred thereafter. For instance, Ch. Prescott at SLAC and other physicists had released on October, 1981, a round table discussion entitled "Is spin physics worthwhile?" in which absolutely no mention was made of the experimental tests of the spin symmetry done by Rauch since 1975 or any other experiments that might indicate even minimally possible deviations from orthodox laws. I felt obliged to bring these tests to Prescott's attention as well as to the attention of the other co—authors of the report. After all, and contrary to their conclusion, spin physics could indeed provide truly fundamental advances. Prescott never acknowledged my letter, nor any of the other co—authors ever did.

Further contacts with individuals on specific issues at national laboratories also remained without acknowledgment (see, the case of the TACUP committee pp. I—436—442). The time for IL GRANDE GRIDO was therefore closing in.

The dangerous financial heading of national laboratories.

The failure of the efforts to stimulate a return to basic values in physics, has implied the continuation, completely unperturbed, of the lines preferred by vested interests in academia: the search for newer and newer particles.

But the accelerators currently available at FERMILAB, SLAC and other national laboratories are now essentially obsolete and unfit for the new tasks. As a consequence, the construction of new, truly large accelerators is under way.

As a physicist, I favor any physical advance, no matter how costly it is. But the size of the new accelerators (several miles) and their costs (billions of dollars) are so huge that it is time to compare the scientific output with the financial investments of public funds. In this sense, I cannot justify the expenditures of billions of taxpayers money at this time just to add, in case of luck, a few new particles to the large zoo of particles already discovered, particularly when truly fundamental questions on the already known particles remain ignored by the establishment in physics. Perhaps in the future, when the U.S. economy is such to permit a surplus of funds, at that time I would gladly support the expenditure.

My primary concern is of human nature originating from budgetarial considerations. The billions of dollars to be spent for the new machines will appear, on budgetary grounds, under the heading of physical research. Nevertheless, an unknown percentage of the funds will go to corporations outside the physics community. If the percentage of the funds leaving the physics community is sufficiently higher than the yearly budgetary increases allocated by Congress to physical research, the construction of the new machines will inevitably imply a reduc-

tion of funds to the physics community and, therefore, the loss of jobs by young and senior physicists.

This I cannot accept lightly. I must voice my opposition as effectively as I can. The scenario is now no longer that of greedy academic barons suffocating possible fundamental advances at birth to protect their interests. The ethical problems would be much much bigger than that, and proportional to the size of the expenditures under consideration, as well as to the human suffering because of the termination of jobs. Senior physicists at leading institutions cannot understand the latter point. Only physicists who have been unemployed with children to support can understand it.

It is a truly incredible story. What will future historians say about the scientific accountability of our society? What will happen in the U.S.A. if foreign laboratories establish the violation of Einstein's special relativity in the interior of hadrons? Will, under these circumstances, directors of national laboratories and their primary executives resign voluntarily from their posts? Or, under the circumstances indicated, will individuals have to initiate actions aiming at the identification of their responsibilities? And what about the responsibility of past presidents and officers?

The number of unanswered questions is endless. But the stakes are simply too high for America to treat them lightly. After all, we are facing a potential manipulation of fundamental human knowledge. As evident from this presentation, my repetitious appeals to executive officers of national laboratories resulted to be a failure on scientific grounds. Nevertheless, the appeals were successful in achieving one objective: to make absolutely sure that executive officers of national laboratories were fully informed of all possible scientific, financial and ethical implications of the case, in order to prevent even the most remote possibility of their saying:

"I did not know!"

Panofsky's last chance.

In March, 1983, the Boston Area Physics Calendar scheduled a talk by Panofsky on general aspects of experimental particle physics to be held at Harvard University. It was against my principles to attend any talk at Harvard for the reasons indicated earlier.

Yet, I wanted to meet Panofsky during his trip to Cambridge. I thought that, perhaps, by meeting each other and by talking to each other, we could reach some common grounds, or, in the absence of a scientifically valuable outcome, we could at least enjoy each others acquaintance.

For these reasons, I wrote Panofsky on March 1, 1983, inviting him for a meeting "possibly outside Harvard", "to exchange ideas on the orderly approach to the problem of the

experimental test at national laboratories of the Lorentz symmetry under strong interactions" (p. I—443).

Panofsky never replied. The writing of this book was therefore confirmed.

Epilogue.

Dear fellow taxpayer, I have expressed to you my judgment regarding the subservience of national laboratories to vested, academic—financial—ethnic interests at leading, outside, U.S. colleges. This subservience and the consequential lack of scientific freedom, have prevented the laboratories from considering the conduction of fundamental physical tests. In turn, this has created a rather massive problem of scientific accountability. In fact, the labs could be using hundreds of millions of dollars in experiments depending on the exact validity of Einstein's special relativity, under conditions for which the relativity is erroneous, thus implying a potential waste of large public sums. I have also expressed my judgment that the vested interests apparently responsible for this situation are so powerful, that no self—corrective measure is conceivable. The vested interests will continue to control national laboratories and they will continue to suppress all possible nonaligned experiments or scientific inquiries, unless . . .you intervene. I have finally expressed the opinion that, from the alignment of various national laboratories among themselves and their subservience to the outside academia, there are sufficient reasons to fear a conspiracy of national proportions perpetrated by leading physicists at leading U.S. colleges to prevent the tests of Einstein's special relativity and other basic laws.

Again, my personal opinions are insignificant. Equally insignificant are the opinions of the past and current directors of national laboratories and their staff. The only important opinion is yours, fellow taxpayer.

In considering the case, permit me to beg you to return to the true physical values: fundamental advances occur in a given society if and only if that society permits their attempts. If a society suffocates the consideration of the experimental verification of basic knowledge such as Einstein's special relativity because damaging to vested interests, that society could be doomed. The ONLY way to establish the special relativity is by verifying it directly, and then verifying it again and again, whenever the slightest doubt arises. When the taxpayer compares these evident physical values with the scientific scene, the emergence of substantial problems of scientific ethics in U.S. physics is simply inevitable. In fact:

THE ONLY DIRECT EXPERIMENTAL DATA CURRENTLY AVAILABLE ON EINSTEIN'S SPECIAL RELATIVITY IN THE INTERIOR OF HADRONS

SHOW CLEAR VIOLATIONS [35, 36]. LACKING THEIR DISPROOF, THIS IS THE ONLY PHYSICAL TRUTH AT THIS MOMENT. THE REST IS MUMBO–JUMBO SCIENTIFIC GREED OF POTENTIALLY SINISTER IMPLICATIONS FOR AMERICA AND MANKIND.

2.4: JOURNALS OF THE AMERICAN PHYSICAL SOCIETY.

Voltaire taught us to risk our lives so that dissident views can appear in print. I believe that the journals of the American Physical Society (APS) are a long, long way away from this illuminated intellectual democracy. I should indicate from the outset that all my comments and personal experiences refer specifically to APS journals dealing with nuclear and particle physics, such as Physical Review Letters, Physical Review D (Particles and Fields) and Physical Review C (Nuclear Physics). Nevertheless the mounting chorus of protests one can read in Physics Today, Science, and other general scientific publications concerning other cases (evidently not reported here), provides sufficient confidence to extend the main problematic aspects to all APS journals. In fact, the situation has reached such a point that attentive observers can readily find quotations of the following type in TECHNICAL papers published in non–APS, REFEREED journals: "This paper was rejected by Phys. Rev. . ."; or "After . . . .months, it had been impossible to resolve the publication of this paper in Phys. Rev. letters"; or "Paper . . .[published in an APS journal] had no sufficient novelty to appear in that journal"; etc.

Statement of the problem.

APS journals have acquired an international reputation of being against the historical way of pursuing NOVEL physical knowledge. I am referring to:

- the publication of plausible, sufficiently well presented CONJECTURES, irrespective of whether aligned or not with predominant lines of inquiries;
- followed by their critical examination by independent scholars, also via published articles.

APS journals are today generally considered the journals most unsuited for the submission of fundamental, potentially new ideas.

Publication of a paper in APS journals is today generally considered to be a qualification of the aligned character of the paper and/or of the author(s) with vested interests in U.S. physics but not necessarily a qualification of physical novelty.

In short, I believe that the publications of the APS are the ultimate and most visible illustration of the totalitarian condition of the current, U.S. physics community. Apparently, I am far from being alone in this view . . . . .

I should stress that the concern of APS journals is not new. It has been voiced and re–voiced numerous times by several scholars and, as such, it is known in academic circles. Only the fellow taxpayer had been kept uninformed until now. That is why this book was conceived and written.

The dimension of the problem.

The fellow taxpayer should know that the problem is of such a magnitude that, nowadays, entire new branches of physics are born or are at the threshold of birth WITHOUT ONE SINGLE PAPER APPEARING IN APS journals.

Again, I shall abstein from reporting experiences by others and restrict the presentation only to personal cases. The first documented case is the birth of a new classical mechanics called, for historical reasons, "the Birkhoffian mechanics". The fellow taxpayer will recall from Chapter 1 that the systems of our Newtonian environment had been traditionally represented via a mechanics known under the name of "Hamiltonian mechanics". This mechanics is certainly effective for planetary motions and other systems with conservative forces (say, a satellite while moving outside earth's atmosphere). However, the insistence of the use of the same mechanics for Newtonian systems at large generally produces mumbo–jumbo academic abstractions of "perpetual–motion–type". At any rate, the Newtonian systems of our environment violate the integrability conditions for the existence of a Hamiltonian representation in the frame of the observer, as established in the technical literature in all needed rigour.

As a result of this limitation of Hamiltonian mechanics and following over one century of contributions by mathematicians and theoreticians, the Birkhoffian generalization of Hamiltonian mechanics was born. Monograph [10] provides a review of this scientific process.

The fellow taxpayer is now encouraged to inspect the list of references of monograph [10], or that of any contribution in the field (that is, strictly non–Hamiltonian). He/she will note a virtually complete lack of references to papers printed in APS journals.

As a further documentation, the fellow taxpayer may consider the ongoing effort to contruct a generalization of quantum mechanics under the name of "hadronic mechanics" (Section 1.6). Admittedly, the new mechanics has been proved to be mathematically consistent, although its compliance with the physical reality is far from being established at this time. We therefore have the case of a potential new branch of physics at the

threshold of birth.

Now, despite the fact that:

- the hadronic mechanics has been studied by a considerable number of mathematicians, theoreticians, and experimentalists for a number of years;
- the mathematical foundations of the new mechanics have been studied at five international workshops (those on the so–called Lie–admissible formulations initiated at Harvard in 1978; see Section 1.9, and proceedings [124–125]);
- a formal presentation of the new mechanics occurred at an international conference in Orleans, France (see proceedings [126]);
- the new mechanics was subsequently studied at two workshops specifically devoted to the physical aspects of the problem (the "Workshops on Hadronic mechanics"; see proceedings [127]);
- despite the appearance of a considerable number of papers in the field;

despite all that, the name "hadronic mechanics" has not yet appeared in print in any APS journal to this day (June 30, 1984).

Papers on the Birkhoffian and hadronic mechanics have indeed been submitted to APS journals by myself and, independently, by several other authors. The point is that these papers were systematically rejected.

The ultimate roots of the problem.

In my view, the roots of the occurrence are the vested, academic–financial–ethnic interests in U.S. academia on Einstein's theories. We are therefore facing always the same, ultimate, roots for ALL the problems considered in this book. I cannot find any other "explanation" which achieves even a comparable credibility.

It is important for the taxpayer to have all the necessary information for the achievement of independent judgment on the matter. The presentation of Chapter 1 and the quoted references provide precisely such information. The taxpayer will therefore recall the following aspects:

- The Birkhoffian mechanics establishes in an irreconcilable way the limitations of Einstein's special relativity in classical mechanics. In fact, the Newtonian limit of the special relativity is strictly Hamiltonian and cannot therefore be compatible with the covering Birkhoffian mechanics. The new mechanics therefore establishes the foundations for a suitable generalization of Einstein's special relativity.

- The Birkhoffian mechanics establishes, beyond any reasonable doubt, the irreconcilable incompatibility

of Einstein's interior gravitation with physical trajectories of the real world, those genuinely non-conservative. In fact, Einstein's interior gravitation was built to admit only Hamiltonian, perpetual-motion—type of internal trajectories. Not even this task was truly accomplished by Einstein, as established by Yilmaz (see Section 1.5). The birkhoffian mechanics therefore establishes the need for a suitable generalization of Einsteinian gravitational theories for the interior problem.

- The hadronic mechanics is an operator version of the Birkhoffian mechanics and, as such, it is irreconcilably incompatible with Einstein's special and general relativities. Of course, I am referring only to the arena of its intended use, the INTERIOR of nuclei, strongly interacting particles, and stars, while recovering conventional formulations for the EXTERIOR dynamics.

In summary, the very names "Birkhoffian mechanics" and "hadronic mechanics" are synonyms of nonaligned research. From the preceding presentation of this chapter, the fellow taxpayer can therefore imagine the vigor with which possible publications in the fields at APS journals have been suffocated at birth.

The most plausible reason for the suppression of potentially fundamental advances at the APS journals is that novelty is always threatening to existing, vested, academic—financial—ethnic interests, or at least that is the way possible advances are perceived by vested interests in control.*

The financial implications of the problem.

The fellow taxpayer should also keep in mind the financial implications of the problem. Only then he/she can appraise sufficiently its national character. All research contracts in physics are granted by governmental agencies on the basis of the contents of the application and, mostly, on the applicant's record of publications. The point is that the publications by grant referees are studiously restricted to PUBLICATIONS IN APS JOURNALS.

The suppression of plausible conjectures and/or dissident views in APS journals therefore implies whether directly or indirectly, the denial of federal contracts. APS publications are there-

* The scientific reality is, of course, different. For instance, as elaborated in pp. 126—129 of this volume, the possible invalidation and generalization of Einstein's special relativity in the interior of hadrons may well permit the resolution of some of the most vexing open problems of current quark theories, such as the true confinement of the unobserved quarks in the interior of hadrons, or the identification of quark constituents with physical, experimentally detected particles.

fore a vehicle for the allocation of large public funds, or their shifting from one research line to another.

My own experience is sufficient to document the occurrence. In fact, as we shall see in the next section, the rejection of I.B.R. research grant proposals for mathematical, theoretical and experimental developments of the hadronic mechanics often occurred on the basis of the claim that myself and the other applicants did not publish articles in the field in APS journals. The fact that we have published articles in the field in several other REFEREED journals had no value. Thus, the systematic rejection of the papers submitted to APS journals on the conjectural physical value of hadronic mechanics, subsequently implied the systematic rejection of a rather considerable and diversified body of grant applications.

The national character of the problem.

The American Physical Society is an independent, private organization which, as such, is not subjectable to external interferences unless requested by law. As a result, I can voice here my concern as an APS member, but it could likely be inappropriate for me to express the same concern in other capacities.

Nevertheless, the problem at APS journals constitutes, in my view, a problem of clear national proportions. No informed person, genuinely interested in the well being of America, can deny this. In fact, the systematic suppression of plausible physical conjectures at APS journals necessarily implies the suppression of the birth of advances of potentially national interest , including military profiles. After all, most classified physical research started via articles in APS journals to achieve the needed credibility.

The standard of reference for excellence in APS publications.

As anticipated in Section 1.7, all papers rejected by APS journals should be compared with the current standard of excellence in the field. It is given by the so—called quark theories that have dominated particle physics soon after their original proposal by Gell—Mann [92] , Zweig and others in 1964.

An outsider would therefore expect that these theories, being the standard of excellence in the field, are non—conjectural and fully established beyond reasonable doubts. Nothing could be more fallacious than that. **Quark theories are among the most CONJECTURAL theories of our time, for a litany of reasons, each one of rather fundamental character** (see, for instance, dissident paper [49]). It is sufficient to recall here that the quarks themselves are purely conjectural at this time, having escaped direct experimental detections conducted for almost two decades

at a cost of hundreds of millions of dollars, fellow taxpayer. Also, quark theories have not yet achieved the so—called strict—confinement of quarks, that is, a formulation possessing an IDENTICALLY NULL AND EXPLICITLY PROVED probability of tunnel effects or of inelastic production of free quarks, as needed to comply with experimental evidence. Current theories generally have a "qualitative confinement" thus being in direct disagreement with the experimental reality in their current formulation.*

In short (and on this point I intend to be repetitious) by no means APS journals reject plausible conjectures because of insufficient physical evidence. Not at all. If this rule were truly applied, APS journals should terminate their publications. Instead, APS journals publish selectively only certain types of plausible conjectures, and reject others.

How this selection is done and by whom? The selection is done on grounds of whether or not a given conjecture is aligned with vested academic—financial—ethnic interests. The decision is taken by the usual groups of people controlling local physical institutions and national laboratories: leading physicists at leading U.S. institutions.

It is all a totalitarian machination conceived, organized and operated in the interest of a few, in basic disregard of scientific democracy, that is, in disregard of the interest of the Country. It is mostly academic politics, only conducted on the ultimate foundations of human knowledge.

Ironically, the paper that started this editorial dynasty at APS journals, Gell—Mann's paper [92], was rejected by Phys. Rev. Letters, as well known in academic corridors. According to insisting rumors, the rejection was done via such offensive reports, to force the author into a plea not to submit further papers to the same journal.

This case, rather than being the exception, fits perfectly into the appraisal presented above, and in actuality it could have been predicted by the attentive reader. Paper [92] was one of those rare, seminal papers that can change the course of physics, of course, when seeded in a scientifically fertile community. This implies that paper [92] was not aligned

* In my capacity as editor of the Hadronic Journal, I received in 1979, a paper on quark conjectures whose lack of confinement was excessively manifest. I therefore submitted the paper to two referees, one with a notorious (financial) alignment with vested interests on quark lines (a theoreticians), and the other with an impeccable record of ethical standards (an applied mathematician). The former recommended publication (upon the implementation of marginal improvements grossly irrilevant here). The other indicated that "the publication in a physical journal of a paper in quark theories without a rigorous confinement of quarks, would be equivalent to the publication in a mathematical journal of a paper stating that $2 + 2 = 318$".

with the vested interests of the time. Rejection in the most vigorous possible form was then an absolute necessity, under current APS operations.

This is exactly what happened. That is the way totalitarian systems operate. We are merely facing their specialization to the case of physical inquiries.

The means for the actuation of APS editorial policies.

The fellow taxpayer can readily anticipate that the people responsible for the current situation at the APS journals are not naive. And in fact, the realization of the scenario depicted above is so sophisticated, to be impeccable at a superficial inspection.

To begin with, the fellow taxpayer should know that, in general, APS journals do not reject papers. The editors merely send the referee reports to the authors for their consideration. After reception of the revised version of nonaligned papers, the process is repeated again, and again, and again at times for years, until the authors are tired of wasting their time, and submit the paper to another journal outside the APS.

As everybody can see, this technique is indeed impeccable, but only on the surface. In reality, the technique hides the violation of a number of basic editorial principles, as well as a sizable scientific accountability by APS editors vis—a—vis the Country.

To begin, a primary duty of editors and referees alike is that of being SCIENTIFICALLY CONSTRUCTIVE, particularly in their criticisms. To fulfill this societal function, a rejection must therefore contain the detailed identification and itemization of the aspects that should be improved by the authors to reach the necessary maturity of publication. Lacking such specific guidelines for improvements, authors face an endless variety of possible, different, revisions. The chances of their selecting exactly the revision desired by the referees is virtually null. Under these conditions, the re—submission of a new version of the paper revised by the authors without specific guidelines by the referees generally results into a waste of time.

A primary means for rejecting nonaligned papers by APS journals is via the absence of scientifically constructive suggestions in the referee reports, with particular reference to the studious avoidance of the indication of the revisions needed to achieve maturity of publication. I can provide, alone, a considerable number of APS referee reports to establish the existence of this antiscientific practice at APS journals beyond reasonable doubts. Additional documentation can be obtained by numerous other physicists in the USA and abroad. The studious, specifically intended nature of the occurrence can also be documented beyond reasonable doubts, because the requests of identification of specific improvements needed to achieve ma-

turity of publication were not honored in a meaningful way.

I have been a referee of a number of journals in the USA and aborad for almost two decades, and an editor of a physics journal for over seven years. I therefore have sufficient experience to identify the above technique of rejection with a mere glance at the report.

But, the lack of scientifically constructive contents in APS referee reports is only the tip of the iceberg. The ultimate responsibility rests, and otherwise must rest with the editors. In fact, the editors are PERSONALLY responsible for:

- the selection of the referees;
- the formal acceptance of their reports and their mailing to the authors; and,
- the selection of the subsequent procedure, e.g., whether to consult another referee.

When APS journals reject a paper via referee reports lacking any CONSTRUCTIVE scientific contents, the primary responsibility rests with the editors. Referee reports are scientific material exactly like the manuscript submitted for publication. The editor is therefore personally responsible for the acceptance of the referee report, or its rejection and return to the referee for improvement PRIOR to its official acceptance by the journal and mailing to the authors. Therefore, when authors receive scientifically vacuous reports, the primary responsibility rests with the editors.

But we are still at the surface of the problem. Anybody with a minimum of knowledge of the structure and organization of the American Physical Society knows that potentially important papers are passed to leading members "in good standing" at the society. This is a known euphemism to indicate leading representative of vested, academic—financial—ethnic interests in control of the field. The rejection of the paper, under these premises is then inevitable.

At any rate, mature editors know sufficiently well the academic, the financial and the ethnic interests of primary referees. As a result, they can judge in advance in the greater majority of the cases whether given, nonaligned papers will be rejected or have a chance of reaching the light with the selected referee. In this sense, the suppression of the publication of unaligned papers, let alone dissident views, is often decided by the editor at the time of the selection of the referee (in full parallelism of what happens for grants — see next section).

The problem, however, is so deep and articulated, that we are still far from its end. The next issues are those regarding the ethical responsibilities of the editors. However, the appraisal of this, as well as of a number of other aspects, demands the consideration of specific cases, and cannot be treated on general grounds.

My first dissident paper submitted to an APS journal.

Back in 1972, I worked with a graduate student of mine on a project in particle physics and submitted a joint paper to Phys. Rev. D entitled "Generalization of the PCT theorem to all discrete space–time symmetries in quantum field theory". The central tools of the paper were the so–called Wightman's axioms, which essentially represent the ultimate embodiment of Einsteinian ideas within the context of quantum field theory.

Since the time I studied Wightman's axioms several years earlier, I was convinced that they were evidently valid under appropriate physical conditions. Nevertheless, I had doubts on the universal validity of Wightman's axioms under unlimited physical conditions of particles, simply because theories of this type exist in academic politics, but not in the real world. The value of the possible identification of the limitations of Wightman's axioms is evident. In fact, such an identification would have stimulated the search for more general axioms, possibly valid under broader physical conditions.

My graduate student and I therefore initiated a laborious work aimed at extracting as many consequences of the axioms as possible, with particular reference to those with a potential capability of direct experimental verification. We did indeed succeed in this task, inasmuch as we generalized one of the central theorems of quantum field theory, the so–called PCT theorem. Upon achieving sufficient maturity, we therefore submitted a paper to Phys. Rev. D which essentially presented our generalized theorem, and a number of comments indicating the purpose for which the paper had been written: identify consequences of Wightman's axioms suitable for their experimental test.

The paper was immediately rejected. Yet, the referee could not disprove our theorem. So I wrote back asking for specific indications where the paper was wrong, while making marginal improvements. This type of submission–and–rejection–followed–by–a–revised–version–followed–by–rejection, went on and on, and on, for ABOUT TWO YEARS, without any flaw being identified by the referees in the central theorem. So much time passed by that, following the submission, my graduate student received his Ph. D. degree; he spent one year (unsuccessfully) looking for a job in the U.S.A.; and then left America for an academic job in Europe!

At that time, I was still very naive. In particular, I rejected the idea that academic politics could dominate the publications of the APS. It took therefore years for me to understand what was really going on. When I did, things changed drastically and rapidly. In fact, I merely removed from the paper any scientifically valuable passage aimed at the use of the results for the verification of the validity or invalidity of Wightman's axioms in the physical reality. As soon as I did that, the paper was published immediately (see ref. [140]). The price I had to pay is the suppression of its primary physical contents.

Correspondence in 1979–1980 with R. K. Adair of Yale University as editor of Physical Review Letters.

On January 26, 1979, R. K. Adair, G. L. Triggs, and G. L. Wells, editors of Phys. Rev. Letters, mailed a memorandum to all members of the APS Division of Particles and Fields regarding general editorial policies (Doc., Vol. II, p. 481). I thought that this was an excellent opportunity to voice my concern on the editorial policies of the APS journals, and to present my suggestions for possible improvement, whatever their value was.

I did present my views, but the action was a total waste of time. The reading of the correspondence with R. K. Adair on the topic (pp. II–288–507) is instructive. It starts with the most polite possible mutual language; it goes through a crescendo of identification of the problems and the scientific action needed for their containment; to reach a point of irreconcilable disagreement. The correspondence was closed via the following dry note by Adair stating (p. II–507)

"Dr. Santilli, I have received your insulting letter of Oct. 23, and I write this note as a termination of our correspondence. R. K. Adair."

Let me state that my language was as scientifically aggressive as possible, but not offensive, as the interested reader can verify. Whether I was offensive or not, that is of no relevance here. The substance of the issue is the point of real interest.

My concern was (and still is) that papers on quark theories routinely published in APS journals did not identify, even minimally, the conjectural character of the basic physical laws and relativities used for the strong interactions, nor they provided a clear separation of experimentally established facts from theoretical beliefs, thus creating the prerequisites for the conduction of physics via totalitarian authority, rather than physical veritas.

The issue was therefore the following: what are the conditions for a paper on quarks to be sound on grounds of scientific ethics and accountability? The lack of direct experimental verification of Einstein's special relativity under strong interactions is an incontrovertible scientific reality of our times. Silence on this situation MUST therefore constitute an issue of scientific ethics and accountability.

Adair never agreed that papers on quark theories had to indicate, at least indirectly or marginally, the conjectural character of Einstein's ideas under strong interactions. The grounds were therefore confirmed for the potential obscurantism that I fear to be under way in U.S. physics.

The moratorium of 1980 on the publication of papers on nonrelativistic quark theories at the Hadronic Journal.

The next episode is that reportedon pp. 136–140 of this

book. the fellow taxpayer should perhaps reconsider it at this point. In fact, the moratorium followed the correspondence with R. K. Adair on questions of scientific ethics and accountability. As the taxpayer will recall, the moratorium was suggested by excessively big inconsistencies of the nonrelativistic quark theories of that time. As the taxpayer will also recall, G. L. Trigg, as APS editor, dismissed the moratorium on grounds that the deficiencies were of "questionable mathematics" (while they were instead of fundamental physical relevance, such as the violation of Galilei's relativity; the violation of the conservation of the total energy; etc.).

At any rate, my efforts to inform APS editors of the moratorium at the Hadronic Journal, with disclosure of all needed information (including an invitation to participate at a subsequent meeting where the issue would be discussed by mathematician experts in the field), all this resulted in a complete waste of time. APS journals continued to publish papers on nonrelativistic quark theories without any apparent consideration of their excessively big inconsistencies, or at least a remote indication of the technical literature accumulated in the field.

But then, one cannot but raise doubts of subservience by APS journals and their officers to the vested, academic—financial—ethnic interests currently controlling the U.S. physics.

The rejection of a paper by Phys. Rev. D to recommend the test of Pauli's exclusion principle under strong interactions.

When, on June 1, 1980, L. H. Howard, director of the MIT Center of Applied Mathematics revoked my visit there under my, independently administreed, DOE contract (Section 2.2), it became a question of principle for me to write a paper of strict nonaligned character under my MIT affiliation. In fact, after a number of draftings and re—drafting, the paper was submitted to Phys. Rev. D on October 4, 1980.

The topic of the paper was to recommend the direct experimental verification of the rotational symmetry via the repetition of Rauch's experiments [96—99] along alternatives essentially reviewed in Section 1.7, pp. 148 and following (such as, the repetition of the tests as originally conducted although with a better accuracy; the repetition of the tests with a multiple of 720 deg in the spin precession; etc.).

The paper was evidently rejected, and then rejected again, and then rejected again, via a step—by—step realization of the technique outlined above in this section. The studiously nonscientific content of the referee reports is very instructive in this case. As an excerpt of the documentation (pp. II—516—530), one can read the following motivation for rejection: *"None of the proposed experiments are substantive. Anyone can ask for better accuracy or for a thermal beam of neutral kaons. The*

Physical Review need not publish idle dreams. (We need constructive suggestions)."

Evidently, the establishing of the rotational symmetry in particle physics in a quantitative way via direct experimental measures was an "idle dream" for this referee as well as for the responsible APS editor. The specific, detailed, experimental suggestions one can read on p. 148 are not substantial in the view of this referee–editor pair. Nevertheless, the plausibility of the deformation of protons and neutrons under sufficiently intense external fields and/or collisions is simply out of the question, and so is the consequential breaking of the rotational symmetry (see Figure 2.2.1 for a review). The validity of the rotational symmetry in strong interactions is today essentially imposed via academic power, rather than a quantitative experimental process. This situation cannot but raise "sustantive" questions of scientific ethics and accountabilities. I therefore answered with (typeset) comments as scientifically heavy as possible (pp. II–523–524).

The paper was rejected again, as expected, and, also as expected, the rejection was based on the total absence of any scientifically constructive process. In fact, the two, additional referee reports amounted to a total of seven (typed) lines, and concluded with the statement, evidently backed in full by the responsible APS editors (p. II–527), that: *". . the author's remarks on spin are totally unfounded and seriously flawed".*

All this, DESPITE THE FACT THAT THE BEST AVAILABLE MEASURES (715.87 ± 3.8 deg [100]) DID NOT (I REPEAT, DID NOT) CONTAIN THE ANGLE NEEDED TO ESTABLISH EXPERIMENTALLY THE ROTATIONAL SYMMETRY, AS EXPLICITLY INDICATED TO THE REFEREES AND THE EDITORS (SEE p. II–528). APS EDITORS THEREFORE ACCEPTED THE ABOVE REFEREE REPORT IN FULL KNOWLEDGE OF THE FACT THAT THE SOLE DIRECT EXPERIMENTAL DATA AVAILABLE AT THAT TIME (AND NOW) SHOW THE VIOLATION OF THE SYMMETRY!!!

It is evident to all that we are facing rather incredible excesses of questionable scientific practices. The natural question for the fellow taxpayer is then: How can such excesses occur these days in America? The answer is crucial for the contents of Chapter 3: The excesses occur, quite routinely, because the U.S. physics community is structured, organized, and operated under conditions of total impunity. No matter what editorial action an APS officer perpetrates against the interests of America and of human knowledge, that officer is absolutely certain of enjoying total impunity as things stand now (exactly the same situation occurs for officers of governmental agencies reviewing research grant applications; see the next section).

In this way, we begin to approach the roots of the suggestions submitted in Chapter 3 for the improvement of the scientific ethics and accountability in the U.S. physics community, beginning with all the necessary means to terminate the current

state of total impunity, as a prerequisite for individuals to face and fulfill their personal responsibilities. *

The handling by APS journals of a potentially fundamental, experimental papers on the origin of the irreversibility of our macroscopic world.

At this point, the fellow taxpayer is encouraged to reconsider the case of the experimental measures by the Québec–Berkeley–Bonn experimental group on the apparent origin of irreversibility in the most fundamental and elementary level of nuclear interactions (their spin component), ref. [103]. The case was reported on pp. 160–168 of this book. Most importantly, the fellow taxpayer should recall that the experimental confirmation of measures [103] would have implied sooner or later the need to generalize Einstein's special relativity beginning with its most fundamental part, the time component.

This case is the experimental background of the following theoretical case at APS journals. In particular, the taxpayer should recall that two papers were submitted by the experimenters to APS journals, the first to Phys. Rev. Letters (which was published [103] only after the iterim reviewed on pp. 160–168). The second paper was submitted to Phys. Rev. C (Nuclear Physics), following the appearance of the experimental rebuffal by a Los Alamos group [104], and following a repetition of the original measures which confirmed findings [103]. This latter paper was rejected by Phys. Rev. C, although it was readily published in a European journal [105].

The way APS journals handled the experimental papers by the Québec–Berkeley–Bonn group on the apparent time–asymmetry of nuclear interactions is so grave and its societal impli-

* I should indicate for fairness that, out of the scientific production reviewed in Chapter 1, APS journals did indeed publish ONE single paper in the field, ref. [123]. This publication, however occurred after about two years of refereeing fights. Also, the acceptance of the paper was preceded by a phone call from a colleague I knew (who was not an APS editor), indicating quite clearly the extreme improbability that APS journals would publish additional papers of mine in the same field for the foreseeable future. This prediction resulted to be prophetic. In fact, the prediction was confirmed by the rejection of the paper on Pauli's principle under consideration in this paragraph. The prediction was subsequently confirmed by the rejection of the theoretical paper on the origin of irreversibility treated below. Finally, the prediction was confirmed by a number of additional episodes I did not report here for brevity, such as the submission in 1983 of a paper to Phys. Rev. D UNDER LEGAL ASSISTANCE because dealing with a rather considerable editorial insufficiency of a paper on the test of the rotational symmetry that had been previously published in the same journal (the paper, even though on the experimental verification of the rotational symmetry, had not quoted Rauch's crucial measures [100], Eder's contributions [64–66] and other papers in the field). The interested reader may find the documentation of this additional case in Vol. II, pp. 682–689.

cations so vast, in my view, to justify at least some appropriate governmental investigations. After all (and as indicated in Chapter 1.7) there are reasons to expect that, following numerous independent solicitations (including mine), the case is under monitoring by the Nobel Committee and other foreign bodies.*

Rejection of a crucial theoretical paper on the possible, interior time–asymmetry of particle interactions.

A most serious episode, which was crucial for the decision to write IL GRANDE GRIDO, and which resulted in the requests of resignation of two members of the APS editorial staff, occurred in 1982–1983. It referred to the stubborn rejections of a theoretical paper I submitted on the use of the hadronic mechanics for the possible identification of the origin of irreversibility, and the regaining of unity of thought. The documentation of this case alone exceeds the mark of 1,000 pages when inclusive of the technical aspects. It has been summarily reproduced in pp. II–516–679. In the following, I can therefore only review some of the most salient aspects. A knowledge of the background technical profile is essential for an in depth understanding of the case (see Section 1.6, pp. 101–109 and Section 1.7, pp. 160–168 on the theoretical and experimental aspects of irreversibility).

A summary of the case is the following. The paper was originally submitted to Phys. Rev. Letters on April 16, 1982 (p. II–532) under the first title: "Use of the hadronic mechanics for the best fit of the time–asymmetry recently measured by Slobodrian, Conzett, et al"; APS ref. No. LR2111 (cited numerous times in the documentation). The paper was rejected on May 20, 1982, by the editor G. L. Trigg (p. II–533). The paper was re–submitted on May 26, 1982, in a revised form (p. II–536), including rather comprehensive information. Trigg rejected the paper again on July 2, 1982 (p. II–542). A second revised version with additional information was re–submitted on July 21, 1982 (p. II–544), which was rejected by Trigg again on September 3, 1982. A third revision was re–submitted with an improved title on September 9, 1982 (pp. II–551), which was rejected again by Trigg soon thereafter. A further, this time final revision was re–submitted for the fifth and last time to the APS Editor in Chief, David Lazarus, on December 14, 1982 (p. II–568), with: additional material; the list of several experts in the fields of the paper; and the recommendation to conduct a comprehensive review, by consulting as many experts in each field touched by the paper as possible. The paper was considered a "new" one and identified with the new ref. No. LZ 2206. It was rejected by Trigg on April 6, 1983 (p. II–580)

*A presentation to the Nobel Committee is reproduced in pp. II–620–622. D. Lazarus was informed on July 6, 1982 (p. II–612).

The rejections implied a number of consequences reviewed later on in this section. As far as the paper is concerned, I became tired of wasting my time with APS journals, and submitted the paper to a European journal where it was received, reviewed, typeset, and printed in about three weeks (see ref. [59]).

Paper LR2111/LZ2206 therefore constitutes a beautiful documentation of the techniques of rejection of nonaligned papers apparently in effect at APS journals, that of tiring the authors via rejections followed by rejection followed by further rejections, all without any scientifically constructive contents, until the authors send their papers elsewhere. In this sense, APS editors and referees can claim victory for paper LR2111/LZ2206. Who the real loser is will be decided by the fellow taxpayer upon understanding the implications for America of the editorial practices in effects at APS journals.

The scientific scene in APS journals underlying the topic of the paper.

The fellow taxpayer should be aware of the fact that, at the time of the episode of paper LR2111/LZ2206, as well as now, publications in APS journals were suffering from a truly incredible lack of unity of physical thought and underlying mathematical structure. In fact, APS journals were (and still are) routinely publishing papers in different segments of physics with a manifest, irreconcilable, mutual incompatibility. This situation has been reviewed in Section 1.6 (see in particular Figure 1.6.3). At this point, I merely recall for the taxpayer's convenience the following facts:

A— the Newtonian systems of our enviornment (missiles trajectories in atmosphere; damped spinning tops; holonomic systems with evidently frictional hinges and constraints; etc.) possess a rigorously established NON—HAMILTONIAN analytic character; they evolve in time according to a NONCANONICAL law; and they are irreversible in the sense of violating the symmetry under inversion of time;

B— the statistical systems of our macroscopic world are also demonstratedly NON—HAMILTONIAN and NONCANONICAL because of well known collision terms which simply cannot be incorporated in the Hamiltonian; also the systems are irreversible, this time in the statistical sense (e.g., entropy);

C— elementary particle systems routinely treated in APS journals are, instead, strictly HAMILTONIAN (or, equivalently, Lagrangian); they evolve in time according to the so—called UNITARY law; and, last but not least, are generally time—reflection invariant.

In particular, while the characteristics of systems A and B are established beyond any possible doubt, those of systems C are

strictly conjectural at this time (e.g., based on the conjecture that quarks are physical particles, complemented by the additional conjecture that quark confine; supplemented by a litany of additional, even more fundamental conjectures, such as that Einstein's special relativity is exactly valid within hadrons; etc.; etc.; etc.).

The point that the fellow taxpayer should recall to reach a mature judgment of the case is that elementary particle systems C are IRRECONCILABLY INCOMPATIBLE with macroscopic systems A and B, thus resulting in the indicated lack of unity of physical thought in APS publications. The lack of mathematical unit is a direct consequence. In fact, the brackets of the time evolution of systems A and B are NON–LIE, while those of papers in elementary particle physics published in APS journals are LIE.

The fellow taxpayer will recall the case of Skylab during re–entry (pp. 28–29 of this book). The system was strictly non–Hamiltonian, non–canonical, and time–asymmetric. As such, Skylab simply could not be reduced in any credible way to a large collection of constituents with a dynamics which is Hamiltonian–Lagrangian, unitary and time–reversible.

The contents of paper LR2111/LZ2206.

With the understanding that the regaining of the unity of physical and mathematical thought will demand the participation of the scientific community at large over a predictably long period of time, the primary objective of paper LR2111/LZ2206 was that of simply initiating the traditional scientific process needed for the future resolution of the issue: the publication of plausible conjectures followed by the publication of independent appraisals.

The idea of the paper was simple and inspired by direct observation of nature (rather than consideration of academic politics). Look at our Earth. Its dynamical evolution within the solar system is fully time–reflection–invariant. To see the irreversibility, you have to enter into our atmosphere and examine OPEN, NONCONSERVATIVE, INTERIOR trajectories such as Skylab during re–entry. Paper LR2111/LZ2206 presented a particle model exactly along the same lines, that is, such that the time–reflection–symmetry is exact for the exterior, closed, center–of–mass treatment, while the interior dynamics is intrinsically time–asymmetric.

The paper (quite brief, being intended for a letter journal) then worked out generalizations suitable for the experimental verification of the theory (the generalization of the so–called theorem of detailed balancing and of the ratio between the analyzing power for the forward reaction with respect to the polarization of the backward reaction).

The paper finally concluded with the apparently full agreement of the theory with the measures by Slobodrian, Conzett, et al [103], under the assumption that they refer to OPEN, NONCONSERVATIVE nuclear reactions, where the nonconservative character is due to the external nature of the target used in the experiments.

The possible regaining of the unity of physical thought was studied in paper LR2111/LZ2206 via the non–Hamiltonian generalization of the interior dynamics. In fact, this reduced all the Newtonian, the statistical and the particle layers to the same class of underlying forces: superposition of action–at–a–distance/potential/Hamiltonian forces and contact/non–potential/non–Hamiltonian forces. Equivalently, the unity of physical thought was recovered by admitting the extended character of systems at all levels, the Newtonian, the statistical and the particle one. The existence of contact/non–Hamiltonian forces at all levels was then consequential.

The unity of mathematical thought was trivial for the theory of the paper. The reader will recall from Sections 1.3, 1.4 and 1.6 the direct universality of the Lie–admissible formulation of the dynamics in Newtonian and statistical mechanics. The theory of paper LR2111/LZ2206 then generalized the interior dynamics of particles also into a Lie–admissible form. In this way, different layers of Nature resulted to be nothing but different realizations of the same, single, unique, abstract mathematical axioms.

In summary, the theory presented in paper LR2111/LZ2206 combined two well established physical truths. On one side, it embodied the well known time–reflection–invariance of the center–of–mass of closed–isolated systems of particles. On the other side, the paper embodied another well established property, the time–asymmetry of nonconservative (e.g., dissipative) nuclear processes. This is a trivial consequence of the non–unitarity of the related time evolution, as well known since the birth of quantum mechanics.

Thus, the physical facts presented in paper LR2111/LZ2206 are simply incontrovertible. The theory presented merely reformulated known nonconservative, nonunitary time evolutions for the interior dynamics via mathematically more consistent and more modern tools (the Lie–admissible generalization of Lie's theory; see Figure 1.6.2, particularly p. 95).

And indeed, no APS referee could even remotely prove that ANY of the arguments of the paper was wrong, as confirmed by the APS editor in chief in our correspondence. The novelty of the paper was evident (see also next paragraph). Its fundamental character is established by the underlying generalizations of basic quantum mechanical laws. The stubborn, repetitious rejections by the APS editors and referees cannot therefore be supported by scientific grounds in any credible way, and must be expected to be due to nonscientific motivations of

academic politics.

The implication for APS journals NOT to participate in the ongoing efforts to construct the hadronic generalization of quantum mechanics.

Paper LR2111/LZ2206 was crucially dependent on the use of the hadronic generalization of quantum mechanics under construction by an increasing number of scholars [127, 133] following the original proposal at Harvard back in 1978 [14]. In fact, the interior dynamics of the theory is time–asymmetric in an intrinsic, dynamical way, e.g., irrespective of any invariance property of the Hamiltonian. In particular, the theory is based on certain generalizations of the most fundamental dynamical laws of contemporary theoretical physics, Schroedinger's and Heisenberg's equations, precisely, according to the Lie–admissible lines of the original proposal of 1978.

Paper LR2111/LZ2206 was therefore a crucial test: to ascertain whether or not APS journals were willing to participate in the laborious scientific process of trial and error which is needed to construct a new discipline. This point was stated, restated, and repetitiously indicated again, not only to the APS editors, but also to the APS Editor in Chief, D. Lazarus (see also below).

The stubborn rejections of paper LR2111/LZ2206 confirmed the apparently studious intent by APS journals NOT to participate in this ongoing scientific process, and to prevent the appearance of the words "hadronic mechanics" in their publications, as stated earlier. After having wasted so much of my time, it is a question of principle for me to avoid the submission of any paper to APS journals, until evident, concrete proof of serious ethical purges have either occurred spontaneously but publicly at APS journals, or are forced by suitable governmental bodies. Apparently, all other researchers in the field have also reached independently the same conclusion.

A taste of the antiscientific nature of the APS referee reports.

To reach a mature judgment, it is important for the taxpayer to inspect the referee reports which were formally accepted by APS and used for the rejection of paper LR2111/LZ2206. To have a first taste of them, let me recall that, at the time of the first submissions, there were only two experiments directly relevant to the topic considered:

- paper [103] by the Québec–Berkeley–Bonn experimental group claiming the existence of the time–asymmetry in nuclear physics; and,
- paper [104] by the Los Alamos group claiming a full time–reflection–invariance.

As a result of this situation, the case was unsettled, that is, lacking further runs of the measures, the experimental information was insufficient to claim which of the two papers was right and which was wrong.

As one can see, the first rejection (p. II–534) was based on the referee statement that: *"the data shown by Slobodrian et al. . . . . are not correct. A repetition of . . .[measures 103] by Hardekopf et al . . . .[ref. 104] yielded data in disagreement with the measurements by Slobodrian, and found agreement between the polarization and analyzing power, as one would expect from time–reversal–invariance."*

The antiscientific nature of this statement is such to raise doubts of potential scientific corruption in this editorial process at APS journals. In fact, no true Scientist could have claimed then, nor could claim now, PARTICULARLY IN A REFEREEING PROCESS, that one of opposing measures [103, 104] is right and the other is wrong. Only an intentional manipulation of basic human knowledge, perpetrated for the protection of vested, academic–financial–ethnic interests, can reach any "claim".

As a further taste of the scientific stature of the APS refereeng process, I may recall a further reason of rejection by a referee consisting of the view that the central equations of the paper were of *"exceedingly general and elementary aspect[sic], expressed in a bizarre notation."* (p. II–534) Besides the evident lack of relatedness of such a view, the fellow taxpayer should be aware of the fact that the paper submitted a generalization of the celebrated Heisenberg's equations $idA/dt = AH - HA$ into the cocovering form $idA/dt = ARH - HSA = A\triangleleft H - H\triangleright A$, where the symbols " $\triangleleft$ " and " $\triangleright$ " expressed the forward and backward character in time, as needed to treat irreversibility (Section 1.6). Now, Heisenberg's equations are some of the most fundamental equations of contemporary physics. Their possible generalization of any type would have equally fundamental, far reaching implications. The referee's report solicited by APS editors and backed up by the same editors did not care whether or not the proposed generalization of Heisenberg's equations was right or wrong. The referee only cared about the fact that the equations were written in a "bizarre notation"! But then, APS journals should not expect credibility from the international physics community!

Numerous additional, highly illustrative aspects can be identified in the refereeing process of paper LR2111/LZ2206. Regrettably, I am forced for brevity to refer the interested taxpayer to the Documentation, Vol. II, pp.531–588.

The lack of qualification of referees selected by APS editors.

A point that transpares quite clearly in the documentation is the manifest lack of qualifications of the referees selected by the APS editors on paper LR2111/LZ2206. The fellow taxpayer should recall that, to qualify as referee of a paper submitted to an APS journal, a physicists must (on the surface) be an "expert" in the field of the paper. Now, **the only possible qualification for being an "expert" in a given field is that the physicist has PUBLISHED AT LEAST ONE PAPER IN A REFEREED JOURNAL IN THAT FIELD.**

It is evident from the documentation that the referees selected by APS for the FIVE submissions and resubmissions of paper LR2111/LZ2206 were not experts in the field of the paper (isotopies and genotopies of enveloping associative algebras, Hilbert spaces and dynamical laws of quantum mechanics). Besides being transparent from the several nonsensical comments in the reports, the lack of expertise was often explicitly admitted by the referees themselves. Yet, the APS editors studiously accepted their reports and rejected the paper.

Note that the APS editors have no excuse here. In fact, owing to the novelty of the fields of the paper, I had provided them with a considerably list of senior mathematicians and theoreticians in the U.S.A. and abroad who were true experts in at least some of the areas of the paper (pp. II–568–573). The APS editors apparently decided to avoid the consultation of true experts and selected instead other non–experts. This illustrates the point made in the introductory remarks to the effect that the rejection of a non–aligned paper may be decided by the editor at its submission, via the appropriate selection of referees with a notorious academic–financial–ethnic non–alignment with the contents of the paper and/or of its authors.

At any rate, the demonstrable lack of qualification of the referees (see also the next paragraph) automatically implies the lack of a scientific process in favor of a nonscientific/political one.

The bottom line is that, despite the manifest lack of expertise, non–aligned papers are equally sent to leading physicists at leading U.S. institutions. This results into a further mechanism for the perpetration of current scientific control. In fact, leading physicists become arbiters, not only of papers in their true field of expertise, but also in other fields in which they have absolutely no qualification whatsoever.

The correspondence with D. Lazarus, as Editor in Chief of the American Physical Society.

As evidently predictable, I reported each and every aspect to the APS Editor in Chief, David Lazarus, beginning with the first rejection, and then continuing thereafter, until the closing of the case. This correspondence alone is per se rather voluminous (pp. II–589–645). The additional time I spent in writing

all these letters to Lazarus, and gathered for him the rather voluminous scientific material, also resulted to be a waste of time.

I did however learn a lot on how APS operates. For instance, my insistence on the referees being true, qualified and documented experts in the field of the paper met with the clarification by Lazarus that APS referees have to be qualified only as far as their APS standing is concerned. For instance, one can read the following passage by Lazarus (p. II–639)

"I have read through the comments of the three reviewers of this paper [version LZ2206] with some care, particularly since I do know their identities. All three are very respectable physicists, and referee no. 2, who dismissed the paper summarily, is a Nobel laureate. . . . . Note carefully that referees 1 and 2 feel that there is probable merit in the work but clearly cannot themselves understand it sufficiently to pass judgment on it [sic!!]. Referee 2 cannot even read the paper, and clearly finds it completely 'obscure'."

The fellow taxpayer should know that theoretical physics has become so specialized that, to understand a paper in Phys. Rev. Letters, one must be a true expert, specifically, in the field of the letter and possess a detailed technical knowledge of ALL quoted references.

My reply could not possibly be graceful, if I had to be in peace with my own ethical principles. In fact, I replied to Lazarus with numerous, rather heavy comments, including the passage (p. II–641)

"In the final analysis, the selection of a (US) Nobel laureate as a referee of my paper may be seen as demonstrably unethical because no (US) Nobel laureate has any meaningful knowledge and record of expertise in the field of the paper (isotopies and genotopies of Hilbert spaces and Lie algebras)."

Another point I learned in the correspondence with Lazarus is that the APS editor in chief is not an editor! This was clearly stated by Lazarus in his letter of January 6, 1983 (p. II–637). But then, the title of the post, "editor in chief" should be changed to something else because grossly misrepresentative for the general APS membership.

The rejection of paper LR2111/LZ2206 against the recommendation of qualified referees.

A further aspect of this episode is that not all referees rejected the paper. In fact, a senior, "leading physicist at a leading U.S. institution", Susumu Okubo of the University of Rochester, New York, did indeed recommend the publication of paper LR2111/LZ2206. In fact, Okubo acknowledged to me that (p. II–567) *". . .I was one of the referees of your paper as you rightly guessed. Although I did not recommend its publication*

to the [Phys. Rev] Letters, I suggested that it should be published rather in Phys. Rev."

Publication in Phys. Rev. was perfectly acceptable to me, as stated and restated in the correspondence with the editors. In fact, the ongoing test was to see whether or not APS journals should participate or be excluded by the scientific adventure under way in the construction of the hadronic mechanics. The selection of the specific APS paper was immaterial.

As one can see, APS editors rejected paper LR2111/LZ2206 despite favorable recommendations such as that by a physicist as senior and as renowned as Okubo. This evidently confirms the apparent, firm, determination at the EDITORIAL LEVEL to reject the paper for reasons of academic politics.

The request of resignation of Charles M. Sommerfield of Yale University as Divisional Associate Editor of Physical Review Letters.

One day in October, 1982, in the midst of the rage of the scientific battle on paper LR2111/LZ2206, I received the following UNSOLICITED letter from Sommerfield at Yale in his capacity as associate editor of Phys. Rev. Letters (p. II—646)

"Dear Dr. Santilli:
The dossier on your manuscript LR2111 on time asymmetry has been sent to me in my capacity as Associate Editor of Physical Review Letters. My task is to determine if the referees have properly performed their jobs in evaluating the paper. In the present case, the referees, all of whom are well known and respected physicists, have done just that. Thus, I can find no grounds for reversing their unanimous recommendation that the manuscript not be published in the Letters.
Best regards,
Charles M. Sommerfield
Divisional Associate Editor
Physical Review Letters"

I immediately answered with the following certified letter, return receipt requested (p. II—648)

"Dear Dr. Sommerfield,
As a member of the American Physical Society, I am hereby requesting that

you tender your resignation from your position of divisional associate editor of the Physical Review Letters,

and terminate all your editorial functions at the Journals of the APS as soon as possible.
This request is the result of your unsolicited letter of September 30, 1982, (which reached me only on October 14, 1982) in which you misused your editorial position, you violated basic codes of our profession, and created doubts on the editorial processing which are damaging to the APS.
In fact, you passed judgment as a physicist on my paper LR2111

submitted to Physical Review Letters dealing with the vast field of non—Lagrangian/non—Hamiltonian, Newtonian, statistical, and particle dynamics in which you have no established record whatsoever of expertise. In addition, the contents of your letter indicates that you did not take the responsibility to become acquainted, even minimally, with this vast new field.

Episodes of this type generally admit the explanation that the editorial action is taken in the sole, intended, specific benefit of particular academic interests, or because of recommendations from members of the same group of academic interests, in disrespect of National interests for the pursuit of novel physical knowledge. In order to prevent even the remote possibility of shadows of this type on the editorial sector of the APS, you are hereby requested to resign.

You must be fully aware that this is a formal request of resignation and that, in case of its lack of due consideration, all necessary action will be implemented as vigorously as possible, as permitted by the codes of laws and of the APS, not to exclude individual and/or group action, in order to protect National interests as well as the image of the APS throughout the World.

Ruggero Maria Santilli
Member of the American Physical Society
96 Prescott Street, Cambridge, Massachusetts 02138
cc: Dr. D. LAZARUS, Editor in Chief, APS
Observers

P.S. You should be made aware that, jointly with your letter of September 30, 1982, rejecting my paper LR2111 on a theoretical treatment of time—asymmetry, I received not one, but two copies (apparently because of a mailing mixup) of the recent paper by the Québec experimental group submitted to PR—C which confirms the original measures of time—asymmetry, by therefore providing a beautiful EXPERIMENTAL confirmation of my own paper."

Sommerfield did not resign from his post. D. Lazarus (who had been immediately informed of the case) did not suggest Sommerfield to resign. A. B. Giamatti, President of Yale University, and F. W. K. Firk, Chairman of the Physics Department at Yale, who were immediately informed of the occurrence (pp. II—675—676), did not even acknowledge my letter. The writing of IL GRANDE GRIDO, as a first step toward the removal of Sommerfield from his editorial APS post, was for me absolutely unavoidable.

The request of resignation of R. K. Adair also of Yale University as editor of Physical Review Letters.

In October, 1982, I subsequently received the following additional, also UNSOLICITED, letter from Adair (pp.II—649—650), in his capacity as editor of Phys. Rev. Letters and chairman of the divisional associate editors. Adair evidently sup-

ported the action by Sommerfield on the reason that, in his view, *"Sommerfield acted, as he should, not as a referee but as an editor."* The letter furthermore specified that *"In your letter to David Lazarus, you speak of the possibility of submitting a revised version of your paper to Phys. Rev. Letters. I must point out to you that paper LR2111 has been rejected, and we will not consider again a paper which is quite similar to LR2111."* This evidently confirmed the predetermined decision of preventing the appearance of the paper in an APS journal, irrespective of any improvement I could conceivably achieve.

Adair's letter had initiated with the statement that *"I am not writing to you to object to your request (?) that he [Sommerfield] resign. The first Amendment to the U.S. Constitution gives you the absolute right to ask anyone, President, Pope or Editor to resign. And President, Pope or Editor can ignore you."*

I immediately answered with the following letter, also certified, return receipt requested (p. II–651):

"Dr. Adair,

It was instructively edifying to read in your letter of October 27, 1982, that you associate yourself and Dr. C. Somerfield with popes and presidents.

I am under the impression that you understood absolutely nothing of the entire issue of my paper LR2111 submitted to Phys. Rev. Letters. However, the position that Yale University continues to give you presupposes you have the full mental capacities to understand the issue. In this latter case, a more probable occurrence is that you simply mimic lack of understanding for the pursuance of objectives to be identified at the appropriate time.

As said countless times by now, PRL has the following two alternatives for paper LR2111.

ALTERNATIVE I. Paper LR2111 is rejected because of the clear identification of scientifically credible errors, inconsistencies, or incompatibilities presented in due scientific language. In this case, you should expect nothing more than my respectful and graceful acceptance.

ALTERNATIVE II. PRL continues to reject the paper on the basis that the available referee reports are credible. In this case, I shall oppose the decision in any conceivable way permitted by law, beginning with the filing of law suits to you and Dr. Sommerfiled, first, as individuals, and second, as associate editors.

All my efforts have been devoted to the implementation of the best possible scientific process in this case, owing to the number of observers, and of international implications, in the best possible interest of the American Physical Society.

Your letter is a total uncompromisable rejection of this orderly scientific process, on mere grounds that 'the professor says so, and therefore it is so'.

The action by you and your friend Dr. Sommerfield could be tolerated if it occurred in countries under totalitarial control,

whether of political or ethnic color. It appears you forget that we are in the United States of America. If aspects of questionable conduct occurred within public offices are brought to the attention of the public at large, the persons involved are socially dead here, sooner or later. It is only a matter of time. You associate yourself to presidents, but you forget President Nixon.

Your letter constitutes the second, completely unsolicited intervention in the case. As such, it can only prove your personal, uncontrollable desire to prevent the publication of the paper, as well as to support your personal friend Dr. Sommerfield, in complete disrespect of the interests of the American Physical Society, as evidentiated by your presumptuous assumption that PRL will not consider again paper LR2111.

In addition, your letter constitutes the second, unsolicited attempt intended to falsify or otherwise annul specific agreements in regard to paper LR2111 reached with Dr. Lazarus as Editor in Chief of Physical Reviews and Physical Review Letters.

In view of these and other circumstances, I am hereby requesting (sic) that you also resign from your editorial post at the Physical Review Letters, and terminate all your associations with the Journals of the American Physical Society.

Finally, I must take all possible precautions, in the interest of the American Physical Society, to truncate this insanity of unsolicited interventions in the orderly scientific process regarding paper LR2111, beginning with formal requests to the appropriate bodies to initiate investigative committees.

Ruggero Maria Santilli, Member of the American Physical Society

cc: Drs. A. B. GIAMATTI and F. W. K. FIRK, Yale University; Drs. D. LAZARUS, G. TRIGG, G. J. DREISS, and D. NORDSTROM, Phys. Rev. and Phys. Rev. Lett.; selected observers."

Adair answered with a letter dated November 12, 1982 (pp. II–652–653), containing the following passages

"I rejected your paper because I decided that the objectives of the journal would be better served by other selections.". . ."the final responsibility for the acceptance or rejection of papers is mine and you may conclude that what disagreements you have with the Editors –– and Associate Editors –– are disagreements with me. As for your 'request' that I resign; after more than four years at this job I have asked to be relieved in the fullness of time but, for the moment, I have more work to do and must reluctantly reject that request."

Thus, Adair confirmed in writing what I had suspected since the beginning, that Trigg was merely serving his name, while Adair was the true, ultimate editor responsible for paper LR2111/LZ2206. This multiplied the reasons of my determination to undertake any action permitted by law so that Adair and his friend Sommerfield terminate all their present and future editorial functions at the APS. IL GRANDE GRIDO is only the first step intended to inform the widest possible scientific com-

munity in all different languages, as well as to set the necessary record for the only judgment that truly counts in scientific matter: that by posterity.

Evidently, I did not even bother to write again to Adair. Nevertheless, I did write to Lazarus at the APS and to Giamatti and Firk at Yale University, by providing all the necessary information and documentation.

The elaboration of one aspect of my request of resignation to Adair may be of relevance for the fellow taxpayer. It is the passage indicating that the actions by Adair and Sommerfield annulled specific agreements I had reached with the APS editor in chief. In essence, during a phone conversation in September, 1982, I had proposed Lazarus to pause in the consideration of paper LR2111/LZ2206 for a couple of months or more, to give time to Phys. Rev. C (Nuclear Physics) to consider the new experimental paper in time—asymmetry submitted by the Québec—Berkeley—Bonn experimental group to rebuff the Los Alamos measures [104]. I had been informed of this submission directly by the authors. Also, this is exactly the experimental paper that Sommerfield's letter had inadvertently included.

Since there were experimentalists in three Countries (Canada, U.S.A. and West Germany) submitting an EXPERIMENTAL PAPER WHICH SUPPORTED PAPER LR2111/LZ2206, Lazarus could not evidently reject my proposal to pause. At any rate, it would have made no difference to the vested interests to reject my paper in September or two months later. Thus, Lazarus gladly agreed to my evidently moderate proposal.

Sommerfield AND Adair could not evidently control their desire to suppress the publication of paper LR2111/LZ2206 as soon as possible, and therefore ignored the two months "truce" I had agreed with Lazarus. They "had" to convey their unsolicited rejection of the paper as soon as possible.*

* By no means the present section exhausts all aspects related to Adair and Sommerfield at Yale University, of which I am aware. As an example, Yale is renowned for the vastity of its libraries, by possessing one of the most vast collections of research journals on a world—wide basis. The care with which Yale's libraries are provided with funds for the updating of this record is also well known. Despite that, Yale University has always declined the subscription to the Hadronic Journal, apparently because of opposition originating from within the department of physics (where Adair and Sommerfield belong), beginning from the first announcement of late 1977, and continuing with announcements mailed to Yale libraries every subsequent year (for an excerpt, see pp. III—677). The fellow taxpayer should recall that the Hadronic Journal is one of the few journals permitting; and actually promoting, explicit studies on the insufficiencies, limitations and inconsistencies of Einstein's theories. The lack of subscription to the Hadronic Journal, which is not evidently due to budgetary restrictions, has evidently implied the suppression of the possible exposure of young minds at Yale to dissident physical thought.

Whatever the truth, a number of things are established: the APS journals, not only rejected my theoretical paper LR 2111/LZ2206 on the time–asymmetry, but also the EXPERIMENTAL paper supporting my arguments. In fact, this latter paper too, like mine, had to be published elsewhere (see ref. [105]).

It is impossible not to suspect that the reason for such a truly unusual vigor in rejections is due to the fact that papers [59, 105] are irreconcilably incompatible with Einstein's special relativity, by therefore being manifestly damaging to vested, academic–financial–ethnic interests in U.S. physics.

The specter of a conceivable conspiracy at APS journals.

But above all, the fellow taxpayer should keep in mind the rumors I have heard in more than one continent, that the rebuffal of experiments [103] by R. A. Hardekopf, P. W. Keaton, P. W. Lisowski, and L. R. Veeser at Los Alamos [104] had been commissioned by vested interests during the consideration process of paper [103], as indicated on pp. 163–168 in this book. If these rumors are even partially true, they provide credibility to the idea that the same group of people, whether APS editors or members, are responsible for the chain of events reported in this section, such as:

1– The lack of cooperation in 1979 for the identification, in the papers published in APS journals, of the unverified character of Einstein's special relativity in the interior of hadrons;
2– The lack of interest in the moratorium at the Hadronic Journal of 1980 on nonrelativistic quark conjectures because of excessively big inconsistencies;
3– The repetitious rejections of my paper of 1980 indicating the need to test the rotational symmetry while the only available direct measures show violation;
4– The apparent editorial misconduits in the handling of experimental paper [103] on the origin of irreversibility;
5– The apparent commissioning of the Los Alamos rebuffal [104] rushed up during the consideration process of paper [103];
6– The rejection of experimental paper [105] confirming the original measures [103];
7– The rejection of the theoretical paper [59] on irreversibility;
etc., etc., etc.

In turn, all these alleged, scientifically evil actions create serious doubts on the existence of a CONSPIRACY at the journals of the American Physical Society for the purpose of suppressing the achievement of potentially fundamental, novel, human knowledge that is contrary to vested interests in U.S. physics, or jeo-

pardize the orderly scientific process of acquisition of novel physical knowledge, that via the PUBLICATION of plausible conjectures, followed by the PUBLICATION of their independent critical appraisals. The complete alignment of behaviour among Adair, Sommerfield, Trigg and possibly other APS editors, the demonstrable lack of qualifications of the referees, the lack of credible scientific criticisms in the rejection of papers [59, 105], and numerous, additional, scientifically evil aspects, are per se sufficient prerequisites for a conceivable conspiracy at APS journals.

Whatever the truth, one thing is certain: the current editorial—refereeing practices at the journals of the American Physical Society are undignifying for the United States of America.

The crossing of the Rubicon.

In one of my last letters to the APS "editor" in chief, D. Lazarus, I stressed that paper LR2111/LZ2206 was my scientific Rubicon (p. II—642). The American Physical Society should identify credible errors and/or insufficiencies in the paper, in which case I would be only grateful. Lacking a true scientific process, I had to follow what I considered necessary for the future of my children: inform the fellow taxpayer. In fact, I stressed to Lazarus that his action (p. II—642)

> *". . . .contains absolutely no light, by therefore confirming the only alternative left to physicists concerned for the future of their children: GO PUBLIC, GO PUBLIC, GO PUBLIC."*

And that is exactly what I did with IL GRANDE GRIDO. In fact, this book is my Rubicon.

Epilogue.

I feel obliged to express my disagreement with A. B. Giamatti, president of Yale University, on a number of grounds of scientific ethics and societal accountablility. My requests that R. K. Adair and C. M. Sommerfield, of the Department of Physics of Yale University, resign from all their editorial functions at APS journals because of apparent editorial misconduits, should have been, ABOVE ALL, subjected to an in depth, comprehensive, and public investigation by Yale University. Following my detailed reports, their considerable enclosures, and my offer for additional information and assistance (pp. III—675—676), Giamatti elected to conduct no action visible from the outside of his campus. This implied a *de facto* backing by Yale University to the faculty members Adair and Sommerfield in regard to their APS functions. In turn, such a *de facto* backing implied, on one

side, the unperturbed continuation by Adair and Sommerfield of their editorial–scientific practices and, on the other side, the dilation of the responsibility from Adair and Sommerfield as individuals, to Yale University as an institution.

No administrator of a leading U.S. academic institution can, or should be permitted to, ignore even minute shadows of ethically questionable behaviour of his/her faculty, particularly when such behaviour invests a public function. When this function consists of public activities so vital to the scientific, economic and military interests of America, such as editorial functions at primary scientific journals, the silence by college administrators simply cannot but be interpreted as potential complicity.

For these and other reasons, no *bona–fide* member of a truly free society can remain in peace until full light is thrown, not only on the apparent editorial misconduits by R. K. Adair and C. M. Sommerfield AS INDIVIDUALS, but also on the apparent responsibility by Yale University AS AN INSTITUTION.

My scientific disagreement with D. Lazarus, editor in chief of the American Physica Society, is so manifestly irreconcilable, to demand no additional comment here. According to his own communication, and contrary to his title, Lazarus is not an editor. If this is correct, Larazus cannot therefore be charged with editorial responsibilities on the several cases reviewed here (and numerous others I could not possibly review for brevity). Nevertheless, Lazarus himself admitted to the administrative responsibility of his post (p. II–637). This is the function for which I had contacted him in the first place, and that is the function in which he disappointed me most. In fact, a primary reason for my contacting Lazarus as APS editor in chief (pp. II–590–623) was to recommend an in depth investigation to ascertain whether or not a scientifically evil conspiracy was under way within his journals along the lines reviewed above. Besides expressing his personal belief on the lack of existence of such an alleged conspiracy (p. II–623–624), Lazarus failed to conduct any credible consideration of the allegation, that is, he failed to organize a public investigation of the allegation conducted by credible persons, such as persons OUTSIDE THE APS AND WITH A NOTORIOUS LACK OF ALIGNMENT WITH THE VESTED, ACADEMIC–FINANCIAL–ETHNIC INTERESTS INHERENT IN THE CASE. Lacking a suitable action at least minimally commensurate to the seriousness of the allegations, Lazarus has done nothing but create a further deterioration of the case, by multiplying the unanswered questions everybody can readily formulate independently.

But again, my personal opinion is insignificant. Equally insignificant is the personal opinion by Giamatti at Yale, or Lazarus at the APS. The only important opinion is that by the fellow taxpayer. This book merely provides information useful for the taxpayer's achievement of independent judgment.

During the consideration of the case, I beg the fellow taxpayer to go back to the true values of this Land. **The future of America, that is, the future of our children, is heavily dependent on the capability of the Country to achieve NOVEL physical knowledge. But such a knowledge can be best achieved via the traditional scientific process: PUBLICATIONS of plausible conjectures followed by PUBLICATIONS of their independent appraisals. Particularly essential for the effective achievement of novel knowledge is the implementation of a true intellectual democracy, where the PUBLICATION OF PLAUSIBLE DISSIDENT VIEWS is lifted to a sacred level. By keeping in mind that ALL PUBLICATIONS AT THE FRONTIERS OF KNOWLEDGE ARE CONJECTURAL, if editors of nationally relevant journals organize themselves to publish selectively only certain classes of conjectures and reject all the others, they become arbiters of the direction along which the research will be conducted, thus acquiring immense scientific power with commensurate responsibility and accountability. If, in addition, the same groups of editors systematically suppress the publication of all plausible dissident views, then they commit a crime against society which, even though permitted by the current code of laws, implies societal damages far greater than those produced by ordinary crimes. The end result under these premises will certainly be beneficial to the vested, academic—financial—ethnic interests preferred by said groups of editors, but it can only be of sinister value for America and mankind.***

*By no means, the problems of scientific ethics at physics' journals occur only at the American Physical Society. In fact, similar problems exist also at other journals scattered throughout the world. A rather visible case is that of PHYSICS LETTERS B, a letter journal in nuclear and particle physics which is considered to follow closely PHYSICAL REVIEW LETTERS in academic prestige. As one can read in the cover of the journal, the sole editor for countries outside Europe is Howard Georgi of the Department of Physics of Harvard University. This implies, in particular, that Georgi has a totalitarian control of ALL submissions from the U.S.A. I believe that this situation is damaging the scientific process and, consequently, Georgi himself as well as the journal. I had a first taste of Georgi's refereeing in 1982 when he rejected a paper of mine via unconvincing arguments (the paper was readily published in another refereed letter journal). A documentation of Georgi's refereeing at PHYSICS LETTERS B is presented in pages II—734—745. It regards a second paper which was rejected without any visible or otherwise credible, technical and/or editorial reason. I submitted the paper to R. Gatto, an European editor of the journal, precisely to avoid Georgi's review (p. II—735). But Gatto promptly remailed the paper to Georgi, thus confirming his totalitarian control of submissions from the U.S.A. The exchange of letters that followed between Georgi and I (pp. II—736—744) are useful for anybody interested in an independent appraisal of the soundness of Georgi's (or Harvard's?) review. Predictably, the topic of the paper was essentially that considered of "no physical value" by senior physicists at Harvard University during my visit there in 1977—1980 (Section 2.1). On a rather aligned basis, Georgi rejected the

2.5: U. S. GOVERNMENTAL AGENCIES

I now pass to the outline of my personal experience with U.S. Governmental Agencies in charge of the consideration, acceptance or rejection of research grant proposals. I should indicate from the outset that the terms "Governmental Agencies" refer only to the National Science Foundation (NSF) and the Department of Energy (DOE). The National Areonautics and Space Administration (NASA) should be excluded for the reason that I have never applied to NASA for a research contract. Military Agencies should also be excluded. Furthermore, the considerations of this book apply solely to the NSF Divisions of Physics and Mathematics, and to the DOE Divisions of Nuclear and High Energy Physics, and they should not be construed as being necessarily applicable to other divisions of the same Agencies. This is due to the fact that my personal experience is limited to the divisions specified above.

The achievement of a mature, independent, and in depth appraisal of the operations of Governmental Agencies demands, among others, information on FUNDED research and on REJECTED applications. The need for both is evident. In fact, only a comparative analysis between funded and rejected applications can provide the necessary elements to achieve an independent judgment, that is, a judgment independent from vested, academic—financial—ethnic interests in the U.S. physics.

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paper with the statement (p. II—738), among others, that *"I do not know whether your whole program makes any sense because I have not studied it deep enough (although people I respect have studied it and claim that it doesn't)"*. The paper, rather brief and concise (being intended for a letter journal), essentially indicated the possibility of regaining the space—reflection symmetry in weak interactions via the generalization of the quantum mechanical unit, from its current (constant) form, to the generalized operator form of hadronic mechanics. I sincerely regret the eipsode and my impossibility to prevent it. Indeed, owing to my former editorial association with Georgi, Gatto should have reviewed the paper himself. As an incidental note, I should indicate here that the HADRONIC JOURNAL has an editorial organization conceived precisely to avoid territorial control by individual editors. In fact, authors can select the editor they prefer, thus permitting papers written in the USA to be reviewed by European editors and viceversa. Additional journals deserving an independent appraisal of their practices are: NUCLEAR PHYSICS (pp. II—690—699); JOURNAL DE PHYSIQUE (pp. II—700—706); LETTERS IN MATHEMATICAL PHYSICS (pp. II—734—745); and others. Regrettably, I cannot review my personal experiences with these latter journals to avoid excessive length. I must therefore refer the interested reader to the above quoted documentation. The bottom line is however always the same: selective publication of plausible conjectures aligned with vested interests in the field, and suppression of equally plausible, but non—aligned conjectures, in disrespect of scientific democracy and the advancement of human knowledge.

The gathering of the information on FUNDED applications is easy. This profile will therefore be ignored hereon. The scanning of articles in physical and mathematical journals will provide the necessary information (Governmental support must be listed in the front page of each article). At any rate, the information is expected to be of public domain and, as such, to be available from each Agency. The scope of this section is to provide the fellow taxpayer with a documentation of REJECTED applications which, unlike that of funded ones, is much more difficult to obtain from both applicants and Agencies alike.

I shall begin by providing the fellow taxpayer with a "taste" of NSF's processing of research grant applications in theoretical physics not aligned with fashionable trends. I shall then pass to an outline of rejections I have experienced over a fifteen year period at NSF and DOE, first, as an individual, and then as president of a research institution. These personal experiences are important to appraise the constructive suggestions submitted in the next chapter. A knowledge of the scientific issues outlined in Chapter 1 is essential for an in depth understanding of this section.*

By no means, my experiences constitute isolated cases. In fact, if we exclude the few leading physicists at leading institutions and their direct pupils, the malcontent in the physics community on the current structure, operations and staffing of Governmental Agencies has reached widespread manifestations in departmental meetings, international conferences, journals, etc. We have now reached such a point that the preservation of the *status quo* may imply lack of political sensitivity in the Country. We may disagree on what to do, but one thing is certain: profound revisions of current structure, operations and staffing of Governmental Agencies MUST be implemented.

2.5.1: DIVISIONS OF PHYSICS AND MATHEMATICS OF THE NATIONAL SCIENCE FOUNDATION.

An old, rather incredible rejection by NSF in 1977.

During the fall of 1977, while at the Lyman Laboratory of Physics of Harvard University, I received almost simultaneously:

*As indicated in Section 1.6 (pp. 120–123), I did apply or contact Military Agencies for potentially classified research originating from the studies reviewed in this book. All I.B.R. applications submitted to the Defense Advance Research Project Agency (DARPA), a Division of the Department of Defense, and to the U.S. Air Force Office of Scientific Research (USAFOSR) were rejected, while other Military Agencies even discouraged the applications. As indicated earlier, all the correspondence regarding these rejections have been removed from the Documentation of this book because of potentially sensitive material.

(a) the acceptance by Springer–Verlag (a publishing house from Heidelberg, West Germany, which is renowned for postgraduate books in physics and mathematics) for the publication of monographs [9, 10]; and,

(b) the rejection by the National Science Foundation of a research grant application I had submitted to its physics division in October, 1976 (Doc. pp p. III–755), precisely for the completion of monographs [9, 10].

The application (NSF number PHY77–03963) evidently included a draft of the monographs. It was processed by Boris Kayser, NSF Program Director for Theoretical Physics. Kayser's processing was formally reviewed and accepted by Marcel Bardon, Acting Division Director for Physics (pp. III–756–774). The rejection was based on referees' reports of the following type solicited, reviewed, accepted and released by Kayser and Bardon (p. III–771):

"I have examined the proposal by Dr. Ruggero M. Santilli PHY7703963 (returned under separate cover). My reaction to it is rather negative. I also thought that Santilli was on the borderline between being a third rate scientist and a crack pot and I do not think that the monumental work can change substantially my opinion. The idea of reading it thoroughly produces in me an incoercible revulsion and if you insist on it I am going to resign as a reviewer. The book is written in a pompous, immodest, self-glorifying style which I detest given also the absolute lack of physical content. In view of this criticism I find the total figure asked for the project quite extraordinary."

The recollection of my first contact with Americans, while I was a young boy in Italy, during World War II.

When I received the above referee's report, my mind instinctively turned back in time, to my recollections as a young boy, when I was among the first to greet American Soldiers who had liberated my town (Agnone, currently in the province of Isernia) during World War II. That was the birth of my sincere admiration and devotion toward the U.S.A. which subsequently grew in time. In fact, during my high school studies I noted that, having been conquered in war, Italy should have been a country controlled by the U.S.A. at least in the same measure as that existing at Eastern European countries. Instead, I was seeing around me free people among free, democratic institutions. The voluntary relinquishing of the control of Italy by the U.S.A. could only indicate to my young eyes a superior nobility in the conception of life.

The reception of the above quoted referee's report brought

me to the reality of the facts that the U.S.A. is not perfect. Nevertheless, the eipsode did not weaken, even minimally, my faith toward the country. Instead, the episode reinforced the determination to provide my own contribution to America, for whatever its value, which lead later on to the decision to write IL GRANDE GRIDO.

America is a Country founded by immigrants that continues to be shaped by immigrants to this day. As an immigrant, I intend to raise my voice as loud as conceivably possible to denounce the current NSF operations as undignifying for the U.S.A., let alone scientifically damaging to the Country.

The senseless character of the episode.

To begin the understanding of the case, the fellow taxpayer must know that, at the time of filing the application, I was an obscure young physicist working alone in my own corner. Also, at that time, I still had the illusion of reaching a "tenured" (permanent) academic job in the U.S.A. I therefore avoided any conflict with colleagues inside and outside my campus. Finally, I am referring to a period of time prior to the publication (or even informal release) of my doubts on the validity of Einstein's ideas in the interior of hadrons. In short, at the time of application PHY77—03963, I could not possibly have represented a threat to anybody.

But then, why did the application have to be rejected via offensive language such as that above? After all, the application could have been rejected via a few dry lines without any need for additional comments.

The affair remains, for me, beyond a rational explanation. Its senseless character is much similar to my experience at that time, when I was a formal member of the Lyman Laboratory at Harvard, yet I was prevented by my senior colleagues to draw a salary from my own grant (see Section 2.1, pp. 194—195 of this book).

The necessarily ungraceful reaction.

I am convinced that it is the duty of any responsible member of the U.S. physics community NOT to accept gracefully offensive language in referees' reports on technical material, whether from Governmental Agencies or the American Physical Society.

As soon as I received the above referee's report, I therefore initiated a number of intentionally ungraceful actions. First, I consulted a law firm in the Boston Area and initiated the search of a corresponding law firm in Washington, D.C., for the purpose of FILING LAW SUITS, PERSONALLY, AGAINST BORIS KAYSER AND MARCEL BARDON AS INDIVIDUALS, AND NOT AGAINST THE NSF AS AN INSTITUTION. The NSF

statute is not expected to authorize its officers to accept offensive language in the review of technical material. The sole responsibility of the case therefore appears to rest, personally, on Kayser and Bardon as individuals.

Furthermore, I applied to NSF for a reconsideration due to manifest improprieties in the processing of the application itself. The hot ball was passed by Bardon to James Krumhansl, NSF's Assistant Director, via Ronald E. Kagarise, NSF's Deputy Assistant Director. In this way, the reconsideration process was formally initiated (pp. III—776—802).

Jointly, I expressed my indignation to the NSF Director General of that time, and to the highest Officer of the Country. This action lead to the appointment of Wayne R. Gruner, NSF's Special Assistant to the Associate Director for Mathematical and Physical Sciences, as the officer in charge of my case.

When, in early 1978, Harvard University finally filed the necessary documents to the DOE following its offer to support my research (Section 2.1), I contacted Gruner to withdraw the reconsideration of the case (p. III—791). Gruner reacted promptly (p. III—792) by indicating *"my pleasure and the pleasure of the Foundation"* in regard to the DOE support.

The roots of the affair.

It is evident that the rejection of the application is not the issue here. After all, NSF routinely receives qualified physical applications for sums exceeding its physics budget. My indignation was due to the senseless use of offensive language in the rejection. In fact, I saw it as a sign of decay of the Country in one of its most vital function: the pursuance of novel physical knowledge.

At the peak of my protests, I pounded Gruner with letters and phone calls to obtain more information so that I could reach the roots of the affair. I wanted to know more about the criteria for selection of the referees, and the NSF processing of their reports. In particular, I wanted more information on the referees' academic status and affiliation.

At one point, during a rather heavy phone conversation, Gruner acknowledged that the auther of the report reproduced at the beginning of this section was *"a truly renowned physicist at a leading U.S. institution"*. My pressures to know whether that institution was Harvard evidently remained without confirmation (but also without denials).

As a result of a considerable experience accumulated over more than fifteen years, I believe that officers of the physics divisions of U.S. Governmental Agencies are servants to leading physicists at leading academic institutions, not only collectively, but also individually.

Whether this is true or false, one thing is certain: manifestly offensive referees' reports must be returned to the referees,

rather than being released to the authors. After all, authors are not permitted the use of offensive language in their papers, books or grant applications! It is evident that Boris Kayser and Marcel Bardon should have rejected the above referee report and terminated the use of this referee because of its manifestly offensive language, let alone the total lack of scientific content needed to pass judgment on the application. The issue left to the taxpayer is the identification of the most probable reasons why Kayser and Bardon DID NOT reject the report and submitted themselves to the referee's threat: *". . if you insist on it I am going to resign as a reviewer."*

The litany of NSF rejections; Part A: Rejections prior to the founding of the IBR.

NSF has rejected ALL research grants applications I have submitted, first, as an individual, and then as IBR president on behalf of fellow mathematicians, theoreticians and experimentalists. I am referring to a considerable number of rejections over a period of about fifteen years. The list of rejections provided below is therefore only partial because the documentation of the early applications has been lost.

NSF REJECTION NO. 1 dated September 22, 1972, (p. III–752), of an application entitled "Investigations on a new analytic extension of the scattering amplitude". The application was connected to the paper submitted to Phys. Rev. D regarding the identification of the limitations of Wightman's axioms (p. 251–252 of this book).

NSF REJECTION NO. 2 dated July 16, 1975, (p. III–753), of an application entitled "Investigations of generalized analytic, algebraic and statistical formulations for interacting systems". The proposal was preparatory to the studies that lead to the Birkhoffian generalization of Hamiltonian mechanics (Section 1.3).

NSF REJECTION NO. 3 dated June 28, 1976, (p. III–754) of an application entitled "Investigations on the origin of the gravitational field". The application dealt with: the possible electromagnetic contribution to the origin of the gravitational field; the possible, consequential, elimination of the vexing problem of the unified field theory; and the proposal of experiments conceived to test, at some future time, the foundations of current gravitational theories,* along the lines discussed in Sec-

* As one can read in ref. [40], the proposal included the submission of experiments on the "creation" of the gravitational field of matter, via a suitable distribution of electromagnetic fields patterned along the electromagnetic structure of material bodies without any mass contribution. Once the mechanism of creation of the gravitational field is understood, far reaching advances are conceivable at the frontier of imagination and beyond. The truncation of research indicated below in the text refers to all these developments.

tion 1.4 (ref. [40]). The rejection lead to my decision to terminate research in gravitation, owing to the extremes of the problems of scientific ethics in the field as outlined in Section 1.4.

NSF REJECTION NO. 4 dated June 30, 1977, (p. III—769), of an application entitled "Necessary and sufficient conditions for the existence of a Lagrangian in Newtonian mechanics and field theory". This is the sample rejection reviewed at the beginning of this section.

NSF REJECTION NO. 5 of support for the "Third Workshop on Lie-admissible formulations"(see p.III—803 for the application; the papers of the rejection could not be identified at the time of printing this book and are not present in the Documentation). For the taxpayer's convenience, let me recall that this is the international meeting that Harvard University prohibited to keep on campus (pp. 200—202 of this book). Also, this is the meeting that initiated our experimental study of the insufficiencies of Einstein's ideas in the interior of hadrons (see the contributions by experimentalists in the third volume of the proceedings, ref. [125]). The application was processed by L. P. Bautz, as Deputy Director of the NSF Division of Physics. Boris Kayser, however, was still in charge of the NSF theoretical physics programs. A short time before the initiation of the Workshop, certain of the NSF rejection because of the lack of decision with sufficient notice,* I called Kayser at NSF pressing for a resolution of the case. Kayser acknowledged the rejection. I asked him whether he was aware of the fact that the application dealt with the SOLE meeting in the U.S.A. which was critical of orthodox doctrines for hadrons. Kayser answered *"Yes"*. I still remember vividly my comment: *"If NSF were to disperse 99% of the budgetary funds in strong interactions to research aligned with quark conjectures, and 1% to non—aligned research, I see no problem. However, since NSF disperses 100% of the funds to quark oriented research and absolutely nothing to dissident views, I see the existence of a BIG, BIG PROBLEM OF TOTALITARIAN DISPERSAL OF PUBLIC FUNDS AT THE DIVISION OF PHYSICS OF NSF."*

*A rather peculiar aspect of NSF operations is that of delaying the communication of rejections of applications for scientific meetings in physics. This forces the organizers to solicite a resolution, so that they can, in turn, communicate the decision to the participants. I have experienced this occurrence a sufficient number of times (see below) to suspect a repetitive pattern. The antiscientific nature of this practice is evident. In fact, it leaves the entire organization of the meeting in suspended animation, thus providing evident scientific damages. Apparently, the practice is not implemented for meetings which, even though not funded, are nevertheless aligned with vested interests in academia. Instead, it appears that the practice is implemented specifically for meetings, such as those I applied for, which are manifestly non—aligned with vested interests. This aspect alone is so diversified, to require a separate, detailed investigation.

The litany of NSF rejections; Part B: Rejection of the primary IBR application.

NSF REJECTION NO. 6 dated March 3, 1983, (p. III–861), of the primary, I.B.R. group application entitled "Studies on Hadronic Mechanics", NSF number PHY83–00195. The NSF officer in charge of the application was this time S. Peter Rosen, Program Associate of the Theoretical Physics Program. The NSF officer that reviewed and accepted Rosen's processing of the application was, again, Marcel Bardon, this time as Director of the Division of Physics (pp. III–847–868).*

The application dealt with comprehensive, mathematical, theoretical, and experimental studies on the construction of the so–called hadronic generalization of quantum mechanics (a new mechanics specifically conceived for the interior of hadrons as outlined in Chapter 1, Sections 1.6 and 1.7 in particular). The application involved a number of senior mathematicians, theoreticians and experimentalists, whether formal members or only affiliated to the I.B.R. Part of the application included the organization of workshops and conferences, as done for the preceding, fully successful research program that lead to the construction of the Birkhoffian generalization of the classical Hamiltonian mechanics. The application was divided into branches, essentially dealing with nuclear physics, particle physics and experimental physics, each branch with its own leader. The application indicated the possibility that hadronic mechanics, rather than being against physical knowledge acquired via quark conjectures, could be of assistance in the future resolution of some of their problematic aspects, such as the achievement of a strict confinement of quarks or the identification of the quark constituents with physical, directly detected, particles (see pp. 126–129 of this book). As I.B.R. president, my role would have been essentially that of coordinator of the various branches of the project and co–organizer of the various meetings.

The note of rejection, signed as usual by Marcel Bardon, was dated March 3, 1983, (p. III–861). The reading of the referees' reports (pp. III–862–863) is quite instructive.

An excerpt from the first referee's report (p. III–862):

". . . I fail to see any results that are remotely persuasive or inspiring to the physicists at large. The author [sic] quotes one experimental paper on time reversal violation as a support for his ideas, but that paper is now discredited . . . [by] Hardekopf et al. Phys. Rev. C25, 1090 (1982)."

To understand this comment in full, it is essential for the fellow taxpayer to have a knowledge of the scientific background considered previously with particular reference to: pp. 101–109 (lack of unity of contemporary physical and mathematical thought); pp. 160–168 (the apparent commissioning by vested

* All names of I.B.R. applicants have been deleted in the Documentation.

interests of the experiment by Hardekopf et al quoted by the referee, during the consideration process by Physical Review Letters of the original results of the Québec—Berkeley—Bonn group, ref. [103]); pp. 257—273 (rejection by APS journals of a theoretical and an additional experimental paper on time reflection violation); pp. 261—262 (the potential scientific corruption in the APS referee process because of the impossibility of deciding at this time which of the opposing experimental data are right and which are wrong); etc.

To see further the alignment of the above NSF referee report with the APS referee reports reviewed in Section 2.4, it is sufficient to recall that the rejection by APS journals of the theoretical and experimental papers on time reflection violation, and the NSF rejection of the primary I.B.R. research grant proposal occurred one after the other, the APS rejections being evidently the first.

But, above all, the fellow taxpayer should know that the irreversibility of proposal PHY83—00195 was referred to OPEN, NONCONSERVATIVE conditions of particles, that is, conditions whose irreversibility has been established since the early days of quantum mechanics. The reference to Hardekopf et al. in the above referee report, not only was a manifestation of potential scientific corruption (for the reasons indicated earlier), but also of total lack of scientific appropriatedness for the case considered (in fact, Hardekopf et al aim at CLOSED, CONSERVATIVE conditions).

Despite these aspects, conveyed repetitiously to NSF officers during the consideration process, S. Peter Rosen and Marcel Bardon accepted the above referee's report to reach a formal decision of an Agency of the United States of America involving the dispersal of public funds!

An excerpt from the second referee's report (p. III—863): *". . . In the past five years, he [Santilli] and his followers have produced no solid achievement worth mentioning."* I detest to be vane. Yet, the fellow taxpayer must know as an example that our group has produced an entire new branch of classical mechanics, the Birkhoffian generalization of the conventional Hamiltonian mechanics along the lines reviewed in Section 1.3. The new mechanics was named after Birkhoff (father) because of historical reasons reviewed in the original publications. While the old Hamiltonian mechanics can effectively treat Newtonian systems only of perpetual—motion—type, the new mechanics is "directly universal" for ALL Newtonian systems verifying certain topological conditions, thus including the realistic systems of our environment. The new mechanics was assumed at the foundation of the hadronic mechanics in the NSF application. Thus, the NSF referee AND officers simply cannot deny its knowledge. Yet, this scientific event was not considered a "solid achievement worth mentioning".

At this point, to reach a minimum of credibility, the U.S.

National Science Foundation should exhibit AT LEAST ONE EXAMPLE of a "solid achievement worth mentioning" reached under NSF support DURING THE SAME PERIOD OF PROPOSAL PHY83–00195. This latter point is evidently crucial to conduct a meaningful comparison among the applications REJECTED and those FUNDED by NSF during the same period.

An additional excerpt by the second referee's report: *". . . None of their papers, except for one, were published in regular refereed journals where most of major mathematical and physical works have been published."* This is a documentation of the point raised in the preceding section, regarding the deep interdependence of editorial processing at APS journals and review processing at Governmental Agencies. Often, the same leading physicist at a leading academic institution suppresses, on one side, the birth of plausible fundamental advances in APS journals, while, on the other side, rejects research grant applications in the same topic, on grounds that the argument has not appeared in "regular refereed journals"!

An excerpt of the third referee's report (p. III–864): *". . .this research has been founded by DOE for the past four years. The results of this DOE supported work appear to have been nil."* I must be vane here and claim that our group has indeed achieved: (A) the identification of numerous reasons leading to the invalidation of Einstein's relativities in the interior of hadrons as well as under strong interactions at large; (B) preliminary, and tentative, yet SPECIFIC AND CONCRETE GENERALIZATIONS of Galilei's [8, 10], Einstein's special [32] and general [50] relativities verifying theorems of direct universality; and last but not least, (C) the formulation of experiments for the resolution of the validity or invalidity of Einsteinian theories under the conditions considered (to avoid the quotation of others at this point, see, for instance, the experiments proposed in ref.s [49, 62] printed prior to proposal PHY83–00195).

All these aspects were reviewed and itemized in the proposal as well as in the various correspondence. Yet, the NSF referee/officers claim that these results are "nil". The task left to the fellow taxpayer is therefore that of reaching an independent judgment whether these results are indeed truly "nil", or they are "nil" only because contrary to the vested academic–financial–ethnic interests of the referee and/or of the NSF reviewers.

An excerpt from the third referee (p. III–865): *"The principal investigator, R. M. Santilli, has a very poor reputation among mathematical physicists and elementary particle physicists."* To appraise this statement, the fellow taxpayer should differentiate the community of mathematical and particle physics into two categories, a first one with vested interests on the preservation of Einstein's theories for personal gains, and a second one with a view of their possible generalizations for the

advancement of human knowledge. There is little doubt that I am one of the few, independent, theoreticians who have proved to possess sufficient courage to PRINT their view on the possible invalidation of Einstein's ideas in the interior of hadrons. The fellow taxpayer can then decide whether my "poor reputation" is established in both groups or only in one of them, evidently the first. Speaking on personal grounds, I feel praised by the fact that I have a poor reputation among vested, academic—financial—ethnic interests on Einstein's theories. In fact, such a "poor reputation" is a NECESSARY QUALIFICATION FOR INDEPENDENCE OF INQUIRY AND NOVELTY OF THOUGHT.

The termination of contacts with Larry C. Biedenharn, Jr., of the Department of Physics of Duke University, Durham, North Carolina.

The second referee concluded the report with the following statement (p. III—863): *"I recognize only two names of theoretists among those quoted by Santilli. They are [S.] Okubo [of the University of Rochester, New York] and Biedenharn. The latter declined joining Santilli according to a copy of the letter. [A rather mysterious blank space occurs at this point prior to the resumption of the report]."*

The fellow taxpayer should know that Biedenharn was an advisor of the proposal, that is, he would have been consulted on specific technical aspects in his field. Evidently, Biedenharn had been listed in the proposal following his formal, written, authorization. I never received any communication by Biedenharn whether verbal or in writing of his intention to withdraw from the project. The referee's statement quoted above therefore leads to the idea that this referee had not stopped short of recommending rejection, but had additionally attempted to discredit the proposal and its authors at NSF, by going further ahead to the point of contacting directly one of the senior members of the proposal (Biedenharn) and securing a copy of his (apparent) withdrawal from the project.

The fellow taxpayer should then decide whether or not the affair verifies all the standards of scientific ethics needed for the dispersal of public funds at an Agency of the United States of America, or we are facing corrupt practices. As far as I am concerned, I see in the too many episodes of this type the completion of the cycle of information indicating the existence of a scientific obscurantisms on Einstein's theories under way in the U.S. physics, as illustrated in my preceding experiences at leading academic institutions, Federal laboratories, and journals of the American Physical Society.

In regard to Biedenharn, despite my sincere regrets and contrary to my best desires, I evidently had no other choice than to terminate all contacts, as I did with a certified letter, return receipt requested (p. III—876).

The rather incredible alignment of the five NSF reviewers.

Besides the apparent scientific corruption in the referees' report and their total lack of scientific content, a most visible aspect is the rather incredible alignment of all the reports toward the rejection of the application. To understand this point, the fellow taxpayer should keep in mind that:

(1) The application had been filed by a new institute of research founded by individual scholars without any governmental support. The decision to fund or reject the application would therefore have had a clear, large, bearing on the decision whether to maintain or suppress the new institution.

(2) The application was not filed by an individual. Instead, it was a group application involving an international team of senior experimentalists, theoreticians, and mathematicians in some seven different Countries.

(3) Even ignoring points (1) and (2), the topic of the application was TO DEVELOP A NEW MECHANICS, THAT IS AN ENTIRE NEW BRANCH OF HUMAN KNOWLEDGE. To understand this point, the fellow taxpayer should keep in mind that new mechanics are created quite rarely through the course of a century. Also, the hadronic mechanics submitted for development, was not the dream of a "crackpot". Instead, its mathematical existence and consistency had been independently proved by mathematicians at the Orléans International Conference of 1981, as recalled in the proposal itself. Finally, the hadronic mechanics, being a covering of quantum mechanics, not only contains the latter as a particular case, but the latter can be approached as close as desired, thus rendering inevitable physical applications in the interior of nuclei, of hadrons and of stars.

Despite these manifestly unique aspects, all five different referees aligned themselves in a truly incredible way toward the vigorous rejection of the proposal. Only inepts and accomplices will see in this a normal routine. Persons who care about the Institutions of this Land and what they represent to the Free World must do much better and be alert, if they are truly committed to the preservation of these Institutions. We must acknowledge that the chances for five seemingly independent reviewers to reject the proposal vigorously are virtually null under premises (1), (2) and (3). We must acknowledge the possibility that something was done by the NSF officers at least to facilitate, if not to encourage the alignment. For that, it would have been sufficient that the NSF officers first, selected for reviewers

known representatives of vested, academic—financial—ethnic interests; and, second, the officers informed at least ONE of them (say, the most representative) of the names of the others. The strict alignment of all of them toward the suppression of due scientific process at an agency of the U.S.A. would be a trivial consequence under these premises. In fact, the mutual loyalty among members of said interests is known to be so strong to dwarf the mutual loyalty within circles of organized crimes.

One thing is certain: when an NSF referee contacts a member of the team of applicants (L. C. Biedenharn, Jr.), to discourage his participation and to secure the documentation of his withdrawal while putting all this in plain light, THAT REFEREE MUST BE CONSIDERED CAPABLE, IN THE DARK, OF ANY CONCEIVABLE SCIENTIFIC CRIME.

The litany of NSF rejections; Part C: Rejections of applications by individual IBR members.

NSF REJECTION NO. 7 undated (received sometime in September, 1982) of an application by L.L.L., a senior IBR physicist, entitled "Variational method of calculating structural properties of solids". The rejection was signed by Lewis H. Nosanow, Acting Division Director of NSF Material Research (p. III—886).

The field of the application is outside my expertise and, as such, I cannot pass any judgment here on the possible scientific merits of the proposal. There is however a human aspect that is worth bringing to the attention of the fellow taxpayer. After all, advances in human knowledge are not made by machines, but by human beings. No society has a true, long term, scientific future unless the human aspect is provided with priority over all technical issues.

L.L.L. is a senior jewish physicist who had managed to leave the U.S.S.R. with his wife and son. When he came to me, he was unemployed with a family to support. He therefore reminded me of the experience at Harvard, when the triplet Coleman—Glashow—Weinberg prevented my drawing a salary under my own grant for feeding and sheltering my family. I therefore provided L.L.L. with my best assistance, which included: contacting all possible Governmental Agencies interested in considering his proposal; paying personally all duplicating and other expenses for the various submissions (three different applications were finally selected, all rejected); contacting jewish foundations in the Boston area for possible assistance to L.L.L. (only, WITHOUT overheads to the IBR); etc. I must admit that I failed on all these counts, by therefore resulting in the impossibility of providing any financial support to L.L.L. The fellow taxpayer must decide whether this was my personal failure, or a failure of the current U.S. physics community.*

* As an incidental note, L.L.L. had reached a senior status as a physicist

NSF REJECTION NO. 8 dated June 13, 1983 (p. III–911), of a proposal entitled "Fifth Workshop on Lie–admissible Formulations". The proposal was authored by four senior mathematicians of the IBR (each holding a joint full professorship in mathematics at other, large, U.S. academic institutions). The proposal was processed by Alvin Thaler, Director of Special Programs at the NSF Division of Mathematics (this is the division handling workshops and conferences). The rejection was signed by E. F. Infante, Director of the NSF Division of Mathematics and Computer Sciences.

The fellow taxpayer should be aware of the fact that in the preceding four meetings of the series, we had conducted jointly mathematical and physical research. However, as clearly stated in the proposal, the fifth workshop of this series was restricted to pure mathematics. In particular, since I am a physicist, I was strictly excluded in the presentation and in the program.

The application was evidently rejected. Again, it is not the rejection per se, but rather some of its rather peculiar aspects that are suitable for reflections. First and above all, the seniority and qualification of the applicants were absolutely impeccable. Second, the topic dealt with a generalization of a truly fundamental part of contemporary mathematics, the Lie–admissible generalization of Lie's theory (see Section 1.8). Rejections under these premises, particularly when compared to the modest amount of funds requested (a few thousand dollars), are already sufficient to motivate the suspicion of possible scientific manipulations at NSF. A number of additional elements do nothing but reinforce such a suspicion. Unlike other programs, the NSF budget for mathematical conferences was fully funded in the period of the proposal, to the point that NSF regularly advertized the availability of funds and solicited the submission of proposals in the Notices of the American Mathematical Society. **Under these premises, the rejection does not appear to have been motivated by the lack of funds.**

The fellow taxpayer would then expect that the rejection was motivated by poor referees' reports. This is not true. Each and every referee report rated the proposal "GOOD" as the fellow taxpayer can verify (pp. III–912–915). **As a result, the proposal does not appear to have been rejected because of lack of qualifications of the applicants, or because of lack of positive referees' reports, or because of lack of funds.**

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while in the U.S.S.R. As such, he had acquired a considerable, if not unique knowledge of the condition of the research in the field in that Country. In his application he had made a reference to this aspect, by indicating his willingness to cooperate for his new Country. The last NSF referee commented on this delicate, tastefully presented point of the application with the statement: ". . .the Russian Menace can safely be ignored in the field for quite a while." Fellow taxpayer, do you think that this represents a responsible way of processing your money at the U.S. National Science Foundation?

BUT THEN WHY, AND ON WHAT GROUNDS, NSF REJECTED THE PROPOSAL?

The most plausible answer under these premises is evident: because of what is sadly known as "NSF politics" (see the comment at the end of this sub–section).

As an incidental note, I should report how we finally received the communication of rejection. The NSF Division of Mathematics had indicated the need of six months for the processing of the application. In 1983, well over the expiration of six months and close to the initiation of the meeting, I was forced to call Thaler in Washington and pressure him to release at least a verbal decision on the application. The entire organization of the meeting had been suspended, evidently because of lack of knowledge whether or not the organizers (I was NOT one of them) would have some minimal funds to support the travel expenses of a few, highly selected mathematicians in the field. After some pressure, Thaler finally acknowledged that, not only the application had been rejected, but the rejection had been decided sometime before, EXACTLY AS I HAD SUSPECTED FROM MY PRECEDING EXPERIENCE OF NSF OPERATIONS IN SIMILAR CASES. I therefore expressed my complaints to Infante, in his capacity of NSF Division Director and officer ultimately responsible for the case (pp. III–908–908). Infante reacted in a way that can only stimulate smiles. He first acknowledged my complaints with a letter in direct disagreement with the statement by Thaler (p. III–910), in which he claims that "At this time, the review and evaluation process of this proposal has not been completed." A few dozen hours later, Infante communicated the rejection of the proposal via a second letter (p. III–911).

In the consideration of the affair, the fellow taxpayer should keep in mind ABOVE ALL the fact NOT STATED IN THE PROPOSAL that the "Lie–admissible generalization of Lie's theory" means the generalization of the mathematical structure of Einstein's theories. As stressed in Section 1.8, once this mathematical generalization is achieved in sufficient diversification, the generalization of the physical part is only a matter of time, as well known to any NSF referee and officer sufficiently qualified for these functions. There is no doubt that vested, academic–financial–ethnic interests on Einstein's theories have benefited by the rejection of the proposal. The issue left open for the fellow taxpayer is to ascertain who is the loser. The applicants, being senior, tenured, renowned mathematicians, cannot possibly be the losers. The answer can then only be one: the U.S.A. is the loser.

Predictably, the episode implied visible consequences. In fact, following this rejection, all IBR workshops and conferences were moved to Europe. It was indeed foolish to dream that other IBR meetings could have a better chance of being funded by U.S. Governmental Agencies.

NSF REJECTION NO. 9 dated April 14, 1983 (p. III–921) of an IBR application by two senior, U.S. mathematicians entitled "Mathematical studies on reductive Lie–admissible algebras and H–spaces with applications to the geometry of nonpotential dynamical systems". The application was processed by a number of officers of the NSF Division of Mathematics, beginning with Harvey Keynes, Program Director of Modern Analysis. The final review and approval of the consideration process was conducted by E. F. Infante as Division Director.

An inspection of the referees' reports and of the individualized comments provided by the IBR for the NSF, is quite instructive, particularly to reach a mature understanding of the true, ultimate criteria according to which NSF operates and disperses public funds.

Again, the qualifications of the applicants were impeccable (one of them is the co–author of a book in Lie algebra which is rather famous in mathematical and physical circles). Again, the fundamental mathematical relevance of the research program was simply out of the question.* The requested budget was not a problem for anybody with a minimum of knowledge of the procedures used by Governmental Agencies in funding research proposals (the only meaningful budget is that the Agency is willing to pay, while that requested by the applicants has only a vague meaning for a theoretical proposal). The NSF Division of Mathematics was fully stocked with taxpayers' money to support valuable mathematical research, and the availability of funds was not a problem.

BUT THEN, WHY WAS THIS PROPOSAL REJECTED TOO BY THE U.S. NATIONAL SCIENCE FOUNDATION?

On the surface, and judging from the referees' reports, the proposal was rejected on PHYSICAL AND NOT ON MATHEMATICAL GROUNDS, with the motivation that (see referee's report "C", p. III–931) *". . .general classes of nonpotential interactions of the type to which the proposed formalism nontrivially applies are not clearly relevant, if indeed they exist at all. The*

*Virtually all spaces of mathematical and physical relevance (such as the Euclidean or the Minkowski space) are reductive within the context of the conventional mathematical formulation of Lie's theory (that expressed via the trivial unit element and the simplest conceivable Lie product; see Section 1.8). The proposal under consideration recommended the generalization of reductive spaces via the use of the Lie–admissible generalization of Lie's theory. The mathematical implications are truly far reaching (e.g., the turning of a nonlinear structure into an isotopic linear form). The physical implications are simply outstanding (e.g., the technique permits the representation of the transition from the exterior to the interior problem of gravitation for realistic interior trajectories, those of non–perpetual–motion–type; or the representation of the variation of the speed of light in the transitin from one medium to another, which is representable exactly via different Minkowski–isotopic spaces, that is, via different generalizations of reductive spaces).

principal interactions of physics are constrained by symmetry and/or causality considerations, and it is not shown that the proposed formalism has anything useful to offer in connection with them." A number of comments are here in order. First, everybody knows that macroscopic systems are potential only in special circumstances (such as planetray motion), while they are generally nonpotential in the physical reality. Different views would imply the existence of the perpetual motion in our environment (Section 1.3). Similarly, everybody knows that a proton cannot orbit in the core of our sun with a conserved angular momentum. The interior problem of gravitation is therefore intrinsically nonpotential (Section 1.5). Also, everybody knows that open, nonconservative particle reactions have nonunitary time evolutions. ALL these systems and countless more are outside the technical capabilities of potential dynamics. The review of these points was studiously avoided in the proposal, first of all because of their physical nature (the proposal being of pure mathematical character) and, secondly, because offensive to the reader (any NSF referee, to possess sufficient qualifications for this post, is expected to know that the perpetual motion does not exist in our environment). Nevertheless, these points and numerous others were presented, reviewed and repetitiously itemized to the reviewers and, in particular, to Infante, via letters, comments on referees' reports, IBR memos and papers, etc. (for instance, the IBR comments on referee's report "C" — see pp. III–928–930– reviewed the "direct universality" of the Lie–admissible approach for the representation of nonunitary time evolutions, as outlined in pp. 94–96 of this book).

As a result of these and other aspects, it is evident that the referee's report under consideration was inappropriate (rejection of a fundamental mathematical application on physical grounds) and, if indeed appropriate, totally deprived of any credibility.

I must therefore encourage the fellow taxpayer to **see the motivations of the rejection outside the lines of the referees' reports, that is, in the unspoken parts of the proposal and of the review process. In fact, every qualified physicist and mathematician knows well that NONPOTENTIAL (NONLAGRANGIAN–NONHAMILTONIAN) DYNAMICAL SYSTEMS ARE IRRECONCILABLY INCOMPATIBLE WITH EINSTEINIAN THEORIES. This is the point in which the proposal was silent. This is the point that none of the referees had the courage to raise explicitly.** The fellow taxpayer must then reach his/her own appraisal of the TRUE, ULTIMATE reasons why NSF rejected this beautiful proposal by two outstanding, senior, U.S. scholars. To reach a deeper judgment, the fellow taxpayer must know that the generalized mathematical tools submitted in the proposal do indeed constitute a generalization of the mathematical structure of Einsteinian theories. **The ultimate issue is not, therefore, that of a mere rejection, but rather whether or not the case constitutes a**

documented illustration of an intentional, organized effort TO PREVENT THE ACHIEVEMENT OF THE GENERALIZATION OF THE MATHEMATICAL STRUCTURE OF EINSTEIN'S THEORIES, or at least to prevent its achievement under the NSF backing. In different terms, the ultimate issue is whether or not we are facing a conspiratorial obscurantism on Einstein's theories by vested academic–financial–ethnic interests in a U.S. Governmental Agency. After all, we are treating here only the last link of a chain of similar indications in academic institutions, Governmental laboratories and journals of the American Physical Society.

NSF REJECTION NO. 10 dated April 21, 1983 (p. III–950), of an IBR application by three senior, U.S., mathematicians entitled "Studies on Lie–admissible algebras". At this point, the fellow taxpayer will see a repetitive pattern. The proposal was processed by Judith S. Sunley, NSF Program Director for Algebras and Number Theory. Sunley's processing was reviewed and approved by Infante, again, as Division Director. The qualifications of the applicants are simply out of the question (each of them is the holder of a full professorship in mathematics at a large U.S. university with graduate school). The mathematical relevance of the proposal was equally out of the question for the reasons indicated earlier. The NSF mathematical division was stocked with taxpayer's money to support valuable research. Etc.

BUT THEN WHY WAS THIS ADDITIONAL MATHEMATICAL PROPOSAL ALSO REJECTED BY THE NSF?

Again, the reading of the referees' report solicited, inspected, and approved by NSF officers is instructive (pp. III–951–953). Again, the fellow taxpayer WILL NOT necessarily find in these reports the true reasons for the rejection. After all, even though not stated in the application, the proposal dealt with the mathematical generalization of Lie algebras, that is, of a central part of contemporary mathematics and physics. Again, the TRUE, ULTIMATE, reasons must be searched in the unspoken parts. The end result cannot but be the same as before: **a reinforcement of the doubts on the existence of a conceivable conspiratorial obscurantism at a U.S. Governmental Agency on Einstein's ideas in an apparent full alignment with corresponding vested interests in leading academic institutions, Governmental laboratories, and APS journals.**

After all, the fellow taxpayer should not forget the extremes attempted by senior Harvard faculty to prevent my studies on the conceivable invalidation of Einstein's theory in the interior of hadrons under Governmental support (Section 2.1), or the rather incredible lack of interests at National laboratories on the tests of Einsteinian theories DESPITE THE FACT THAT ALL AVAILABLE DIRECT ELEBORATIONS OF EXPERIMENTS SHOW VIOLATION (Section 2.3); or the incredible

stubborness by APS journals to prevent the publication of papers in the field (Section 2.4).

The proposal under consideration here had one peculiarity that is worth reporting to the fellow taxpayer. In late January, 1983, I received a rather unusual letter by Judith S. Sunley (p. III–940). She announced having contacted DIRECTLY AND WITHOUT ANY PRIOR NOTICE the highest administrative officers of each primary affiliation of the applicants, asking for their formal authorization of the IBR administration of a possible NSF contract, as well as a number of additional administrative commitments, all this PRIOR TO THE ACTUAL, FORMAL, APPROVAL OF THE APPLICATION. Each administrative officer contacted by Sunley immediately provided all the needed authorizations (see pp. III–941–947, where all names of individuals and of institutions have been evidently deleted). AND THEN, SOON AFTER THAT THE PROPOSAL WAS REJECTED!!!

A host of unanswered questions are raised by such unorthodox behaviour (U.S. Governmental officers are notoriously cautious on matters of this type). I have my personal theory and I intend to pass it to the fellow taxpayer for whatever its value. Judging from phone calls and other elements, I believe that the proposal had been INFORMALLY ACCEPTED at the time when Sunley contacted the primary administrative officers of the three large U.S. colleges (plus the IBR). At that time, the information was still restricted within a limited circle of the NSF Division of Mathematics. As soon as the informal decision of support propagated to other branches of NSF, such as the Division of Physics (see below for what happened there), pressures by representatives of the apparent, organized, scientific obscurantism on Einstein's ideas initiated their action for the intent of suppressing the funding of the proposal. Success under impunity was assured by the current structure and organization of the U.S. science.

Admittedly, this is my undocumented, personal, theory of the affair. Nevertheless, one thing is certain: a rather drastic change occurred soon after Sunley implemented her unorthodox initiative, and that change was in the negative. The forces of the spider's web that lead to such a change are unknown to me.

NSF REJECTION NO. 11 dated June 8, 1983 (p. III–967), of an IBR application by a senior physicist as principal investigator, entitled "Theoretical, experimental and applied studies on a possible pulsating structure of the Coulomb force of individual electrons". The proposal was processed by David Berley of the Elementary Particle Program, for the experimental profile, and (AGAIN!) Boris Kaiser for the theoretical part (p. III–962). Such a dual processing was reviewed and approved by Rolf M. Sinclair, Acting Director of the NSF Division of Physics.

The proposal was rejected with manifest, vulgarly offensive language, in the referees' reports, such as that of the third reviewer (p. III–970) stating that

"Under no circumstances should precious resources be wasted on such TRASH [emphasis mine]".

I hope the fellow taxpayer sees the reasons why I had no alternative then launching a worldwide denounciation of the current ethical status of the U.S. physics. If this book is not sufficient to promote the deep changes that are needed for the improvement of scientific ethics and accountability, all conceivable initiatives permitted by law will be undertaken, beginning with the promotion of suitable class actions against the U.S. National Foundation to prevent further damages to the dignity of the Country.

The seemingly corrupt character of the NSF referee here considered is clear. On technical grounds, the research project (not reviewed previously in this book) referred to a conceivable pulsating structure of the electrostatic force among two elementary charges, such as the electrons, although the hypothesis could evidently be referred to other elementary charges, such as the quark constituents. Now, suppose that the referee can prove the erroneous nature of the hypothesis for two electrons.* But then, the same referee has absolutely no reliable information to reach any conclusion for the case of quark constituents, whether in favor or against the hypothesis. The corrupt character of the referee, that is, his/her studious adulteration of scientific facts for nonscientific motivations, simply cannot be ruled out.

NSF REJECTION NO. 12 dated June 8, 1983 (p. III–985), of an IBR proposal by a senior physicist entitled "Studies on nonpotential scattering theory". The processing of the application was done by S. Peter Rosen, NSF Program Associate of the Theoretical Physics Program. The review and approval of Rosen's processing was done by Rolf M. Sinclair, Acting Director of the Division of Physics.

The reading of the referees' reports accepted and released by Sinclair (pp. III–986–990) is quite instructive. For example, the first referee begins with the claim (p. III–986): *"I have no confidence in the soundness of . . .the institution with which he*

*This is already debatable. In fact, the consistency of the hypothesis for two ordinary electrons has been proved in the literature beyond a reasonable doubt for the case of nonrelativistic dynamics. The consistency or inconsistency of the hypothesis for the relativistic case as well as for the additional quantum electrodynamical case had not been studied at the time of the submission of the proposal, as clearly stated in the proposal itself (which recommended exactly that study among others). The point is that, traditionally, all hypotheses which are consistent at the nonrelativistic level have been proved sooner or later to admit a consistent relativistic extension. Also, the electromagnetic coupling constant is so large, and the effects of the hypothesis are comparatively so small, to render the hypothesis quite natural. After all, its physical basis is the old idea that electrons are oscillations of the geometry of space. If this is true, the current theories of the electrons' field are irreconcilably insufficient to represent nature (although I do not call them "trash").

[the principal investigator] is associated." Lack of confidence in an institution evidently means lack of confidence in its members. This referee therefore claimed lack of confidence in the 39 members of the Institute for Basic Research, scattered throughout the (western) world, WITHOUT KNOWING THEIR NAMES!!! In fact, their names have not been disclosed by the IBR, because such a disclosure is discretionary to each member (Appendix B). This referee therefore had no information on IBR members, except those of the principal investigator and of the administrative officers. How can Governmental officers have confidence in the credibility of this referee? It is evident that this person pursues schemes of academic politics, rather than science. Yet, U.S. Governmental officers DID consider the report as valid, and they DID use it in the decision making process regarding the dispersal of public funds. The report also claims that the Lie–admissible differentiation used is nonexistent. The Lie–admissible approach is a mere mathematical re–formulation of known NON-UNITARY time evolutions of OPEN systems according to the elementary rules reviewed on p. 95 (of this book). If the seemingly "technical" argument of this referee were correct, nonunitary time evolutions would be prevented to exist, and we would have the perpetual motion everywhere in the universe!

The remaining reviews are plus or minus, of the same caliber of the first. I shall therefore avoid boring the fellow taxpayer with the repetitious illustration of their lack of scientific content.

Quite likely, NSF officers selected as referee representatives of the circles of vested, academic–financial–ethnic interests controlling the U.S. physics. The suffocation of non–aligned research under these premises was then a mere consequence.

The comments made in pp. 169–170 however persist. **The conventional (potential) scattering theory has huge financial implications inasmuch as it is used for the data elaboration of most of current experiments in nuclear and particle physics. If strong interactions do indeed have a nonpotential component (Section 1.6), these data elaborations are incorrect,as established in ref. [113]. The proposal under consideration suggested the development of the nonpotential generalization of the potential scattering theory as a NECESSARY PREREQUISITE for the future resolution of the issue. The existence of huge problems of scientific accountability at the U.S. National Science Foundation is then consequential.**

In fact, the study submitted in the proposal MUST be conducted. The only debatable issue is the institution where the research has to be conducted. Now, I would have accepted with grace the NSF backing of the claim of lack of soundness of the IBR, PROVIDED THAT NSF WOULD HAVE FUNDED THE PROJECT AT SOME OTHER INSTITUTION. The reality is that, to this writing (July 10, 1984), NSF has not funded the

research elsewhere (evidence to the contrary would be welcome). The existence at NSF of huge problems of scientific accountability is then unavoidable. **Large public funds (estimated in the range of hundreds of millions of dollars per year) continued to be spent to this day on data elaboration of strongly interacting experiments, in total ignorance of the critical literature PUBLISHED IN REFEREED JOURNALS (such as Nuovo Cimento, Hadronic Journal,* and others).**

NSF REJECTION NO. 13 dated December 16, 1983 (p. III—999), of an IBR proposal entitled "Studies of quantization of systems with gauge symmetries". The proposal was processed by Su—Shing Chen, Program Director for Geometric Analysis. The processing was reviewed and approved by E. F. Infante, as Division Director of Mathematical Sciences.

This rejection represented the climax of all NSF rejections of the IBR applications. In fact, it was perpetrated AGAINST the referees' reports. As the fellow taxpayer is encouraged to verify (pp. III—1000—1004), all referees praised substantially the principal investigator (a senior, foreign, applied mathematician), and his outstanding record of achievements (including a prestigious monograph on the topic of the proposal). The proposal was therefore rated by the referees as "Excellent", "Very Good", etc.

This last rejection did indeed have visible consequences. When combined with some fifteen years of experiences with NSF all of the same nature, it confirmed to me the apparent existence at NSF of an organized mandate to prevent our group of scholars to conduct research under NSF backing.☆ I therefore withdrew the last two IBR applications pending at DOE and, a few days following the reception of Infante's last rejection, I initiated the writing of IL GRANDE GRIDO.

> **Lack of consideration by the NSF of a comprehensive experimental—theoretical—mathematical proposal to test the validity or invalidity of Einstein's ideas under strong interactions.**

Understandably, I did not intend to terminate in a graceful

*Another corrupt statement that repeatedly appeared in NSF reviews is that the Hadronic Journal is not a refereed journal. The erroneous nature of the statement is well known to the authors who have published or attempted to publish a paper in that journal. The corrupt character of the statement is evident, because based on the venturing of a judgment with full awareness of the lack of any solid information on the subject.

☆ Note that money was not a factor in most of the applications, inasmuch as a few thousand dollars would have been sufficient. The ultimate, objective seems to be that of preventing the appearance of papers dealing with the possible invalidation of Einstein's ideas, under the official backing of the U.S. National Science Foundation.

way my contacts with the current NSF officers. I therefore studiously left at NSF a sort of "time bomb". In fact, I collected into a single document all the experimental, theoretical and mathematical proposals rejected by NSF with a coordinating preface and the new title "EXPERIMENTAL, THEORETICAL, AND MATHEMATICAL STUDIES ON A POSSIBLE GENERALIZATION OF EINSTEIN'S SPECIAL RELATIVITY FOR EXTENDED, DEFORMABLE, STRONGLY INTERACTING PARTICLES" (pp. III–1122–1131).* The insidious aspect is that I did not submit to NSF this huge document as a proposal. Instead, I submitted it to E. F. Infante as an "advance consultation" via a detailed letter of presentation mailed in copy to some 31 senior scholars who had been involved in the research in one form or another (their names have been evidently delected in the Documentation).

As I had predicted, Infante passed the hot ball from his desk to the NSF Division of Physics, where the material truly belonged and, in particular, to Marcel Bardon. Exactly as predicted, Marcel Bardon ignored this document in violation of NSF's statutory obligations. To this day (July 15, 1984), no communication has ever been received from NSF on this advance consultation since the notice of reception and referral by E. F. Infante dated May 20, 1983 (p. III–1127).

Lack of interest by Edward Knapp, NSF Director General.

It is the duty of every member of a free society to inform the highest possible officers of any, even minimal, doubt of ethically questionable practices involving public funds. I therefore informed Edward Knapp, NSF Director General, of each and every aspect reviewed in this section (and more), via copies of all various letters, documents, complaints, comments on referee reports, IBR presentations, papers, memos, etc. This process was done with the same repetitious intent I had studiously implemented for Derek Bok, President of Harvard University (Section 2.1), or for Leon M. Lederman, Director of FERMILAB (Section 2.3), or for David Lazarus, Editor in Chief of the American Physical Society. Again, this pattern was intended to prevent Knapp's statement: "I did not know!"

These (unilateral) contacts concluded with a summary letter (p. III–867), which reviewed: (a) **the primary scientific objectives of the studies (resolutions of the validity or invalidity of Einstein's theories under strong interactions), and the NSF responsibilities on the topic; (b) the rejection of technical proposals by qualified senior scholars via approved referees' reports with vulgarly offensive language; (c) the rejection of proposals**

*The fellow taxpayer should remember here the plausibility of the deformation of hadrons under sufficiently intense collisions, with consequential breaking of the rotational symmetry and invalidation of Einstein's special relativity (see Chapter 1, or Figure 2.2.1, p. 209, for a brief outline.

at times against the totality of the recommendations of the referees; (d) the rejection of proposals while NSF did not fund at other institutions similar projects specifically referred to the possible invalidation of Einstein's theories; (e) the causing of unnecessary damage to the applicants by NSF officers, beyong the mere rejection of the proposals; (f) the NSF repetitious pattern in delaying the communication of rejections of funding for nonaligned meetings, for the apparent intent of damaging their organization; (g) the case of the rejection of the primary, IBR, group proposal whereby one of the referees had contacted directly one of the advisors of the project, L. C. Biedenharn of Duke University, had apparently succeeded in pressuring him to withdraw from the project, and had even secured copy of an (apparent) letter by Biedenharn to this effect; etc.

This final report to Knapp concluded with the following passage: *"As indicated to you in preceding correspondence, I am considering a National campaign aimed at having the American Physical Society formulate and adopt a much overdue CODE OF ETHICS, as well as having the judical and political systems create independent means for its strict enforcement. This letter is intended to give you and your officers all the necessary prior knowledge of the possibility that the totality of the documentation regarding our research grant applications, jointly with individualized comments, of course, might be released to the appropriate Committees of the U.S. Senate and House of Representatives, as well as to the press. In case you and/or your officers have any objection to such a release, you should let me know immediately. However, in case no objection exists (or can be raised), no acknowledgement of this letter is needed."* To make sure of the propagation of the information, I mailed a copy of this final letter to R. M. Sinclair at the NSF physics division, and to E. F. Infante at the mathematics division.

No reply was ever received from Knapp, not even a gesture of courtesy!

Whether Knapp ever did anything following my reports, or he ignored them altogether, is of no relevance here. **The important point is the lack of any investigation of the cases organized by Knapp IN A WAY AS PUBLIC AS POSSIBLE AND AS VISIBLE AS POSSIBLE OUTSIDE THE NSF.** The point is evident for anybody with a minimum of knowledge of the operations of Governmental Agencies. In fact, the lack of a public investigation fully visible to the outside, is a de facto backing of the action by the NSF officers. This is nothing else than, again, a repetition of what happened at Harvard University, at National laboratories and at journals of the American Physical Society.

These considerations have a crucial constructive role. It is of the essence for the fellow taxpayers to understand that **such extremes of disinterests at the highest administrative levels of the U.S. physics community, are routinely conducted because of the current, absolute, total, and guaranteed impunity.** In turn, this

is essential to understand the potential effectiveness of the constructive suggestions submitted in the next chapter for the improvement of the conditions of the physics community.

Epilogue.

I have expressed my personal views that

■ Officers of the U.S. National Science Foundation are servants to leading physicists at leading academic institutions.

■ The condition of servility leads to the impossibility by NSF officers to reject questionable reports by leading physicists and to accept them no matter what their content is. This, in turn, implies the inevitable use of corrupt referees' reports* in the Governmental process of dispersing public funds.

■ The use of manifestly questionable reports and/or practices in the decision–making process has created a huge problem of scientific ethics at the National Science Foundation which has been growing constantly during recent years, by multiplying the concern in numerous segments of the physics and mathematics communities in the U.S.A. and abroad.

■ The National Science Foundation has accumulated throughout the years a monumental problem of lack of scientific accountability in the dispersal of public funds on Einstein's special and general relativities, by avoiding the funding of research on the apparent invalidation of Einstein's theories in the physical reality. The preceding outline and the related documentation establish beyond any reasonable doubt the existence at NSF of a mandate to prevent the funding of IBR research proposals in mathematics and physics. Nevertheless, this was not sufficient reason for writing this international denounciation. The staggering problems of scientific accountability at NSF have been created by the joint LACK of funding the needed research on the invalidation of Einstein's relativities at some other institution.

■ The seemingly deep interconnection between NSF officers and leading physicists at leading academic institutions, Governmental laboratories and journals of the American Physical Society, has provided sufficient elements to suspect the existence of a conspiratorial obscurantism in the U.S. physics for the intent

*The fellow taxpayer should keep in mind that my documentation is only a minute fraction of that available by other NSF applicants scattered throughout the world. Also, I should report that the terms "crackpot", "trash", "no achievement worth mentioning" and the like have been formulated with respect to my person and my work only within the rings of greed surrounding NSF. Outside those rings, my work has been appraised beyond my best expectations, with terms such as "Truly epoch–making" [Journal of Applied Mathematics], "outstanding" [Applied Mechanics Review], and numerous, similar reviews in several languages, printed in journals scattered throughout the world [as obtainable from the publishers of my monographs in theoretical physics].

of suppressing, discrediting or otherwise jeopardizing qualified research on the insufficiencies, invalidation and possible experimental disproofs of Einstein's theories, in the sole benefit of vested, academic—financial—ethnic interests in the U.S.A., and in basic disrespect of the societal need for advancements in basic knowledge.

But, again, my personal opinion is insignificant. Equally insignificant is the personal opinion of Edward Knapp, NSF Director General, Marcel Bardon, Boris Kayser, Rolf M. Sinclair, S. Peter Rosen and other officers of the NSF Division of Physics, as well as E. F. Infante, Judith S. Sunley, Alvin Thaler, and other officers of the NSF Division of Mathematics. The only important opinion is that by the fellow taxpayer who supports the research funded by NSF as well as the salaries of the above quoted NSF officers.

In the consideration of the case, I beg the fellow taxpayer to initiate appropriate action aimed at a genuine improvement of the pursuit of novel physical and mathematical knowledge via public funds, as well as preventing additional, manifest, damages to the dignity of the Country via senseless refereeing practices. It all boils down to a basic, unreassuring, point: **a Country vitally dependent on the advancement of basic knowledge, such as the U.S.A., which penalizes rather than supports,critial examinations of basic issues, such as the validity of invalidity of Einstein's theory under strong interactions, could be doomed within a sufficient time scale, even though amidst the glitter of temporary technological advances.**

2.5.2: DIVISION OF HIGH ENERGY PHYSICS OF THE DEPARTMENT OF ENERGY.

The original determination by the Department of Energy to support our research.

Under the directorship of William A. Wallenmeyer, and with Bernard Hildebrand as chief of the Physics Research Branch, the Division of High Energy Physics of the Department of Energy (DOE) proved, beyond any doubt, its original determination to support the research reported in Chapter 1. In fact, DOE did indeed succeed in providing support to our group while I was at Harvard during the period 1977—1980, despite the vigorous internal opposition there reported in Section 2.1. Subsequently, during the years 1980—1983, when it resulted impossible to continue the research under Harvard's administration, DOE accepted the administration of a nonacademic corporation even though the research was of purely academic character.

The invaluable function of the DOE support.

It is a truism to say that all the scientific results reported in this volume regarding the insufficiencies, generalizations and experimental resolutions of Einstein's theories, are due to the above DOE support. Despite its limited character,* the support permitted the initiation and conduction of numerous scientific initiatives. This resulted into a significant volume of scientific production by the various members supported by the contract (which includes the publication of: six research monographs, nine volumes of proceedings of conferences and workshops, and a total number of over 150 papers).

The litany of subsequent DOE rejections of IBR applications.

In mid 1981, the relationship with the DOE changed rather substantially, and we began to experience a chain of rejections of IBR applications, which later on became a mere litany. We first experienced the rejection of a rather unique mathematical application signed by five senior, renowned, U.S. mathematicians (pp. III–832–901). The repetitious rejections included all primary group proposals submitted by the IBR to the DOE (pp. III–804–846), and numerous other applications that had been also rejected by NSF.

The possible link of the DOE rejections with the founding of the IBR.

On my part, back in 1980, I could not possibly continue the coordination of a growing, international, group of mathematicians, theoreticians and experimentalists while working in an office at home. On the other part, David C. Peaslee, then at the DOE, had told me the minimal chances for DOE continuing to support my academic research under a nonacademic administration. Also, the possibility of my continuing research on the limitations and possible generalizations of Einstein's theories in a U.S. physics department had to be virtually excluded, as seen in pages 220–222. This left no other choice than to organize a new, independent, research institution, the IBR (Appendix B). Apparently, the change of attitude at DOE initiated precisely with the founding of the IBR. The apparent alignment with vested interests in the Cantabrigian academic community is evident and needs no comment here.

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*To have an idea of the limited amount of funds, the average DOE support to our group during the years 1980–1983 was of the order of $ 60,000.00, including all administrative overheads and indirect costs, the holding of a yearly conference or workshop, publication charges, travel support, etc.

I still remember vividly when, after a long struggle, we finally succeeded in purchasing the Prescott House within the compound of Harvard University to provide permanent housing for the IBR.* I called David Peaslee at the DOE in Washington from the Cambridge Registry of Deeds the very moment following the registration of purchase, to thank DOE for past support and to invite him to be our guest at the inauguration ceremony of the new Institute. Peaslee declined the invitation, although I sensed a touch of sadness in his voice. He had been the DOE officer in charge of our contract since its initiation at Harvard back in 1977. He knew everything, including the financial and human sacrifices which had permitted the founding of the IBR without any Governmental contribution. I had the impression that, in declining our invitation, Peaslee was performing his duty against his personal wishes. At any rate, he left the DOE soon thereafter.

My gratitude toward Wallenmeyer and Hildebrand of the DOE.

Whatever the reasons for the DOE rejection of so many and so qualified applications on so manifestly fundamental topics, I want to be on record to respect these decisions. In fact, I have nothing but respect, admiration and, most of all, gratitude toward Wallenmeyer and Hildebrand. After all, I owe them everything I have accomplished. It is just that simple. If new situations have forced them to terminate the support, I cannot but accept it with grace.☆

It was regrettable that not even a minute amount of funds could be provided to support the IBR research reviewed in this book. In fact, even a very small support of, say, a few thousand dollars per year, would have at least permitted the continuation in the U.S.A. of our yearly research meetings. Instead, the suppression of funds had to be total, thus forcing the IBR into alternative forms of financing, of which this book is an expression.

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*To have an idea of the difficulty of the purchase, one should keep in mind that Harvard University has an understandable interest in the purchase of buildings within its compound. The Prescott House had, therefore, to be literally purchased under Harvard's nose, as it was indeed the case. An additional difficulty was created by the fact that Cambridge is under Rent Control with its notorious limit on possible income, and consequential restriction of bank appraisals of the value of certain buildings well below their actual value. As a result of these and other circumstances, the purchase of a considerable piece of Real Estate had to be achieved without any bank mortgage.

☆I should stress the difference with NSF here. My intentionally ungraceful reaction to NSF considerations of our applications is due to the NSF acceptance of vulgarly offensive language in the referees' report, and other aspects which never transpared in the DOE considerations.

2.5.3: DIVISION OF NUCLEAR PHYSICS OF THE DEPARTMENT OF ENERGY.

The climax of IL GRANDE GRIDO.

Among all the various, scientifically evil episodes presented in this book, that which I consider to be, by far, the individual, most distressing episode was perpetrated by Enloe T. Ritter, Director of the Nuclear Physics Division of the Department of Energy. The episode regards the rejection of an IBR proposal submitted to Ritter in June, 1982, under the title (pp. III–1064–1121)

EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION

(see pp. 145–150 of this book for a description).

The proposal essentially suggested the repetition of the neutron interferometric experiments done by H. Rauch, Director of the Atominstitut of Wien, Austria, since 1975. It was motivated by the fact that the latest measures show the VIOLATION of the rotational symmetry (see Section 1.7). The proposal dealt with the most fundamental possible experiment a particle and nuclear physicist could conceive these days, as stressed throughout this volume. In fact, the confirmation of the experimental measures on the breaking of the rotational symmetry for extended (and therefore deformable) particles under intense, short range, interactions, would imply the need for suitable generalizations of Einstein's special and general relativities.

The first difficulties in 1981 at the Institute Laue–Langevin (ILL), of Grenoble, France.

The experimental team had conducted the tests of the rotational symmetry at the nuclear reactor of the ILL laboratory since their initiation in 1975. As recalled in Section 1.7, the first experiments were done on neutron beams without short range interactions, and they resulted to be in full agreement with the predictions of the exact rotational symmetry, as expected. No academic difficulty of any relevance occurred during this initial period, to my knowledge.

In 1978, the experimenters repeated the measures, also at the ILL reactor, but this time with the (involutary) inclusion of short range interactions. Initial measures released in 1978 [99] resulted to be still compatible with orthodox doctrines. Nevertheless, a new re–elaboration of the experiment done in 1981

because of improved values of constants and other factors, began to show a violation of the rotational symmetry subsequently announced in ref.s [100, 139] (see Figure 2.2.1, p. 209, for a conceptual review). In turn, the initiation of the detection of violation of orthodox doctrines signaled the initiation of academic difficulties experienced by the experimenters.

In early 1981, a group of mathematicians and theoreticians (including myself) launched the organization of the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE–ADMISSIBLE TREATMENT, to be held at the University of Orléans, France, in early January, 1982, under the formal support of the French Government (via local Institutions), as well as a small participation of the DOE (via my grant). The Proceedings of the meetings were published in ref.s [126].

Predictably, H. Rauch was the key, invited, experimental speaker. Rauch and his team therefore applied to the Institute Laue–Langevin in 1981 for the re–run of the measures. The running time was planned not later than November–December, 1981, in such a way to be able to report at the Orléans International Conference of early 1982, at least some preliminary results of the new measures.

To the "astonishment" of the experimenters (p. III–1020), the Institute Laue–Langevin declined authorization for the re–run of the experiment at that time (p. III–1018). The decision had been taken by a committee (apparently)* headed by Otto Shult of the Institut für Kernphysik der Kernforschungslage in Jülich, West Germany. A rather intense scientific crisis then followed which included telegrams, certified mail, and the like (pp. III–1019–1048). The crisis was encouraged by unverifiable rumors such as:

- The rumor that the difficulties in France had originated at leading physics institutions in the U.S.A. Whether this is true or false, it is quite plausible that the information leading to the ILL rejection (to re–run the measures in time for the Orléans International Conference) originated outside the Institute Laue–Langevin. In fact, the proposal had been submitted in the traditional dry style used by experimenters with its notorious paucity of information; or,
- The rumor that irate French scholars had filed detailed reports of the entire affair to high levels of the French and West German Governments (the apparent chairman of the committee, Otto Shult, being from West Germany). Whether this is true or false, it seems sure that

* The decision was communicated by a secretary without any indication of the names of the members of the responsible committee. It took some pressure on T. Springer, Director of the Institute, to finally obtain some information on the names of the committee.

the negative decision at the ILL had not been unanimous.

One thing is certain: the measures were not permitted in 1981, and this most crucial experimental information was missed at the Orléans International Conference of early 1982 with predictable scientific damage. The same measures are missing to this day. In fact, we only have re—elaborations [100, 139] of the 1978 measures [99], as stressed in Section 1.7.

Whether in Cambridge, U.S.A., or in Grenoble, France, the gains by vested, academic—financial—ethnic interests resulting from preventing the re—run of measures [99], have been indicated throughout this volume, and they need no further elaboration here.

The opposition at the Massachusetts Institute of Technology and at the National Science Foundation against the re—run of the experiment.

The Austrian—French experimental team did not need U.S. money to repeat the measures, even though any financial support would have been evidently welcome and valuable. The primary reasons for the experimenters' interest in a possible DOE support was the officiality of such a backing, including the hope that it would contain the political difficulties experienced in the re—running of the experiment at ILL.

With this spirit, the IBR provided full support to the Austrian—French experimental team, to file the above indicated proposal. The understanding was that money was not a factor, that is, the "U.S.A." could be part of the "Austria—France—U.S.A. Collaboration" even with a minimal amount of money at the borderline with decency for an experiment (say, a few thousand dollars).

The proposal was first subjected to one year of delay because of the lack of cooperation by a co—investigator who had joined in the meantime the Massachusetts Institute of Technology (see the report of the affair on pages 222—226 of this book). In turn, this left little doubt as for the apparent opposition at MIT against the re—run of measures [99].

Additional delay was caused by the Physics Division of the National Science Foundation. In fact, after resolving the MIT impasse, the proposal was submitted to NSF. Rather than initiating the consideration process, Rolf M. Sinclair, the NSF program director in charge of the case, commented to our submission with the rather unbelievable view (p. III—1055): *"The proposal is excessively brief in experimental details and fails to describe what would be done and by whom, and would probably be impossible to have reviewed."*

I personally did not believe one word of this statement, as indicated to Sinclair in a detailed letter of comments (pp. III—

1056–1060). The proposal quoted ALL the preceding experimental papers in the field (whose detailed knowledge MUST be assumed by anybody to qualify for NSF reviews). In particular, the proposal identified in all the necessary technical details the improvements intended for the new runs. In this particular instance, there simply was no room for academic dances: the measures had been conducted several times since 1975 and, therefore, THE EXPERIMENT WAS NOT NEW AT ALL. It had simply to be re–done with the indicated higher accuracy which would have confirmed or disproved the latest values showing VIOLATION. Owing to the absolutely fundamental character of the problem, delays in the scientific process because of irrelevant or imaginary details could likely imply the existence of unspoken, non–scientific objectives. In the essence, this was the reason of the crisis at the ILL, and this was the reason of my irreconcilable disagreement with Sinclair at NSF.

At any rate, the items requested by Sinclair simply could not be provided at the time of the submission in a way better than that presented in the proposal.* Thus, I could only interpret Sinclair's position as expressing a negative attitude at NSF against the re–run of the experiment apparently because of its evident damage to vested interests in the U.S. academia caused by the possible, consequential invalidation of Einstein's theories. The NSF proposal was therefore withdrawn by the IBR to avoid a total waste of time and money (p. III–1061).

The thirteen months of consideration of the proposal by Ritter at DOE.

With all this rather incredible (but documented) background, the proposal was finally "accepted for consideration" by Ritter in June, 1982 (p. III–1101). The proposal had remained exactly the same as that submitted to NSF. Nevertheless, to avoid possible criticisms, the proposal was complemented by a rather voluminous amount of scientific and administrative information (see pages III–1068 and ff.). For instance, the minimal need of funds was stressed and reiterated numerous times, in writing and verbally. In particular, the IBR made it clear that possible U.S. funds would have priority in the hiring of U.S. experimentalists to be trained by the Austrian–French team in the experimental measures, for their possible subsequent repeti-

*For instance, in regard to personnel, the project contemplated the use of the original team, as well as new U.S. experimentalists. The point is that their hiring could possibly be considered only AFTER the formal approval of the proposal with a budget call specifically intended for the hiring. At the time of the submission, only generic information could be provided, and certainly no name of specific U.S. experimentalists could be voiced prior to a formal announcement of the openings, and the screening of the applicants in conformity with the rule of Affirmative Action Employment and other administrative requirements.

tion in the States. After all, it was very easy to predict that, for a relevant experiment such as this one, the measures have to be done, re—done, and then done again before claiming any final scientific conclusion.

The statutory six months of consideration had passed without any decision at DOE. Then, on November 12, 1982, Ritter asked for authorization to retain the proposal under consideration for another six months (p. III—1112). The IBR gladly accepted the request with an additional, detailed report on various aspects, including the formal authorization that the Institute Laue—Langevin had provided in the meantime for the re—run of the tests (p. III—1048).

On July 25, 1983, after thirteen months of consideration, Ritter communicated his rejection of the proposal with a few dry lines, by therefore reaching a decision manifestly aligned with the negative attitudes previously experienced at MIT and at NSF.

Predictably, the arrival of Ritter's letter of rejection in mid July, 1983, marked my formal decision to write this book.

Epilogue.

As indicated earlier, I believe that Ritter's rejection of the U.S. participation in the experiment to test the validity or invalidity of the rotational symmetry, is the individual, scientifically most evil act I have ever experienced in my academic life for the following reasons (among others):

- ★ **The needed funds were or otherwise must be absolutely insignificant for the budget of the Nuclear Physics Division of the U.S. Department of Energy. In fact, only a few thousand dollars would have been sufficient (at one point, I was tempted to donate myself this small sum to DOE, so that, in turn, DOE could support the U.S. participation in the project). Financial considerations must therefore be excluded from any meaningful or otherwise credible reason for the rejection.**
- ★ **The towering value of the proposal as compared to ALL other proposals under consideration by Ritter at that time, and the high qualifications of the experimenters, were simply out of the question. It is a truism to say that the virtual entirety of particle physics is in suspended animation because of the lack of resolution of the issue (including relevant military profiles touched earlier in this book). Also, after having done and re—done the experiment since 1975, the experimental team is universally recognized as THE most qualified in the field on a worldwide basis. Thus, insufficient scientific values and/or insufficient qualifications of the applicants must be also excluded by any meaningful or otherwise credible motivation underlying the rejection.**

★ The gains by vested academic—financial—ethnic interests in the suppression of the U.S. participation in the tests and, possibly, in the suppression of the tests altogether, are self—evident. In fact, lacking an experimental resolution of the validity or invalidity of the rotational symmetry, corrupt academic barons at leading U.S. institutions can continue to pocket large public funds via contracts (estimated in the range of hundreds of millions of dollars per year; see Section 1.9) which are centrally dependent on the exact validity of the rotationally symmetry, without any consideration whatsoever of its possible violation.

As a result of all this, I believe that Enloe T. Ritter, Director of the Nuclear Physics Division at the Department of Energy, has acquired a staggering PERSONAL problem of scientific accountability vis—a—vis the fellow taxpayer. As I wrote him in a letter of January 15, 1983, mailed in copy to D. P. Hodel, Secretary, and S. Brewer, Assistant Secretary of DOE (p. III—1119):

". . .no ethically sound scholar can silently accept the scientific, economic and military implications caused by the indefinite deferral of the tests. The rotational symmetry is at the foundation of the contemporary physical knowledge. The suppression of its direct verification which has been successfully achieved until now by vested, organized, academic—financial—ethnic interests, has all the ingredients of a scientific crime against this beautiful Land, against our children who have to live in it, and against the pursuit of novel human knowledge."

As in other cases, my personal opinion is insignificant. The sole important judgment as to whether or not Enloe T. Ritter has indeed committed a "scientific crime", is that by the fellow taxpayer. In turn, the sole judgment which can possibly be even more important, is that by posterity. In fact, posterity can and will unquestionably appraise, one day, whether or not we are currently experiencing in the U.S.A. an organized conspiratorial obscurantism on Einstein's theories and its foundations, beginning most importantly with the rotational symmetry.

CHAPTER 3

CONTAINING THE PROBLEM OF SCIENTIFIC ETHICS IN U. S. PHYSICS

I now pass to the constructive role of my experience: its value for the identification of means to contain the problem of scientific ethics in the U.S. physics community.

The attitude which appears recommendable to all members of the community, including physicists, administrators, governmental employees and officers of professional associations, is that of mutual forgiveness of past wrongdoings, and a commitment to join forces to build a better future.

Since the American Physical Society (APS) has not adopted a *CODE OF ETHICS* until now, all judgments regarding issues of scientific ethics in physics have a strictly personal character, and should not be expected to be necessarily shared by others [this evidently includes all judgments passed or considered in this book]. As a result of this situation, the only value of past experiences, including mine, is that of possible assistance in the building of a better future.

This is the spirit for which IL GRANDE GRIDO was written and this is the spirit here submitted to all members of the physics community.

The insufficiencies of the proposed recommendations.

In the following, I shall submit a number of recommendations inspired by my personal experience, as well as by the experiences of other colleagues I know. In essence, I asked myself the question: what are the improvements in the organizational structure of the U.S. physics community which would have rendered this report unnecessary?

To prevent excessive expectations, I would like to stress from the outset that this constructive part of IL GRANDE GRIDO is insufficient in content, diversification and presentation. To achieve sufficient maturity, each recommendation should be investigated by a team of experts and would require other resources which I simply do not have. I only hope that the

recommendations originating from my personal experience as a physicist will be of some value for the appropriate legislative, governmental and societal bodies.

A rudimentary definition of "scientific wrongdoing".

For the sake of the following presentation, I shall assume the preliminary definition of "scientific wrongdoing" as "any act which is committed or omitted by one or more individuals and/or institutions with the awareness that it is harmful to society because detrimental to scientific knowledge".

An important aspect the fellow taxpayer should keep in mind, is that scientific wrongdoings, in general, DO NOT constitute "crimes" according to the current code of law. In fact, they do not refer to stealing of money and other conventionally unlawful acts (which are not addressed in this book). This book therefore addresses the paradoxical situation in which given acts by individuals and/or institutions are fully legal; yet they may be, by far, more damaging to society than ordinary crimes.

3.1: RECOMMENDATIONS TO THE U. S. CONGRESS.

RECOMMENDATION # 1: LEGISLATE A *BOARD OF SCIENTIFIC REVIEW* (BSR) FOR THE CONSIDERATION OF CLAIMS OF SCIENTIFIC WRONGDOING IN PHYSICS, MATHEMATICS, BIOPHYSICS AND OTHER BASIC SCIENCES.

The tragedy of individual scientists who believe to have been the victims of scientific wrongdoings, is that there is no "court" where to file their complaints. As indicated earlier, scientific wrongdoings are generally permitted by the current code of laws. The filing of scientific claims in ordinary courts is therefore, generally ineffective, if not inappropriate. The filing of complaints to the appropriate committees of institutional, professional or Governmental organizations is equally ineffective for a variety of reasons including: the lack of guaranteed consideration of the claim; the lack of organizational guidelines for the proper appraisal of the wrongdoing; the general secrecy of the consideration; etc.*

*As a specific example, when I became convinced that the editorial handling by Physical Review Letters of theoretical and experimental studies on the violation of the time–reflection symmetry for open nuclear reactions (reported on pp. 160–168 and 256–271 of this book; and pp. II–531–660 of the Doc.) provide vast scientific, economic and military damages to America, I contacted the chairman of the Publication Committee of the American Physical Society, P. W. Anderson of Princeton University. During a phone conversation, Anderson stressed the fact that his committee could consider only cases of papers that had received a "final rejection" by an APS journal. This organizational structure of the committee implied the

In addition, a reason for the current decay of scientific ethics is the guaranteed complete impunity for any act, decision or omission whatsoever, provided that it is permitted by the current code of laws. This un—reassuring situation is evidently due to the current lack of a "scientific court". The recommendation here submitted, most respectfully, to the U.S. Congress is precisely that of legislating this essential, currently missing, scientific institution.

MAIN ORGANIZATIONAL LINES SUGGESTED FOR THE BSR:

AFFILIATION:	To the Office of the Attorney General in Washington, D.C.*
COMPOSITION:	Five members appointed by Congress, from any suitable layer of society (not necessarily of scientific background)☆ including the Attorney General, the tenure of each member being limited to a maximum non—renewable period of four years, with the possible exception of the Attorney General.
CHAIRPERSON:	The Attorney General or a person designated by the same.
ADVISORS:	The BSR should appoint Advisory Committees from within the (National and international) scientific community

virtual impossibility of even filing a complaint, let alone receive a fair consideration. In fact, as elaborated in Section 2.4, APS journals do not generally provide "final rejections" (or even "ordinary rejections" for that matter), because the editors merely mail, re—mail, and then mail again to authors the negative referees' reports on undesired papers without any indication as to when the rejection becomes "final". After ascertaining the organizational insufficiencies of the APS Publication Committee, I searched for other committees, both within and outside the APS, without any result. In fact, I was unable to identify one single committee, and/or appropriate body, whether in Government or in the Courts of Law, which was sufficiently staffed to even understand my claim, let alone act on it.

*The Attorney General of the United States of America is the chief law officer of the Federal Government, whose primary duty is that of protecting public interest. As such, the Office of the Attorney General is particularly suited to house the Board of Scientific Review.

☆The fellow taxpayer should remember the reason of scientific dispute with the Massachusetts Institute of Technology (Section 2.2): the possibility that the charge distributions characterizing protons and neutrons are not rigid, but experience deformations as a result of external forces. This possibility was readily understood by my neighbors (who belong to walks of life other than science). However, the same possibility was not readily admitted by MIT physicists apparently because of its political implications, such as the breaking of the rotational symmetry and the violation of Einstein's special relativity. Because of this sadly known academic politics, scientists ARE NOT recommendable as executive members of the BSR.

and act on specific cases following non—binding advice by the appropriate Committee.

FUNCTION: The BSR should have legislated authority to consider any claim of scientific wrongdoing, whether filed by individuals or warranting consideration in the opinion of the Attorney General and others. The BSR should furthermore have legislated authority to impose suitable punishment, compensation and remedy to any individual and/or institution found guilty of erroneous conduit, such as the termination of an existing Federal research contract, or the prevention of Federal contracts for a given period of time. Finally, the entirety of the proceedings of the BSR should be published and made available to the public (with the evident exception of cases of National security).

Needless to say, a considerable amount of research by a team of differentiated expertise is needed to bring the proposal to maturity, particularly in the organizational and operational details. A few aspects, however, should be firm. First, to be effective, the Board should be legislated OUTSIDE professional organizations, such as the APS as a NECESSARY CONDITION FOR CREDIBILITY. Second, The BSR should take into consideration *CODES OF ETHICS* if and when adopted by individual scientific organizations. Nevertheless, the BSR decisional guidelines should not be restricted to comply necessarily with said Codes. Third, the so—called "leading academic institutions" should be permitted to have their representatives on the Advisory Committees, but the control of any Committee by representatives of said institutions would imply the lack of credibility of the Board's action. In fact, the leading academic institutions are expected to be the primary reasons of concern of the Board. At any rate, qualified advisors can be readily found in "lesser leading", that is, "lesser politically entangled" institutions throughout the U.S.A. and abroad.

RECOMMENDATION # 2: MANDATE THE ROTATION OF EMPLOYEES AT GOVERNMENTAL AGENCIES PROVIDING FEDERAL RESEARCH SUPPORT

One of the strengths of the U.S. Constitution is the wisdom to limit the period of time one individual can serve as President. One of the current weaknesses of Governmental Agencies is the unlimited permanency of their employees. This has resulted in a life—long tenure by specific individuals in the dis-

persal of public funds in specific sectors of research. The unreassuring nature of this situation is evident, because of the inevitable, voluntary or involuntary associations of said Governmental employees with outside circles of interests. For instance, Marcel Bardon, Boris Kayser, Rolf Sinclair and others have been running the Division of Physics of the National Science Foundation as far back as my memory can go, since I landed here as an immigrant in the late sixties.

The damage to science of such life—long tenures in the dispersal of public funds in research contracts may be staggering. One of its visible forms is the ABSTENTION by a growing number of individuals to apply for research support. Until this occurrence was made up of isolated cases, it was of no concern. But the occurrence is now widespread throughout all sectors of research, with an evident damage to the Country.

The only way to break the sadly known circles of "insiders" and "outsiders" in Federal research contracts is to mandate the rotation of governmental employees in charge of the consideration process. This can be only accomplished by a Congressional legislation on the limitation of the duration of permanency by governmental employees in each given Agency division. This can be done in a way compatible with current laws on civil service, e.g., by shifting the personnel to different divisions.

The ineffectiveness of the current means to cope with the problem is well known. Typically, the burden of attempting a rejuvenation of the personnel at Governmental Agencies is passed from one given Administration to the Director of the Agency. This burden generally results in creating a barrier (rather than an atmosphere of cooperation) between the Director and the personnel. The end result I have observed repeatedly is the permanency of the employees, and the rapid termination instead of the directorship itself.* In fact, the change of Directorships at the various Governmental Agencies (e.g., the NSF) is a rather frequent event in Washington, D.C. and, per se, an un—reassuring fact. The point is that Directors do not process research grant applications. Only individual officers do that. The frequent change of Agency Directors, therefore, has no impact on the problem.

RECOMMENDATION # 3: MANDATE IN THE YEARLY BUDGET OF EACH GOVERNMENTAL AGENCY THE TOTAL AMOUNT OF FUNDS TO BE DISPERSED TO SCIENTISTS AS INDIVIDUALS AND THE MAXIMAL AMOUNT OF EACH GRANT PER EACH SECTOR OF RESEARCH.

A further deficiency of the current organization of the U.S. science is the general impossibility for scientists to apply for federal research support AS INDIVIDUALS, without any unnec-

*See the case of E. Knapp, former NSF Director, as reported a number of times in Science in 1983.

essary academic and/or corporate conduits. I am referring to the numerically largest percentage of support, that for theoretical research conducted by one individual and possibly one or more associates. These grants essentially provide support for salary, travel and publication charges, by therefore requiring no special administrative skill. The contracts can therefore be handled by the Principal Investigator under the Agency guidelines without any need of wasting public sums in unnecessary administrative conduits, whether academic or corporate.

It should be indicated here that my practical inability to apply for research support as an individual has been a primary reason for the appearance of this book. As well known, the NSF statute does indeed permit scientists to apply for support as individuals. However, as equally well known, the cases of actual NSF grants to individuals are extremely rare. At any rate, the very submission of a proposal to NSF without a "qualified" administrative backing by an "established" academic or corporate entity, is generally considered as disqualifying. The content of the application and the qualifications of the applicant are notoriously of secondary relevance.

These are essentially the reasons why the direct support to individuals is an insignificant element of the current scientific organization in the U.S. These are also the reasons why Congress should mandate the total yearly amount of funds to be dispersed to individuals. In fact, lacking a mandatory quota, we remain at the current *status quo,* where the item "grants to individuals" is essentially a curiosity line in the budget of Governmental Agencies.

The need for Congress to mandate a ceiling on the maximal possible amount of each individual grant is equally evident. In fact, it is needed to avoid disequalities occurring when physicists belonging to "leading" institutions receives sums disproportionately higher than those granted to physicists belonging to lesser prestigious affiliations or no affiliation at all.

In numerical terms, I would like to recommend the mandatory dispersal into contracts to individuals of a minimum of 50% of the annual budget in theoretical physics, with a ceiling of $ 50,000 per each individual contract for FY 1985 (with different numerical percentages for other sectors, such as experimental physics)[☆] As a more specific numerical example, the NSF budget for theoretical physics for FY 85 contemplates the dis-

[☆]The percentage of grants to individuals for experimental physics should be evidently lower than that for theoretical physics, because of the usefulnes in this case of academic administration, e.g., for the realization of complex equipments. Yet, a number of grants to experimentalists do not warrant any academic administration, e.g., when modest equipment is required. For this reason, Congress should mandate the percentage of grants to individuals also for the experimental sector (30% of the total experimental budget is recommended here), as well as to all other segments of basic research.

persal of $ 13.9 M (excluding gravitation)*. The proposal here submitted would mandate the dispersal into contracts to individuals in FY 85 of a total of $ 6.9 M. With an average grant of $ 25,000 per individual, the proposal would permit the support of 276 theoretical physicists in FY 85. The residual $ 6.9 M would be dispersed as currently budgeted (for research contracts under academic and/or corporate administration). Assuming a minimum of 50% overheads,☆ and the same average of $ 25,000 per individual, the remaining $ 6.9 M would support 138 additional theoretical physicists for a grand total of 414 supported individuals.

The improvements of support are evident. In fact, by assuming that the entire amount of $ 13.8 M is dispersed as currently budgeted (with an irrilevant percentage to individuals),‡ by assuming again (for mere illustrative purposes) that the administrative conduits pocket 50%, and that the average individual support is $ 25 K, we would reach a total number of 276 supported individuals, versus 414 for the above proposal. Recommendation # 3 therefore implies a 150% increase in the number of supported scientists WITHOUT INCREASING THE BUDGET ONE SINGLE PENNY.

The predictable opposition by vested academic interests.

It is evident that academic interests will oppose the proposal because it implies their loss in FY 85 of at least $ 1.8 M in overheads for the NSF theoretical physics budget alone. The pertinent issue for the U.S. Congress is not what pleases or displeases academic administrators, but rather what serves or disserves National interests. When public funds are allocated for research in theoretical physics, they should not be used as a form of charitable contribution to academia. In fact, the administrative function provided by academia is not necessary for the contracts considered.

Finally, the most negative point discouraging academic support, unless of proved necessity, is the amount of academic politics each individual scholar has to overcome for the mere purpose of reaching all the necessary approvals to apply. These political difficulties are generally interpreted at Governmental Agencies as a guarantee that the proposal has passed the review by the local "peers". In the reality of the academic world, however, this implies that, often, the original proposal had to be

*See Physics Today, April, 1984, p. 58.

☆This estimate may result to be conservative, for academic institutions have pocketed overheads of up to 75% of a given total grant, thus leaving only the residual 25% to the Principal Investigator for direct use in the project.

‡An instructive reading is, for instance, the yearly book *"National Science Foundation Grants and Awards"* available from the U.S. Government Printing Office.

adulterated in such a way to comply with the vested interests of the local peers. The advantage of eliminating altogether the academic or corporate administration, whenever unnecessary, is therefore evident.

The more balanced conditions of basic research existing in several Foreign Countries.

A further point which should be brought to the attention of the U.S. Congress is that the funding of basic research at a number of foreign Countries appears to be considerably more balanced than that currently in effect in the U.S.A. on numerous counts. The study of these foreign organizations is therefore recommended.

As a specific example, the Canadian physics community is known to be smoother than its counterpart in the U.S.A. One of the reasons is the wisdom of the Canadian Government to LIMIT THE TOTAL AMOUNT OF INDIVIDUAL SUPPORT, thus permitting the support of a proportionately higher percentage of physicists, with the evident decrease of internal tension. By contrast, the current emphasis in the U.S. is in the so–called "excellence", that is, in the maximization of competition in the hope of stimulating quality. The end result is a proportionately smaller number of supported physicists, as compared to Canada, with the consequential, inevitable, multiplication of internal tensions of which this book is a direct manifestation.

The illusory nature of the current emphasis on "excellence".

To appraise whether or not the current organizational structure of funding basic research does stimulate "excellence" or not, we must recognize openly the following facts.

1) Qualified proposals for Federal research support exceed available budgets at all Governmental Agencies.
2) The selection, among all qualified proposals, of which one should be funded and which one rejected is generally made on the basis of NONSCIENTIFIC elements, such as the academic affiliation of the applicant, the aligned or non–aligned character of the contents and/or of the authors with vested academic–financial–ethnic interests in the field, and other factors not even remotely connected to the technical contents of the application.
3) Few "leading" institutions pocket, by far, the greatest majority of Federal research funds.

Under these premises, the current emphasis on "excellence" is a mere mask for the uninformed. The emphasis evidently serves

well the interests of the few "leading" institutions and, according to some observers, the emphasis has been conceived precisely for that purpose. Nevertheless, the idea that the current organizational structure in the funding of basic research truly stimulates "excellence" has today lost all grounds of credibility.

The constructive function of *IL GRANDE GRIDO.*

Once this first point is acknowledged, the understanding of the loss for America is a mere consequence for anybody with a minimal knowledge of the way these "leading" colleges operate. It is at this point where the disclosure of my experience becomes useful. In fact, one can see that, within these leading institutions, the chances of filing an application on a research topic non–aligned with vested interests there, are absolutely null, no matter how relevant the application is.

Consider, for instance, Harvard University. As recalled in Section 2.1, only full professors there qualify as principal investigators of federal research grants. This means that, if a junior member at Harvard has an idea which is brilliant, but contrary to the vested interests of his/her direct, senior, supervisor, that junior faculty has no realistic possibility whatsoever of applying to a Governmental Agency for support.* The only hope for that junior faculty to be a truly free scientist within a truly democratic scientific society, is for the U.S. Congress to pass suitable legislation (the chances that Harvard modifies its statute should be dismissed because unrealistic, with similar situations occurring at the other "leading" colleges currently pocketing the majority of research funds). For that, it is sufficient that ANY member of Harvard faculty, whether junior or senior, has the dual option of, either applying under Harvard's administration (whenever ADMINISTRATIVELY NECESSARY) or as an individual. In turn, this is practically meaningful if and only if Congress mandates the minimum total amount of funds to be dispersed on research contracts to individuals per each Agency, jointly with the maximal individual amount (Recommendation # 3). In addition, Congress should pass legislation intended to break possible rings of alliances within the academic–governmental complex (Recommendation # 2), as well as provide effective means for individual scientists to voice their complaints (Recommendation # 1).

Lacking suitable Congressional legislation, the future scenario of the U.S. science is readily predictable. Governmental Agencies will continue to serve the vested interests of "leading" institutions, with an evident loss of scientific resources outside said institutions. Second, the "leading" institutions will continue

*The submission, say, to the NSF Division of Physics of an application as an individual would be immediately disqualified under these premises evidently because the application had been internally rejected at Harvard.

to permit only grants under their administration, even when such administration is basically unnecessary and un—warranted, with evident waste of public money. Third, only these applications compatible with vested academic—financial—ethnic interests in control of each given sector of a "leading" institution, will be permitted to be filed for federal research support, with an evident loss of scientific resources, internally, within said institutions.

The damages to science are multifold.

The moment of truth.

If we are truly sincere in the intent to serve the future of America, rather than that of minoritarian groups, it is time to

- # **recognize the current totalitarian character of the scientific organization in the U.S.A.;**
- # **admit the fact that the current governmental funding of research favors and actually encourages such totalitarian conditions; and,**
- # **legislate all the necessary improvement conceived to break such an academic—governmental complex, as a condition to guarantee true freedom of scientific inquiry.**

3.2: RECOMMENDATIONS TO THE AMERICAN PHYSICAL SOCIETY.

RECOMMENDATION # 4: FORMULATE AND ADOPT A *CODE OF ETHICS* IN PHYSICS.

By inspecting the latest (December, 1980) Professional Ethics Project report of the American Association for the Advancement of Science (AAAS) authored by R. Chalk, M. S. Frankel and S. B. Chafer, one can see that VIRTUALLY ALL U.S. SCIENTIFIC ORGANIZATIONS, INCLUDING THE POTATO ASSOCIATION OF AMERICA (p. 134), SUBSCRIBE TO A *CODE OF ETHICS,* EXCEPT THE AMERICAN PHYSICAL SOCIETY (and a few others). This is an evident, most unreassuring situation. In fact, the physics community at large, including the academic, corporate and military sectors, has been using billions of dollars of taxpayers money for decades without any *CODE OF ETHICS* (as well as any genuinely effective control by the political or the judicial systems). A situation of this type is simply untenable. Further delays in the formulation and adoption of a *CODE OF ETHICS* can only substantiate the suspicion that the lack of the Code is the result of a specific intent by opposing, high ranking, vested interests within the society.

RECOMMENDATION # 5: THE AMERICAN PHYSICAL SOCIETY COUNCIL SHOULD ESTABLISH A STANDING COMMITTEE ON THE *CODE OF ETHICS.*

The APS has a number of standing committees on various matters (publications, international freedom of scientists, education, applications of physics, etc.), but NOT on ethics. This situation is also un–reassuring and must be corrected.

Article VI–5 of the current APS Constitution states: *"The Council may establish such other committee as it may deem desirable in the management of the activities of the Society. The Council shall appoint, or delegate to the President the appointment of, the Chairperson and members of each such committee".*

Recommendation # 5 is therefore submitted to the APS Council for the establishing of a Standing Committee on the *CODE OF ETHICS* with the following duties:

a) to assist the APS membership at large in the formulation of the *CODE OF ETHICS;*
b) to have the *CODE OF ETHICS,* so formulated, formally adopted by the Society with related revision of the Constitution and By–Laws; and,
c) to continue thereafter the standing function of overseeing possible future updatings, modifications and improvements of the *CODE OF ETHICS;*

as well as any additional function considered recommendable by the Council.

I DO NOT recommend that the committee should review claims of scientific wrongdoings. In fact, such review, to be genuinely effective, should be done by a Federal body OUTSIDE the Society (Recommendation # 1). This is the reason for the suggested name "Committee on the Code of Ethics" rather than "Committee on Ethics".

RECOMMENDATION # 6: THE AMERICAN PHYSICAL SOCIETY PUBLICATIONS COMMITTEE SHOULD REVISE CURRENT REGULATIONS PERTAINING TO THE REFEREEING OF PAPERS IN APS JOURNALS.

One of the most visible and insidious problems of current editorial practices at APS is the life–long tenure as referees by leading physicists at leading institutions. This guaranteed status has implied the practice that everything goes, as far as the contents of the referee's report is concerned. It is evident that a serious improvement of the refereeing process (that is, one beyond a powdery mask for inepts) must imply the termination of refereeing status at APS by dishonest referees, NO MATTER HOW HIGH THEIR STANDING IS AT THE SOCIETY, whenever caught in scientifically unethical or inappropriate practices. Other weaker forms, even though superficially more democratic, may hide schemes intended to preserve the impunity of corrupt refereeing, or serve vested, academic–financial–ethnic inter-

ests.

More specifically, the recommendations I submit for considerations are the following.

SUGGESTED REVISIONS PERTAININGTO REFEREES:

6–1) Referees' reports should comply with the *CODE OF ETHICS* (as soon as adopted by the society);

6–2) Referees' reports should not contain offensive comments or non–scientific comments on the technical contents of the paper submitted;

6–3) Referees' reports should be constructive in their criticisms, that is, in case of rejection, they should itemize the improvements recommended in all the details needed for their actuation by the authors, and down to the individual passage, formula and/or word, whenever appropriate;

6–4) Referees should accept the review of papers if and only if they are not reviewing, at the same time, research grant proposals by any of the authors;

6–5) Referees should accept the review of a paper if and only if they have a documented record of expertise in the specific topic of the paper (and not in the field at large).

Referees who violate any of the above rules should be terminated or suspended in their function by the society for a period of time commensurate to the violation. As a specific example, consider the report claiming that one of the opposing experiments [103, 104] on time–reversal symmetry is wrong and the other is right without any third, independent repetition of the SAME experiment (pp. 261–262 of this book). That referee committed a manifest violation of scientific ethics and its refereeing function should have been terminated by the society, irrespective of its academic, ethnic and other affiliation. The termination and/or suspension of the refereeing function, particularly if made public, would be a major deterrent of scientific wrongdoings in refereeing.

SUGGESTED REVISIONS PERTAINING TO EDITORS:

6–6) Editors should inspect each referee report for compliance with conditions 6–1/6–5 above. In case of any major default, upon consultation with the Editor in Chief, the editor should have authority to terminate or suspend the referees in their function for the appropriate duration of time. The referees' reports found in major default of conditions 6–1/6–5 above should then be ignored in the consideration process, and new reports solicited.*

*In case of lack of adoption of the revision here proposed by the APS, the editors are recommended to implement revision # 6–6 on their own and have a documentation of it. After all, the editors have the power to select

6–7) In case of mere insufficiencies of the reports with respect to conditions 6–1/6–6 above, the editor should mail the reports to the referees (AND NOT TO THE AUTHORS) for all the necessary improvements to comply with said conditions (PRIOR TO THE RELEASE OF THE REPORTS TO THE AUTHORS).

The effectiveness of a *CODE OF ETHICS* at a given society is as deep as the encouragement for its compliance which is provided by the society itself. A well known deficiency of the current editorial practices at APS journals, is the powerless condition of individual authors for whatever scientific wrongdoings and/or abuses they experience during the submission of their papers.

This deficiency must be resolved as a necessary condition to dissipate the current dark shadows of totalitarian conditions of the U.S. physics community. It is evident that authors must be empowered with, and actually encouraged to use, much more effective means of filing their complaints, particularly when exposed to manifestly corrupt referees and unresponsive editors.

SUGGESTED REVISIONS PERTAINING TO AUTHORS:

6–8) The APS should support authors in their possible claims at the BSR and/or other appropriate bodies outside the society.

6–9) The organization of the Publication Committee should be revised to permit authors to file their complaints DURING the consideration process.

An illustrative example: the current conspiratorial obscurantism on irreversibility.

An illustration is useful here to appraise the constructive potential of the recommendations submitted so far. The fellow taxpayer should recall the case of the experimental paper [103] by the Québec–Berkeley–Bonn group on the apparent violation of the time–reflection symmetry for open nuclear reactions (possible origin of the irreversibility of our macroscopic world). As recalled on pp. 160–168, the paper had been submitted to Phys. Rev. Letters (a letter journal for rapid publications) where it was kept for over one and one–half years, for the apparent intent of permitting an experimental group at Los Alamos National Laboratories to rush disproving measures [104] and have them quoted in paper [103]. Vested, academic–financial–ethnic interests controlling the sector in the U.S.A. immediately claimed measures [103] wrong and their rebuffal [104] correct, prior to

or avoid any given referee. The documentation of the practice is here recommended in the editors' own interests, in the event the case is considered by the Board of Scientific Review for possible editorial misconduits.

the availability of any third, independent, experimental resolution of the issue. The world wide acceptance of the U.S. orthodox position routinely followed, thus resulting in the apparent conspiratorial obscurantism in this fundamental aspect of human knowledge.*

Assume now that the recommendations submitted until now were implemented and in effect back in 1981. What would be the scientific scene today? I can readily tell you, fellow taxpayer, that the scientific scene today would have been substantially better.

The mere POSSIBILITY that the Québec–Berkeley–Bonn experimental group (or any other person) could have filed a complaint to a Federal board of scientific inquiry (Recommendation # 1) would have forced the editors of Phys. Rev. Letters to the proper editorial processing of the case, that is, RAPID PUBLICATION of paper [103], followed by a subsequent, equally rapid,

*See Sections 1.4, 1.6 and 1.7 for a review of the technical aspects. Certain aspects are crucial for the understanding of the conspiratorial nature of the obscurantism, such as the fact, well known to all physicists, that center–of–mass trajectories of closed–isolated systems are indeed, in general, time–reflection invariant (this is the case of our Earth, to begin with). To see the irreversibility, one must enter within the structure of a system and study open reactions. Corrupt academicians support their claim via papers on the time–reflection invariance of the center–of–mass treatment of closed–isolated systems, in full awareness that this information has no bearing whatsoever on the problem of irreversibility in the interior structure of the system, that is, for each open–nonconservative constituent. Other scientific wrongdoings occur on the technical means to truly claim existence of lack of existence of irreversibility (analyzing power of the forward reaction as compared to the polarization of the backward reaction). Corrupt academicians base their claim of exact time–reflection invariance in nuclear physics via experimental data on the so–called cross–sections, in full awareness of the fact that these means imply averaging processes that eliminate the effect, as stressed, elaborated and repeated again in the literature. As a result of all these (and much more) facts, the only possible scientific conclusion at this time is that the problem is basically unresolved. In the transition from nuclear to hadron physics (the structure of protons, neutrons, and other strongly interacting particles), we abandon Science even more and enter into the realm of personal beliefs without any possibility of experimental resolutions in sight for the foreseeable future. In fact, the current lack of irreversibility within the interior of a proton or a neutron is today imposed via shear academic power based on a pletora of assumptions, none of which is established via direct experiments (such as that quarks exist; that Pauli's principle is exact; that Einstein's special relativity holds; etc.). The lack of resolution of the problem of irreversibility within this finer layer of nature is more unresolved than ever. Yet, academic barons suppress its unresolved character in violation of the most elementary rules of scientific ethics and accountability. The fellow taxpayer should be aware of the consequences of a passive acceptance of this situation, including those for the security of the United States of America. If the time–reflection invariance is truly violated in the INTERIOR of protons and neutrons, potentially new weapons may be conceived by foreign countries.

publication of rebuffal [104] whenever scientifically mature. Second, the very existence of a Federal board of scientific inquiry would have forced the Los Alamos experimentalists to repeat ALL the measures originally conducted in paper [103] PRIOR to venturing any claim, rather than conducting only a small portion of them, as permitted by the APS editors. Third, the very existence of said Federal board would have forced vested interests in the U.S. academia to acknowledge the only possible scientific truth: WE DO NOT KNOW AT THIS TIME WHICH OF MEASURES [103, 104] IS CORRECT AND WHICH IS WRONG, UNTIL ALL MEASURES [103] ARE REPEATED BY THIRD INDEPENDENT PARTIES A SUFFICIENT NUMBER OF TIMES. In turn, the confirmation of the open character of the problem would have, on one side, prevented the rest of the scientific world to follow the position of the U.S. orthodoxy, and, on the other side, would have stimulated new studies. Rather than the current conspiratorial obscurantism, we would have had a beautiful intellectual democracy in which ALL possibilities are duly explored and appraised prior to the final settling of the issue. The remaining recommendations would have assisted in the achievement of the same goals (such as the adoption by the APS of a *CODE OF ETHICS*), or permitted complementary improvements, such as the funding by governmental agencies of proposals on BOTH the preservation AND the violation of the symmetry, by preventing the current monopolistic restriction of federal funds only to research projects based on the conjecture of the exact validity of the time–reflection symmetry in the particle world.

In short, the existence back in 1981 of appropriate means to contain the problem of scientific ethics, would have permitted a genuinely democratic scientific process, resulting today in basic advances at the foundations of scientific knowledge.

As a final point, the fellow taxpayer should be aware that the problem under consideration is not an esoteric one of no practical relevance. Not at all. The problem is of such fundamental physical relevance that can affect YOU, let alone your children, economically and militarily. In fact, the resolution of the problem of the origin of the irreversibility of our macroscopic world could permit far reaching advances, from particle physics to solid state physics, including new military applications.

All this has been lost because of manifest deficiencies in the current organizational structure of the U.S. science, with particular reference to the lack of effective means to contain excesses of academic greed.

3.3: RECOMMENDATIONS TO DIRECTORS OF FEDERAL AGENCIES.

RECOMMENDATION # 7: ENCOURAGE EMPLOYEES OF FEDERAL AGENCIES GRANTING RESEARCH CONTRACTS TO DISCLOSE THEIR ETHNIC BACKGROUND.

A strength of America is the variety of its different ethnic groups, all coexisting with the same rights under one Flag. A weakness occurs whenever one individual ethnic group is permitted to acquire control of any given sector of the Federal government, for that sector will likely operate in the interest of the ethnic group in control, and to the detriment of the Country. Another weakness occurs whenever one individual ethnic group is excluded from a given sector of the Federal Government over a sufficiently long period of time. Participation to Federal activities by as many ethnic and/or minoritarian groups as possible should therefore be encouraged, but the two extremes should be opposed. I am referring to the opposition in equal measures of one specific ethnic group being prevented from participating in a given public sector, or taking over numerical control of a public sector.

The value of these evident rules of democracy becomes magnified when referring to the dispersal of public funds. If a given division of a given Federal agency is permitted to be controlled by ANY ethnic group, that division will likely disperse the majority of public funds to the ethnic group in control, in disrespect of the need to serve the Country via more equanimous practices.

The ONLY way to prevent, or otherwise identify the problem is that each governmental employee participating in the dispersal of public funds via federal contract should disclose his/her ethnic background. As a specific example, each and every member of the Division of Physics of the National Science Foundation (including the secretarial employees) should disclose his/her ethnic background in order to ascertain whether or not ANY ethnic group has acquired control of the division, or whether or not ANY ethnic group has been excluded over a sufficiently long period of time.

The task of each Federal Agency soliciting and making available to the public a disclosure of ethnicity by its employees, can be best performed by the Agency Director.

My ethnic origin is Italian. I am proud of it and I foresee no conditions and/or circumstances whatsoever that would prevent me from disclosing VOLUNTARILY my ethnic origin. I expect ALL other members of a free society to have the same feelings toward their own ethnic origin.

To state it differently, I recognize the right to the confidentiality of the ethnic background to an individual living in a country oppressed by totalitarian regimes, and other circumstances. However, when that individual lives in a free, democratic society such as the U.S.A., and becomes a Governmental employee dispersing public funds, that individual has the moral obligation to disclose his/her ethnic background. The lack of such voluntary disclosure under the premises indicated, can only imply an evil scheme to me. How about you, fellow taxpayer?

RECOMMENDATION # 8: IMPROVE CURRENT OPERATIONAL RULES FOR THE CONSIDERATION OF GRANT PROPOSALS ALONG LINES SIMILAR TO THOSE RECOMMENDED FOR THE IMPROVEMENT OF THE REFEREEING OF PAPERS AT APS JOURNALS.

I am referring to Revisions # 8–1 through 8–9 pertaining to referees, reviewers and authors essentially along the corresponding Revisions # 6–1 through 6–9.

A number of additional revisions should be implemented, specifically, for the consideration process of research grant proposals, such as:

8–10) FINAL DECISION SHOULD BE REACHED ON GRANT APPLICATIONS ONLY AFTER THE AUTHORS PROVIDE THE AGENCY WITH THEIR COMMENTS ON THE REFEREES' REPORTS.

The current disparity between the processing of papers and that of research grant proposals is evident and well known (but not acted upon). When an editor rejects papers, the authors have the possibility of commenting on the possible erroneous character of the review. Whenever the author's case is sufficiently founded, the editor can then approve the manuscript without any modification.

For the case of grant proposals, the situation is different. In fact, final decisions are made by the Agency without any consultation with the authors regarding the veridicity of the referees' reports. When these reports are grossly erroneous, offensive, or manifestly corrupt, applicants are practically left with the sole possibility of waiting for a sufficiently long period of time, and then submit a new proposal.

The possibility of applying for a reconsideration, even though existing on paper, is excluded here as an effective means of communication between applicants and reviewers. This is so for a number of reasons, such as: the lack of certainty that the reconsideration will be indeed permitted; the general perception of a reconsideration as an admission of wrongdoing in the review process; etc. At any rate, I did succeed in initiating a pro-

cess of reconsideration at the NSF (in regard to the vulgarly offensive referee reports for a research grant application pertaining to the writing of monographs [9,10]; see pp. 276–279 of this book). However, I succeeded only upon reaching the highest Officer of the Country, the Agency Director and other prominent Officers; the reconsideration process demanded the creation of a new post (that of "Special Assistant to the Associate Director for Mathematical and Physical Sciences", see Doc. p. III–792); the officer in charge of the reconsideration soon found himself sandwiched between my relentless accusations of scientific corruption in the NSF refereeing of the proposal, and the predictable support of the referees provided by NSF officers; and similarly unpleasant as well as ineffective situations. Judging from my personal experience, I therefore have no doubt that the current process of reconsideration should be eliminated altogether and substituted with more effective means.

Those recommended here are essentially two. On one side, applicants and reviewers should communicate PRIOR to the Agency reaching any decision. In particular, authors should receive a copy of the referees' reports on their applications and be permitted to express their comments PRIOR to the Agency achieving the final decision. Said comments should then be appraised by the review panel, and be part of the information leading to the final decision. In this way, if a referee makes a statement which is demonstrably wrong, or unfounded, or unethical, the authors have a chance to prove it, and the Agency has a chance of being informed. After all, the scientists who can provide the best, most detailed and elaborated comments on the referees' reports, are the authors themselves.

But . . . . to prevent that the consideration process becomes a farse for uninformed, this is not enough. The organizational structure of science should be complemented with a Federal scientific court, the BSR, where applicants can file claims of misconduits in the reviews of grant applications, with the understanding that said court shall punish reviewers and referees alike found guilty of scientific wrongdoings.

Under these premises, we can expect, on one side, a more cautious attitude by corrupt referees and, on the other side, a more cautious attitude by reviewers with excessive ties to vested, academic–financial–ethnic interests.

#8–11) AGENCY DIRECTORS SHOULD HAVE THE AUTHORITY TO TERMINATE OR SUSPEND THE EMPLOYMENT OF REVIEWERS VIOLATING THE CONFIDENTIALITY OF THE REFEREEING PROCESS EVEN AMONG REFEREES.

This is a key point for the set of recommendations submitted in this book. Whether only suspected or actually done, reviewers do have the power of preventing the funding of specific applications. The mechanics for these actions is known to all

scholars with a sufficiently deep knowledge of operations of Governmental Agencies, and it is surprisingly simple. In fact, it is sufficient for the reviewer to select, as referees, those academicians who have notorious vested interests opposing the topic and/or authorship of the proposal.

However, this is per se insufficient to guarantee the rejection of the application. In fact, if some of the referees are "dissident" (that is, not sufficiently aligned in the rejection), the rejection itself is not sure. In order to achieve the alignment of all the referees toward the rejection, it is essential that at least one of the referees (say, that most politically involved) knows the names of the other referees. Once this is done, the unanimous recommendation of rejection is certain. The actual scientific contents of the proposal is only matter for naive people, in my view.*

It is evident that, to better serve America, this possibility must be prevented (or the practive terminated?). Each referee of a research grant proposal of a U.S. Governmental Agency must keep his/her status absolutely confidential. By complement, Agency reviewers must be prevented from disclosing the names of the referees to any of them. In turn, such prevention is effective if and only if embodied in regulations contemplating the termination or suspension of employment for transgressors. Other weaker forms may satisfy inepts and accomplices, but they would leave current practices basically unchanged.

3.4: RECOMMENDATIONS TO INDIVIDUALS.

Recommendations to individual scholars.

Scientific corruption, like any other form of curruption, feeds on three problems: (1) IMPUNITY, (2) COMPLICITY, and (3) SILENCE. The containment of the problem of impunity has been addressed with Recommendation # 1. The containment of the problem of complicity has been addressed with a number of suggestions, such as Recommendation # 4 (the APS should formulate and adopt a *CODE OF ETHICS)* or Recom-

*The fellow taxpayer should remember the rather incredible alignment of ALL the referees toward the rejection of the primary group proposal submitted by The Institute for Basic Research to the NSF for experimental, theoretical and mathematical studies on the construction of a new mechanics, the hadronic generalization of quantum mechanics. As pointed out on pp. 385–386, the chances for all referees to be so strongly against the funding of the proposal are minute on all statistical grounds. The ethical standards of the review is qualified by the referee (p. III–865) who contacted one of the senior members of the proposal (L. C. Biedenharn, Jr., of Duke University) to ensure his withdrawal from the project. If that particular referee had been informed by NSF officers of the names of the other referees, the alignment of all of them toward the rejection would have been an easy consequence.

mendation # 7 (Federal employees granting research contracts should disclose their ethnic background).

The containment of the problem of silence is evidently a task of individual members of the community, whether researchers or administrators or governmental officers.

My recommendations to individual scholars are essentially those I have practiced.

RECOMMENDATION # 9: INDIVIDUAL SCHOLARS SHOULD BRING SCIENTIFIC WRONGDOINGS TO THE ATTENTION OF THE HIGHEST RESPONSIBLE ADMINISTRATORS, OR OTHERWISE INFORM THE WIDEST POSSIBLE AUDIENCE, AS SOON AS THEY BECOME AWARE OF THEIR OCCURRENCE.

The form of communication will evidently vary from individual to individual, and much depends on the courage by each individual. But the underlying issue is crystal clear:

WHEN EXPOSED TO APPARENT SCIENTIFIC CORRUPTION, SILENCE CAN BE COMPLICITY IN SCIENTIFIC CRIME.

The newsletter SCIENTIFIC ETHICS.

Another known deficiency of the current organization of the U.S. science is the absence of an editorial vehicle for the rapid, unobstructed, publication of reports on questionable scientific ethics by courageous members of the community. This situation is well known to all scholars who have attempted to publish a comment and/or a letter to orthodox vehicles of the community, such as PHYSICS TODAY (the official vehicle of the American Physical Society), or SCIENCE (the official vehicle of the American Association for the Advancement of Science). Other vehicles do exist and are indeed receptive, but they are generally perceived as being outside academia and, as such, do not carry an appreciable weight in the community.

This situation is also un—reassuring. Suppose that a major scientific wrongdoing occurs somewhere and sometime in the U.S.A. Suppose that individual scholars become aware of such a wrongdoing and are willing to bring the case to the attention of the scientific community. The chances for such scholars of succeeding in having his/her claims published in one of the established vehicles are very small.

I have been aware of this situation for years. In fact, I have tried myself unsuccessfully to publish even moderate appeals on ethical problems of refereeing, without any relevant success. For example, a letter on the topic submitted to PHYSICS TODAY was published with such editorial cuts to the point of compromising its understanding, and definitely not

representing its original intent.* Another letter of denounciation (this time on the offensive language used in the reviewing of technical books) was rejected altogether by SCIENCE with the editor's statement that the frequency of the occurrence did not warrant attention!

Because of the insufficiencies reported above, a newsletter is currently being organized under the title of **SCIENTIFIC ETHICS.** The newsletter is specifically intended for the rapid publication of un–adulterated (but refereed) contributions on ethical issues, and appears to be particularly suited for debating any of the issues treated in this book.

Recommendations to individual administrators.

When the individual who becomes aware of possible scientific misconduits is a high ranking administrator, the need for action becomes compelling. My recommendation to individual administrators is simple:

> **RECOMMENDATION # 10:** WHENEVER AWARE OF APPARENT SCIENTIFIC WRONGDOINGS, INDIVIDUAL ADMINISTRATORS SHOULD SOLICITE OR OTHERWISE ORGANIZE PUBLIC INVESTIGATIONS OF THE CASES.

The time when a college president can afford the luxury of absteining from initiating public action on ethical issues involving public interests is, or otherwise must be, over because complicity via silence may have very serious consequences. In fact, the international power of colleges such as Harvard or Yale University, carries such a weight at other colleges throughout the world, that the end result could be a conspiratorial obscurantism.

At any rate, if a conspiracy truly exists in the U.S. physics on Einstein's relativities, the persons that should carry the heaviest responsibilities are precisely the presidents and primary administrators of leading colleges. For all legal and practical purposes, they are the "administrators" of public money obtained via federal research contracts. This implies, in particular, their responsibility to ensure a well balanced use of public funds, thus including the encouragement, let alone permission, of dissident scientific views AT THEIR OWN CAMPUS. In fact, the voicing of dissident views is notoriously suppressed at departmental levels, whenever opposing circles of vested interests are in control. The sole possibility for the existence of such dissident

*The letter was published in Physics Today, April, 1983. Its objective was that of putting in black and white the fact that *"the problem of refereeing does not exist at a remote college in North Dakota. It exists instead at the colleges where the major refereeing load is carried out, that is, at Harvard University, at The Massachusetts Institute of Technology, at Yale University, and the like."* This crucial passage was totally omitted by the editor, jointly with several others.

views, and for the college's fulfillment of scientific accountability, therefore rests where it should be, at the administrative level.

To state it differently, until now, college presidents and leading administrators have implemented the practice of virtually complete lack of interference with departmental research programs. It is now time to reconsider this practice. It is time for leading, or otherwise responsible administrators to appraise departmental research grograms, and undertake all the necessary action to complement such programs, whenever requested for the fulfillment of scientific accountability by the college, vis—a—vis the taxpayer.*

Recommendation to individual taxpayers.

But, above all, the person that should initiate an active role in the conduction of science is that providing the funds: the fellow taxpayer. This is why I have suggested the executive members of the Board of Scientific Review to be selected among ordinary taxpayers, and NOT among scientists (Recommendation # 1). But, even if truly legislated by Congress, the BSR is and remains insufficient. The individual taxpayer, with his/her own initiative, remains the true, ultimate arbiter. The most radical suggestions are therefore submitted in this work to the fellow taxpayer.

RECOMMENDATION # 11: TAXPAYERS ASSOCIATIONS SHOULD FILE CLASS ACTIONS AGAINST ANY INDIVIDUAL AND/OR INSTITUTION SUSPECTED OF SCIENTIFICALLY UNETHICAL CONDUCT.

The most visible illustration for the need of organized action by individual taxpayers is provided by the Program of Gravitation within the Division of Physics of the National Science Foundation. As elaborated in Section 1.5, this Program has, supported for decades, research centrally dependent on Einstein's theory of gravitation, generally without any consideration and/or quotation of the technical literature on its erroneous character.

*Derek Bok, President of Harvard University, has acquired a personal problem of scientific accountability, and has propagated such a problem from Harvard's physics department to the entire university, precisely because of his lack of interference with departmental decisions regarding research programs. In fact, once aware of the virtually absolute impossibility of conducting research at Harvard's physics department on the apparent invalidation of Einstein's relativities, Bok should have initiated PERSONAL action, by soliciting, inviting, or otherwise promoting dissident research at some other branch of the university. Then, and only then Harvard would have avoided the current problems of scientific accountability on Einstein's relativities, as reported in Section 2.1. Much similar situations exist, not only at Harvard University in other segments of science, but also at virtually all leading colleges in the U.S.A.

The grip of greed controlling the sector is so strong, organized and diversified, that only one thing can implement an intellectual democracy at NSF: a class action organized by individual taxpayers against the individual officers of the National Science Foundation and their referees who are responsible for the current dispersal of public funds for research in gravitation.

By "intellectual democracy" I am referring to the well balanced condition in which sufficient funding of research based on Einstein's gravitation is evidently continued, but, jointly, NSF disperses a sizable percentage of the budget to dissident research on the incompatibilities of Einstein's gravitation with the physical reality, and on the needed, more appropriate formulations.

I want to be on record here to indicate that, in my view, the situation is so hopeless, that none of the recommendations submitted in the preceding sections of this chapter will permit the achievement of a true intellectual democracy in gravitation at NSF. Only a class action by individual taxpayers can.

3.5: CONCLUDING REMARKS.

Dear fellow taxpayer, permit me to conclude with a few remarks presented in the same spirit as that of the preceding ones, as sincerely felt in the interest of America, and submitted for whatever their value. The remarks below are inspired by a mixture of precautionary pessimism, which is necessary for objectivity, and contained optimism on the capability of the U.S.A. to improve ethics in science.

The first point of contained pessimism I would like to convey, is that scientific corruption has existed since the birth of science, and will continue to exist until the end of academia. The "elimination" of the problem of scientific corruption is, therefore, practically unrealizable. The only realistic goal is that of "containing" the problem within tolerable boundaries, as addressed in this book.

A second point is that scientific corruption exists at the highest levels of academia. It is important that politicians, administrators and the U.S. Government at large become aware of it. We may evidently disagree on the appropriate "definition" of scientific corruption, as well as on the "dimension" of the problem. But, to avoid shadows of hypocricy or complicity, we must all agree on the "existence" of the problem at the highest, decision making layers of U.S. science.

A further point calling for precautionary pessimism is that the American Physical Society is not expected to be capable, alone, of bringing scientific ethics within contained, acceptable,

boundaries. This is due to the fact that the vested interests that have prevented, until now, the formulation of a *CODE OF ETHICS,* not only are still there, but they have actually prospered owing to decades of impunity. It is therefore time for the appropriate political, legislative and other bodies OUTSIDE THE APS, to begin suitable action for the containment of the problem of scientific ethics, IRRESPECTIVELY OF ANY ACTION THAT MAY OR MAY NOT BE UNDERTAKEN BY THE APS.

My primary reason for being optimistic is that the U.S. taxpayer is, today, a well educated and sophisticated person, possessing a rapidly expanding system of information, and capable of identifying, in full, ethically questionable occurrences even with minimal information.

As an example, if a politician appoints, as members of a review panel on National Laboratories, only members from Harvard, MIT, Yale and other leading colleges, the fellow taxpayer will instantly suspect a potentially unethical occurrence, without any need of looking at the wording of the final report. In fact, the fellow taxpayer is sufficiently sophisticated to understand that a serious study on National Laboratories should begin with the critical review of the institutions controlling the laboratories, that is, of the members of the panel itself! At any rate, more qualified members of review panels can be readily found abroad as well as at less politically entangled institutions. Only then, a review panel can be perceived as being truly intended in the interests of science, rather than in the interests of minoritarian groups.

To state it openly, the days when the selection of representatives (or referees, or reviewers) from leading academic institutions was synonymous of credibility, are over because of the relaxation of the ethical standards within the leading institutions themselves. Today, the selection of representatives (referees or reviewers) from said institutions could be a liability for academicians, administrators and politicians alike.

Similarly, if the APS will continue to ignore the need for the formulation and adoption of a *CODE OF ETHICS,* the fellow taxpayer will certainly see in this its most probable cause: the existence of corrupt, high ranking interests within the society which oppose the Code. Even if a Code is eventually formulated and accepted by the APS, but only as a powdery mask for inepts without genuinely effective rules, the fellow taxpayer will be able to see the deficiencies by just looking at the Code.

Above all, my reason for optimism is the fact that the contemporary U.S. taxpayer is fully capable of understanding all the essential TECHNICAL issues, to the point that, if one specific issue cannot be expressed in a form readily understandable by the taxpayer, that issue is not truly important. As a result, I believe that the fellow taxpayer can understand, in full, all the primary technical issues underlying this ethical probe on Einstein's

followers in the U.S.A.

I would like therefore to close this book with encouragement toward an appropriate mixture of precautionary pessimism, and contained optimism. in particular, I would like to encourage the fellow taxpayer to initiate an active role in the conduction of U.S. science, such as to call the directors of National Laboratories and inquire whether DIRECT experimental tests of the validity or invalidity of Einstein's special relativity in the interior of strongly interacting particles (pp. 143—170) are running there or not. If these experiments are not going on, and lesser relevant experiments continue to be preferred, the conspiratorial obscurantism suspected in this book on Einstein's relativities would be confirmed, and the grip of greed would still be in firm control of the sector.

If the U.S. taxpayer initiates such an active role in the conduction of the U.S. science, then, and only then, I see reasons for unlimited optimism for the containment of the problem of scientific ethics, as well as the basis for a new scientific civilization founded on intellectual democracy, with potential advances in human knowledge beyond our most vivid imagination.

APPENDIX A: THE EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH, GENEVA, SWITZERLAND.

In Fall, 1977, while being at the Lyman Laboratory of Physics of Harvard University, I applied to the European Organization for Nuclear Research (CERN) for a one year appointment as Scientific and/or Research Associate. The research program consisted of contacting local experimenters at CERN for the purpose of ascertaining the feasibility of experimental verifications of Pauli's exclusion principle under (external) strong interactions. The reader will recall from Sections 1.6 and 1.7 that the principle is a fundamental pillar of quantum mechanics. It was conceived by W. Pauli for the atomic structure and the electromagnetic interactions at large, under whose conditions it resulted to be strictly verified. The principle was subsequently assumed as valid under the different physical conditions occurring in the interior of nuclei, without any direct experimental verification. By recalling that physical knowledge is established quantitatively via experiments and not by theoretical beliefs alone, the proposal submitted to CERN suggested the initiation of a scientific process (the consultations with local experimentalists) that could subsequently lead to the resolution of this historical deficiency of contemporary physics.*

*Pauli himself had stressed in his limpid teaching that his principle had been conceived for physical conditions implying the lack of (appreciable) overlapping of the wavepackets of particles. These conditions are verified for the peripheral electrons of the atomic clouds because of very large mutual distances as compared to the size of the wavepackets. When the physical conditions are such to imply the overlapping of the wavepackets in an appreciable amount, we have the lack of necessary applicability of the principle for a number of well known technical reasons (such as the fact that the conditions imply the lack of necessary separability of the wavefunction, let alone the proof of its totally antisymmetric character). These latter conditions are exactly those in the interior of nuclei where particles are in appreciable conditions of mutual penetration, not only of their wavepackets, but also of their charge distributions. These are the well known historical roots of the doubts on the EXACT validity of Pauli's principle in nuclear physics which have been quantitatively studied by the hadronic generalization of quantum mechanics (Section 1.6). The understanding is that the APPROXIMATE validity of the principle is out of the question in nuclear physics. Thus, the objective of the research proposal submitted to CERN was that of resolving the issue in a quantitative way, that is, by establishing via direct experiment the QUANTITATIVE value of physical conditions in which the principle can be assumed as exact, with the complementary conditions being those within which the principle MAY be exact. The situation for the validity of Pauli's principle within the interior of a proton or a neu-

The application was acknowledged by W. Blair, Head of the Fellow and Associate Service at CERN with a note of January 31, 1978 (See the Docum. Vol. II, p. 445).

On March 14, 1978, I wrote the following letter to Blair (p. II–446):

"Dear Professor Blair,

I would like to express my appreciation for the courtesy of your letter of January 31, 1978, indicating that my application for a Scientific Associate Appointment will be considered at the meeting of April 11, 1978.

In this respect, I would like to indicate that a recent grant application with Professor Shlomo Sternberg, Chairman of the Department of Mathematics here at Harvard, to the U. S. Department of Energy (formerly ERDA), has been recently funded. As a result, I will have financial support for the next two academic years.

Owing to this new occurrence, I would like to confirm my application for a scientific associateship appointment, but modify my application for an appointment without salary."

The letter then continued with the indication that my research project was now part of an official program of the United States of America under administration by Harvard University, with my scientific associate being the chairman of Harvard's Department of Mathematics of that time. I also indicated that, owing to my commitments at Harvard, my visits at CERN could only be sporadic without any need of an office. The letter concluded by stating:

"Clearly, the issue I am referring to goes considerably beyond my capabilities as an isolated researcher. My interest in a scientific associateship at CERN is therefore twofold: I would like first to attempt to stimulate the awareness of CERN colleagues on the need to conduct the indicated experimental verification, in due time. Secondly, I would like to collect the personal viewpoints of experimentalists (on the technical difficulties for a possible verification) as well as theoreticians (on the reasons for or against such an experimental verification)."

W. Blair subsequently communicated the CERN decision to REJECT MY APPLICATION FOR HOSPITALITY via a letter dated April 18, 1978 (p. II–447).

I immediately contacted L. van Hove, CERN Director at the time, by expressing my doubts, in the strongest possible

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tron is much more nebulous and not resolvable in a direct quantitative way at this time. Within these smaller physical conditions, the validity of Pauli's principle is essentially inferred via the conjecture of the existence of yet unidentified sixteen, different quarks and sixteen different, unidentified antiquarks, and other assumptions. The validity of the principle then becomes a mere assumption following a primitive set of assumptions, none of which is established in a direct and incontrovertible way. This signals that we have left the arena of SCIENCE and entered the shadowy arena of ACADEMIC POLITICS, which is the ultimate essence of this appendix.

language, of the apparent existence of scientific corruption at CERN in the handling of the affair because:

- ★ I did not need any money (as stated in writing);
- ★ I did not need any office space (as also stated in writing);
- ★ I evidently did not need the use of any CERN equipment and/or facility;
- ★ The reasons for my interest for occasionally visiting CERN dealt with a known, historical, fundamental, open problem of contemporary physics not studied at CERN at that time (or thereafter); and, last but not least,
- ★ the CERN rejection implied the prohibition of occasional visits by a scientist under official support of the U.S. Government.

Regrettably, with the passing of time, I have lost the documentation of these letters with van Hove, as well as several additional exchanges we had throughout the intermediary action of a mutual acquaintance from Belgium. These letters are therefore missing in the Documentation of the case (Vol. II, pp. 444–447). The outcome of my complaints to van Hove are however absolutely incontrovertible and need no documentation. In fact,

- ■ van Hove did absolutely nothing of any value;
- ■ the prohibition for me to visit CERN remained strictly in force; and, last but not least,
- ■ no investigation whatsoever was ever initiated at CERN on the apparent scientific corruption underlying the affair.

Another thing should be crystal clear for the reader of this book. The very existence at CERN of one physicist studying the experimental verification of Pauli's principle under strong interactions, would have provided large damages to the vested academic–financial–ethnic interests there. In fact, the mere "consideration" of the experiments could have been perceived as an acknowledgment of doubts on the exact validity of the principle. In turn, the principle is a pillar of virtually all conjectures on quarks going on at that time at CERN and throughout the world. In fact, the compliance with Pauli's principle was instrumental in forcing quark supporters to invent the so–called notion of "color", which implied the multiplication of the number of conjectured, unidentified quarks (as well as large research contracts, numerous chairs in theoretical physics, and the like).

Years passed by without any event worth reporting here. Then, in 1982, van Hove resigned as CERN Director. His position was subsequently assumed by H. Schopper, a physicists from West Germany. I heard rumors in academic corridors that H. Schopper was bringing a "new wind" to CERN. This and

other aspects suggested my contacting Schopper for the purpose of recommending the initiation at CERN of experiments for the resolution of the validity or invalidity of Einstein's special relativity in the interior of hadrons.* The correspondence (reproduced in full on pp. II–465–477) turned out to be, not only useless, but actually damaging.

Regrettably, the problem of scientific ethics and accountability at CERN cannot possibly be treated in this appendix, inasmuch as it would require a separate, extensive report. Nevertheless, in the interest of Europe (as well as of the laboratory itself), it is appropriate to recommend the initiation of the consideration of the problem. Above all, the European press should keep CERN under constant scrutiny for ethical standards and scientific accountability, a task which has not even been initiated to this day, to my knowledge. Without such an independent appraisal of CERN research, the laboratory may well decay in time, despite its historical, outstanding, contributions to human knowledge.

*The proposal was esstentially that submitted to U.S. National Laboratories, such as the measure of the mean life of unstable hadrons at different energies (Section 1.7 and 2.3). The European taxpayer should be informed of the fact that CERN possesses all the necessary equipment to run this and other experiments in a matter of a few months by therefore resolving this fundamental problem of human knowledge. There must therefore be no doubt whatsoever on the fact that the LACK of experiments of such manifestly basic nature is due to a SPECIFIC, ORGANIZED, INTENT by vested interests in control of the laboratory, and NOT to the lack of equipment or insufficient technology.

APPENDIX B: AN ISLAND OF SCIENTIFIC FREEDOM: THE INSTITUTE FOR BASIC RESEARCH IN CAMBRIDGE, U.S.A.

[Reprinted from Hadronic Journal, Volume 6, 1967–1974, 1983]

1. HISTORY

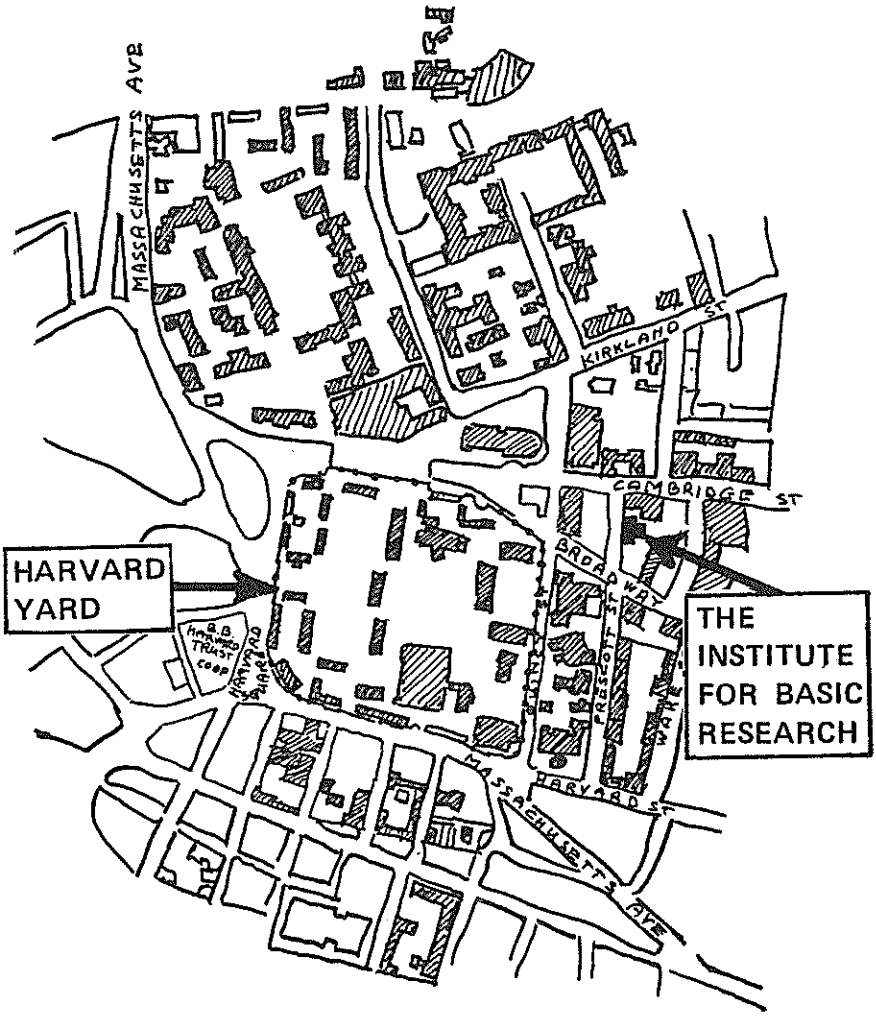
The mathematicians and physicists who led to the founding of the Institute for Basic Research (I.B.R.) initiated their gathering at the **First Workshop on Lie–admissible Formulations** *held at Harvard University in 1978. The group grew considerably in subsequent years. By 1981, it was clear that coordination of the research could be better accomplished by organizing a new, independent, institute.*

The I.B.R. was incorporated in Massachusetts on March 2, 1981, as a nonprofit academic institution with a charter similar to, but independent from, that of local institutions. The building known as the Prescott House, adjacent to Harvard University, was purchased on July 29, 1981, to provide permanent facilities for the I.B.R. in the heart of Cambridge's academic community. The building comprises 18 offices in the charming victorian style of New England. This number can be readily increased via suitable remodeling. In the absence of I.B.R. members, the offices are leased to individual scholars and graduate students of local universities.

Inauguration of the I.B.R. occurred on August 3, 1981, jointly with the initiation of the **Fourth Workshop on Lie–admissible Formulations.** *The ceremony was attended by the Governors, the Officers, and the Advisors of the Institute; representatives of the firms serving the Institute in accounting, law, and finance; and distinguished scientists from the U.S.A., and from Australia, Austria, Canada, Chile, France, Greece, Israel, Italy, Mexico, Sweden, Switzerland, Venezuela, and West Germany.*

To ensure long term stability and independence from fashionable research, the I.B.R. has been organized with a financial backing independent from government support. In fact, the I.B.R. has been founded via private funds, and has been operated via donations and volunteer work by the founders, officers, advisors, members, their spouses and friends.

The I.B.R. is a nonprofit academic corporation with federal tax exemption. All donations to the I.B.R. are, therefore, tax deductible in the U.S.A. under classifications 170(b)(1)(A)(vi) and 509(a)(1) of the Internal Revenue Code. The I.B.R. federal identification number is: 04–2750391.


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2. RESEARCH PROGRAMS

Lie's theory, with its diversification into algebras, groups and geometries, constitutes one of the most fundamental branches of contemporary mathematics.

A primary mathematical objective of the I.B.R. is the study of possible generalizations of Lie's theory [beyond grading–supersymmetric extensions]. Priority of research is given to generalizations of Lie algebras that admit generalized group and geometric structures.

A first generalization of Lie algebras of Lie–admissible type was proposed by A. A. Albert at the University of Chicago back in 1948. Additional generalizations of Lie–isotopic and Malcev–admissible type have been proposed by I.B.R. members, and they are currently under intensive mathematical study by a growing number of scholars.

Contemporary physical theories, such as classical mechanics, statistical mechanics and quantum mechanics, constitute a realization of Lie's theory beginning from their most fundamental dynamical part, the time evolution.

A primary physical objective of the I.B.R. is the study of possible generalizations of contemporary mechanics whose existence can be inferred from the generalized forms of Lie's theory provided by mathematical studies. The hope is to achieve a deeper and more refined description of physical systems that admit contemporary descriptions in first approximation.

By combining contributions in mechanics, algebras and geometries beginning from the past century, I.B.R. members have already succeeded in generalizing the contemporary formulation of classical mechanics for conservative systems, into covering mechanics possessing a Lie–isotopic and Lie–admissible structure for the closed and open description, respectively, of the systems of our physical reality, those with potential–Hamiltonian as well as contact–non–Hamiltonian forces. The generalized formulations have been called Birkhoffian and Birkhoffian–admissible mechanics, respectively, because of pioneering contributions made by G. D. Birkhoff in 1927.

The study of a generalization of quantum mechanics as operator image of the generalized classical mechanics indicated above is well under way, for the representation of strongly interacting particles (hadrons) as closed systems possessing an interior dynamics more general than that of the atomic structure. In turn, a generalization of quantum mechanics for the interior strong problem may assist in the resolution of some of the fundamental open problems of the theoretical physical of the last decades; identification of quark constituents with physical, already known particles; etc.

Additional applications of the advanced mathematical

and physical knowledge achieved by I.B.R. members can be foreseen in several other branches of contemporary human knowledge, ranging from theoretical biology to controlled fusion, or to computer modeling.

3. ORGANIZATION

To minimize costs, the I.B.R. research objectives are pursued via a combination of members actually working at the Cambridge premises, and members residing at other institutions. Therefore, joint membership at the I.B.R. and at other institutions is encouraged. Coordination is ensured by frequent contacts, periodical research sessions, and yearly workshops. Appointments are made under the titles of Full Professor, Associate Professor, Assistant Professor, and Research Assistant. All appointments are on a nontenured, yearly, renewable basis.

The I.B.R. is comprised of a **Division of Mathematics** *and a* **Division of Physics.** *A third* **Division of Applied Research** *[e.g., for energy] is currently under consideration. In Fall, 1983, the total number of I.B.R. members was 33 [11 mathematicians and 22 physicists]. By 1985, the total number of I.B.R. members is expected to be 50. Presently, 65% of I.B.R. members hold joint full professorship positions at other academic institutions in the U.S.A., Canada, Venezuela, Italy, Switzerland, France, West Germany, Israel and Pakistan.*

The I.B.R. is administered by a **Board of Advisors** *whenever necessary. General executive authority of scientific character is then invested in the President, while operational authority will be invested in a Director, who is to be appointed in the future.*

Several precautionary measures have been implemented by the founders of the I.B.R., beginning with the conception of the Charter, to ensure **genuine freedom in the pursuit of novel scientific knowledge.** *For instance, members of the I.B.R. have no authority in the appointments of new members, which are conducted by an outside Committee comprised of distinguished scientists and administrators in the U.S.A. and abroad.*

This organizational structure, which is apparently new in the U.S.A., has been implemented to minimize the formation of groups of scholars with vested interests in one given trend, and the not uncommon suppression of research along other trends, whenever said groups are invested with executive authority for new appointments.

4. EDITING

The I.B.R. considers editorial efforts an important aspect of its contribution to advanced scientific inquiry. Members of

the I.B.R. and non–members alike are involved in the Institute's editorial operations.

The I.B.R. houses the editorial office of the **Hadronic Journal,** *a journal on basic physical advances which is at its seventh year of publication under the Editorship of J. Fronteau (France), for Statistical Mechanics; R. Mignani (Italy), for Theoretical Physics; H. C. Myung (U.S.A.), for Mathematics; and R. M. Santilli (U.S.A.), as Editor in Chief; with an Editorial Council comprising several internationally known scientists.*

The I.B.R. also houses the Secretarial Office of **Algebras, Groups and Geometries,** *a new journal on fundamental mathematical advances that is scheduled to initiate publication in January, 1984, under the editorship of H. C. Myung (U.S.A.), with an Editorial Council comprising several distinguished scholars.*

Furthermore, the I.B.R. houses the Editorial Office of several yearly reprint series such as:

- **Hadronic Mechanics,**
  A. Schober, Editor;
- **Mathematical Studies on Lie–admissible algebras,**
  H. C. Myung, Editor;
- **Applications of Lie–admissible Algebras in Physics;**
  H. C. Myung, S. Okubo and R. M. Santilli, Editors;
- **A Nonassociative Algebra Bibliography,**
  M. L. Tomber, Editor;
- **Advances in Discrete Mathematics and Computer Science,**
  D. F. Hsu, Editor.

5. CONFERENCES

The organization of conferences, workshops, and summer schools in physics, mathematics, and other branches of science constitutes an important function of the I.B.R.

During the first year of operation, the I.B.R. organized the **Fourth Workshop on Lie–admissible Formulations,** *held in Cambridge, U.S.A., on August, 1981.*

The I.B.R. also participated in organizing the **First International Conference on Nonpotential Interactions and Their Lie–admissible Treatment,** *held at the University of Orléans, France, in January, 1982. The conference was attended by scientists from around the world, including official convoys from the U.S.S.R. and China. The conference resulted in the publication of four volumes of proceedings, for approximately 2,000 pages of research.*

The I.B.R. subsequently organized the **First Workshop on Hadronic Mechanics** *and the* **Fifth Workshop on Lie–admissible Formulations** *that were held jointly on the premises on August, 1983.*

Currently, the I.B.R. is organizing the **Second Workshop on Hadronic Mechanics** *to be held in Europe in Summer, 1984, and the* **Second International Conference on Nonpotential In-**

teractoins and Their Lie–admissible Treatment *to be held also in Europe in Summer, 1985.*

Additional workshops, and conferences on gravitation, computer science, philosophy of science and other fields are under consideration.

6. GUEST HOUSE

The I.B.R. is provided with a furnished, four–bedroom **Guest House** *located directly on the water's edge of Allerton Harbor, some 18 miles South of Cambridge. The Guest House has been used by several I.B.R. members, or visitors, their families and friends for brief stays and research sessions, amidst a beautiful natural environment, with stimulating walks on majestic shorelines of the Atlantic Ocean, and enchanting sunsets on the Boston Skyline. The Allerton Harbor houses three marinas, and is an ideal setting for all nautical recreational activities.*

7. MEMBERSHIP

Applications for I.B.R. membership can be submitted at any time to the

Admission Committee
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138, U.S.A.

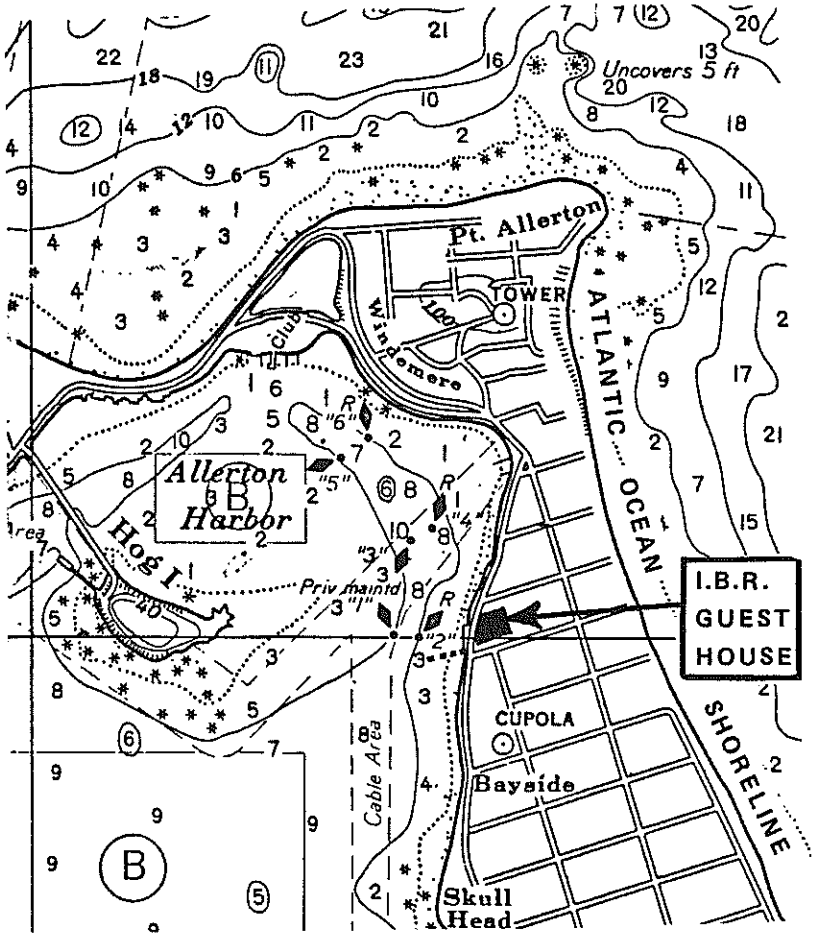
To avoid delays, all applicants should

1) *specify whether membership is desired in the Division of Mathematics or of Physics;*
2) *indicate the academic title the application is submitted for;*
3) *include a brief, one–page summary of current research interests;*
4) *provide a curriculum vitae et studiorum with a list of publications; and,*
5) *solicite at least three letters of recommendation to be mailed directly to the Admission Committee.* **(Important Note:** *The I.B.R. does not solicit letters of recommendation).*

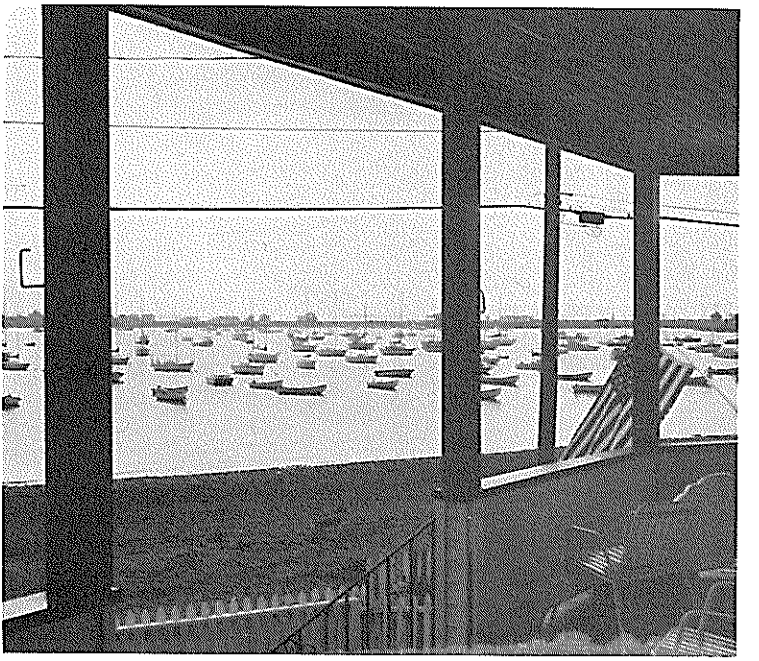
Members are initially appointed on a honorary basis without financial support and without any obligation . Appointments are structured to be compatible with pre–existing academic commitments.

I.B.R. membership can be disclosed [jointly with other memberships or individually] in publications, lectures, and all academic activities at large. However, such a disclosure is not obligatory, but only discretionary for each individual member.

I.B.R. membership provides a number of opportunities


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Anchorage
Allerton Harbor
Hog I.
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"1"
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"4"
"5"
"6"
ATLANTIC OCEAN
I.B.R. GUEST HOUSE
SHORELINE
CUPOLA
Bayside
Cable Area
Skull Head
B
B





such as:

- *participation in ongoing research activities, conferences, and editorial programs of the institute;*
- *possibility of initiating new, independent, research programs; organizing new conferences, workshops or summer schools; or launching new editorial programs;*
- *seeking financial support from governmental, corporate or private sources under I.B.R. administration whenever appropriate.*

The financing of general logistic expenses is the responsibility of the I.B.R. Board of Governors. The financing of individual I.B.R. members essentially rests in the initiative of each individual member.

The I.B.R. does not require a membership fee. Whenever possible, voluntary, tax–deductible donations depending on the individual capabilities, are welcome.

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ABOUT THE AUTHOR

RUGGERO MARIA SANTILLI received the (Italian equivalent of the) Ph.D. in theoretical physics at the University of Turin, Italy, in 1966. The subsequent year he moved with his family to the U.S.A. where he remained ever since, by holding research, faculty or visiting positions at a number of academic institutions, including Harvard University, The Massachusetts Institute of Technology, The University of Miami, Florida, and others. Currently, he is the President of The Institute for Basic Research, a new, independent research institution in physics, mathematics and other basic sciences located within the compound of Harvard University. Santilli has taught physics at all levels, from prep courses, to advanced seminar courses for graduate students. His curriculum includes

- ★ over 100 articles in particle physics published in various journals;
- ★ seven monographs in theoretical physics published by: Springer–Verlag, West Germany, Hadronic Press, U.S.A.; The University of Turin, Italy; and the Avogadro Institute, also in Turin;
- ★ Co–founding of a journal in theoretical physics, of a journal in pure mathematics and of numerous other editorial initiatives;
- ★ Co–organizing of several international workshops and conferences in physics and mathematics;
- ★ Co–founding of The Institute for Basic Research, Cambridge, U.S.A.

Santilli is a member of several scientific organizations, including the American Physical Society, the American Mathematical Society, and others. He is the recipient of a number of honors, including the Historical Medal of the City of Orleans, France, the Gold Medal of the Province of Campobasso, Italy, and others.

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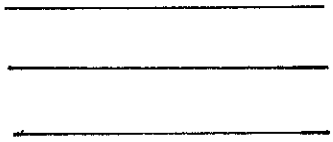
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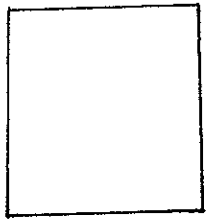
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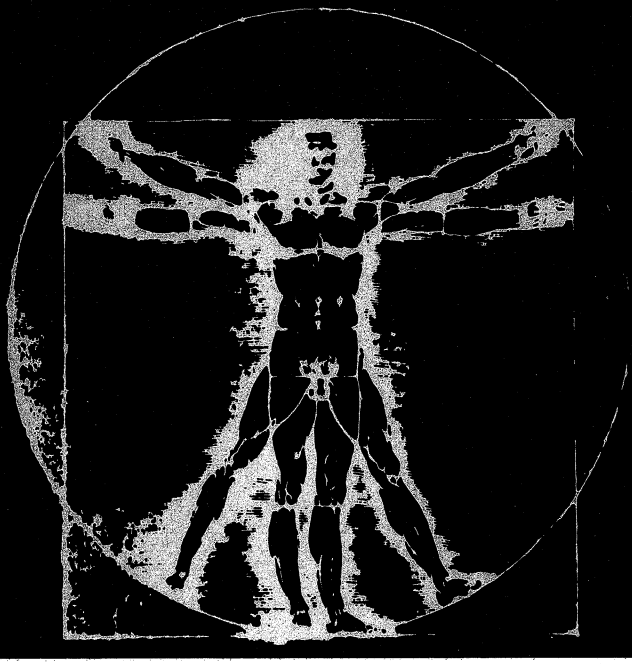
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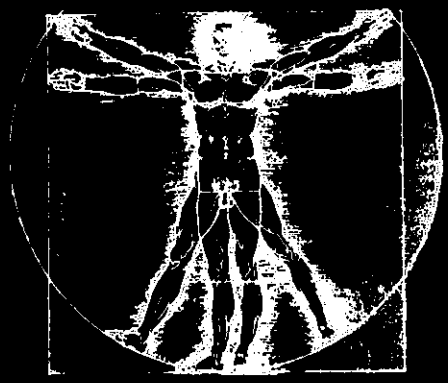
VOLUME

I

DOCUMENTATION

OF

IL GRANDE GRIDO





Ruggero Maria Santilli

DOCUMENTATION

OF

IL GRANDE GRIDO

Volume I

Ruggero Maria Santilli

— 1984 —
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OF
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VOLUME I

by

Ruggero Maria Santilli

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PART I:

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PART IA:

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YEAR
1977–
1978

HARVARD UNIVERSITY

OFFICE OF THE SECRETARY
17 QUINCY STREET

CAMBRIDGE, MASSACHUSETTS
October 24, 1977

SIR,

I beg to inform you on behalf of the University and the Dean of the Faculty of Arts and Sciences that you are appointed

Research Fellow in Physics

to serve from September 1, 1977 to June 30, 1978 subject to the Third Statute of the University (*overleaf*).

Your obedient servant,

Secretary to the University

Ruggero M. Santilli

STANFORD UNIVERSITY
STANFORD, CALIFORNIA 94305

DEPARTMENT OF PHYSICS

December 28, 1976

Dr. Ruggero Maria Santilli
Center for Theoretical Physics
M.I.T.
Cambridge, MA 02138

Dear Dr. Santilli:

Your papers look very interesting, and I will be very glad to meet with you to discuss these matters during your trip to California. If you call me when you arrive, I'm sure we will be able to arrange a convenient time to meet. My office phone number is (415) 497-2687.

Yours very truly,

Steven Weinberg

SW/dal

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 23, 1977

Prof. Ruggero Maria Santilli
Room 6-405A
Massachusetts Institute of Technology
Cambridge, MA 02139

Dear Professor Santilli:

Thank you for your resume. We will almost certainly be able to offer you a position as an Honorary Research Fellow. The department does not meet again as a group until September, so I cannot say anything official, but we do not expect any trouble with your appointment. We look forward to having you in our group.

If you have any further questions, please contact me.

Howard M. Georgi

Howard Georgi

HG/dt

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

July 1, 1977

Professor Ruggero Maria Santilli
Room 6-405A
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professor Santilli:

I am writing just to let you know that it has been possible to get a vote of this department, so that your position as Honorary Research Fellow is now official.

I look forward to getting to know you next fall.

Sincerely,

Steven Weinberg
Higgins Professor of Physics

SW/bm

September 5, 1977

Professor S. WEINBERG
Harvard University

Dear Steven,

I would gratefully appreciate your help in regard to the recent governmental invitation for supporting my research. Permit me to indicate, if at all needed, that I have no aim of remaining at Harvard. I am essentially interested in having Harvard administer the contract the first year and then move it to another campus where I have possibilities of tenure. Your help in the administrative steps to bring the invitation into a reality would be gratefully appreciated. As you know: I am currently unemployed; I have two children in tender age to feed and shelter; my wife is a graduate student; our savings are non-existent; and the unemployment benefit last only a few weeks.

Sincerely yours

V Ruggero

Harvard University, Lyman Laboratory September 1977
Informal Seminar Course

by Ruggero Maria Santilli

Title	**The Inverse Problem in Newtonian Mechanics and Field Theory**
Objective	a study of Lagrange's and Hamilton's equations
Means	1. integrability conditions for analytic representations of systems with couplings not necessarily derivable from a potential, 2. methods for the computation of a Lagrangian or, independently, of a Hamiltonian, when they exist, from given arbitrary systems, 3. outline of the applications to Newtonian mechanics, space mechanics, optimal control theory, plasma physics and classical field theory.
Prerequisites	knowledge of mechanics and differential equations
Duration	25 lectures of 1.5h each
Organization	(a) 50% of time dedicated to the methodology of the Inverse Problem for ordinary differential equations (Newtonian mechanics, etc.), (b) 30% of time dedicated to the extension of the methodology to partial differential equations (field theory), (c) 20% of time dedicated to applications and open problems, (d) several illustrative examples will be either worked out in class or distributed, (e) a number of homeworks will be assigned.
Reference	a limited number of copies of two forthcoming monographs on the Inverse Problem by R.M. Santilli (to be published by Springer-Verlag) will be made available.
Outline	enclosed in a tentative form
Organizational Meetings	**Time:** 4:30 p.m., October 3, 1977. **Place:** Room 267 (alternatively, the interested persons can contact R.M. Santilli at Room 437, Lyman Lab, Harvard University, office 495-3212, home 969-3465).

The Inverse Problem in Newtonian Mechanics and Field Theory

Course Outline

1. Introduction. Significance of systems with arbitrary couplings in Newtonian Mechanics, Space Mechanics, Optimal Control Theory and other disciplines. Need for the applicability of known analytic methods to the broader systems considered. Identification of the Inverse Problem.

2. Elemental Mathematics. Review of the existence theory for implicit functions, solutions and derivatives in the parameters. Review of the calculus of differential forms, the Poincaré-Lemma and its converse. Review of the calculus of variations.

3. Variational Approach to Self-Adjointness. Equations of variation, adjoint system, conditions of self-adjointness for quasi-linear second order systems. Reduction to first order systems and extension to arbitrary order.

4. Lagrange's and Hamilton's Equations. Hilbert Differentiability Theorem and basic properties. Equations of variation of Lagrange's and Hamilton's equations and their properties. Self-adjointness of Lagrange's and Hamilton's equations. Characterization of the ordered analytic representations of arbitrary systems.

5. Fundamental Theorems. Theorems on the necessary and sufficient conditions for the existence of a Lagrangian or, independently, of a Hamiltonian. Methods for the computation of these functions from the given equations of motion. Analysis of their structure from the viewpoint of the interactions and classification of the admissible couplings.

6. Application to the Newtonian Transformation Theory. Algebraic and geometrical significance of the conditions of self-adjointness. The new class of equivalence transformations of a Lagrangian characterized by its integrability conditions (isotopic transformations).

7. Application to Symmetries and First Integrals. Noether's theorem, its inverse and its generalization to higher orders. Use of the isotopic transformations for the computation of new first integrals. The concept of isotopically related symmetry groups, algebras and brackets.

8. Extension of the Inverse Problem to Field Theory. Conditions of variational self-adjointness for quasi-linear partial differential equations and relation to the conventional concept of self-adjointness in linear spaces. Fundamental theorems on the necessary and sufficient conditions for the existence of a Lagrangian density and its construction. Analysis of its structure and the relation with chiral Lagrangians. Application of the methodology to the transformation theory.

9. Outline of Applications. Electric circuits inclusive of losses. Nonlinear nonconservative plasma equations. Spinning top with drag torques. Space mechanics with drag forces for interplanetary dust. A missile trajectory problem. Nonconservative equations in continuum mechanics. Variational analysis of the Weinberg-Salam model of unified gauge theories of weak and electromagnetic interactions and the problem of its extension for the inclusion of strong interactions.

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Dr. R.M. Santilli
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WB/mc 2153

Dear Dr. Santilli,

Mr. Borsodi from Springer-Verlag New York sent me copies of your manuscripts: "The Inverse Problem in Newtonian Mechanics" and "Generalizations of the Inverse Problem in Newtonian Mechanics." He also sent me some information on Volume III, "The Inverse Problem in Field Theory," and your outline of September 1st referring to Volumes IV and V.

As editor of the series "Texts and Monographs in Physics" publihsed by Springer-Verlag, I think that at least parts I and II of your manuscript should be published. The opinions of various referees have been rather favorable and some of them even feel that your work might become a kind of standard reference on the subject. At the moment, almost nothing can be said about Volumes III-V and so I propose to offer you a contract for Volumes I and II only with the understanding that we will publish the following ones if they, too, stand up to the criticism of our referees.

In the case that you have any questions, please contact Mr. Borsodi in New York as you have in the past. He will also discuss the details of the contract with you. If you have any problems concerning the scientific part of your book, please do not hesitate to write to me.

Sincerely yours,

Dr. W. Beiglböck
(absent after dictation)

cc: Mr. Borsodi

Springer-Verlag KG
Postfach 105280
Neuenheimer Landstraße 28-30
D-6900 Heidelberg 1

Telephone : (06221) *487-1
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Telex : 04 . . .
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TO: Professors S. Weinberg, M. Tinkham and H. Georgi October 20, 1977
FROM: R.M.Santilli
SUBJECT: Second progress report [NOTE OF 5/25/78: THE FIRST REPORT IS MISSING FROM THE FILE]

I am now in a position of informing you of my studies on strong interactions as promised in my first report. In essence, I have been interested since the time of my graduate studies at the University of Torino (1963-1966) in studying the old idea that strong interactions are not derivable from a potential (e.g., Enrico Fermi) but with particular reference to the problem of the hadronic structure.

I have prepared for your consideration five highly condensed summaries of my efforts which are here enclosed. Besides submission, these papers have not been released. As for my first Harvard paper, my status is identified in the acknowledgments.

These are my first papers on the hadronic structure despite the fact that I have been literally working at the problem for over a decade. I think you might be interested at the reasons for such a delay as well as know in more detail my research program.

Monographs. Soon after the decision to study the problem indicated, I realized that the methods for the treatement of (local) forces not derivable from a potential were virtually ignored in the existing literature (of 1963-1964). It still is as of today. I therefore decided to undertake a long term and laborious program aiming at the identification of the methods needed for these forces.

As you know, I have worked since 1973 at three monographs on the so-called Inverse Problem of Analytic Mechanics (two monographs for the Newtonian aspect and one on the field theoretical aspect). They essentially identify the integrability conditions for the existence of analytic representations with conventional Lagrange's and Hamilton's equations (without external terms) of system with local but arbitrary couplings, the methods for the computation of these functions and a study of the significance of the methodology for other aspects (e.g., transformation theory, symmetries and first integrals for systems with arbitrary local couplings).

You will be pleased to know that these monographs have been officially accepted for publication by Springer-Verlag in their series "Monographs in Physics" under the title "Foundations of Theoretical Physics", Volumes I, II and III. This is a result of over one year of inspection of the manuscripts by European, US and USSR experts which appeared advisable from the novelty of the presented analysis. Regrettably, I had to decline offers from US publishers for several reasons.

This does not mean instant publication, as we know. And indeed I intend to release the manuscripts for publication only when I am sure that I have done a fine job. The informal seminar course I am currently delivering on these techniques is proving invaluable for my reaching the utmost possible maturity.

My true interest, however, is not the Inverse Problem per sé. The line of study in which I am actually interested is the so-called Lie-admissible problem. It essentially consists in the study of the same systems of the Inverse Problem (those with forces not derivable from a potential) but this time with the equations originally conceived by Lagrange's and Hamilton's, those with external terms. The intriguing aspect is that the presence of these external terms induces a covering of current analytic mechanics at all its level, analytic, algebraic, geometrical etc. For an outline

of my laborious efforts see Tables 3-7 of the enclosed note 4. You might be interested to know that the study of the Inverse Problem became mandatory in 1973 for a central technical difficulty I had identified at that time(and subsequently solved)in the construction of a genuine Lie-admissible covering of Lie's theory.

Springer-Verlag is aware of these efforts and interested in publishing them as Volume IV of my series if I have the time of organizing my published, submitted and unsubmitted notes which I have accumulated through the years.

Despite the availability of these new techniques, I was still unable to confront the contruction of an actual model of structure of the hadrons based on more general forms of the hadronic forces owing to the need (for technical reasons which you can identify in the enclosed papers) of estending the methodology to systems with generally nonintegrable subsidiary constraints. This called for a third stage of labor.

In 1976 I reached sufficient maturity to deliver a seminar course on the methods for the treatement of local systems with arbitrary forces and subsidiary constraints. My lecture notes are available in an untyped form. The essential aspects of these notes are: (a) the study of arbitrary constrained systems with the most poweful analytic tools known to me: the integrability conditions for the existence of Lagrangian or Hamiltonian formulations but this time reformulated on the hypersurface of the constraints, (b) the embedding of the currently available constrained systems of Dirac type into a more general class of Bolza type (as available from the Calculus of Variations and of particularly intriguing significance for short range, rather than electromagnetic, interactions) and (3) the identification of a yet broader class of the system considered (local, constrained, nonselfadjoint) which is only directly treatable with my Lie-admissible techniques.

Springer-Verlag is again interested in publishing my notes as Volume V of my series, provided that some time in the future I have the time to finilize them.

I should add that in all these manuscripts (for a total of some 4,000 pages), the terms "strong interactions" are ignored. The techniques are presented for their arena of unequivocal applicability: Newtonian Mechanics and Continuum Mechanics.

Research. Once the technique for the classical treatement of forces not derivable from a potential were sufficiently clear in my mind, I initiated my efforts aiming at the identification of the rudiments of their quantization. The use of Lie-admissible techniques turned out to be the most intriguing research topic in which I had been involved. Only after I had achieved such rudiments, I was finally able to confront the problem I had decided to attack a decade earlier: a tentative construction of a structure model of the hadrons with forces not derivable from a potential. The enclosed papers present a concise summary of my latest efforts.

It is an easy prediction that your inspection of these papers will demand your best patience, scientific vision and genuine interest in basic research. The reason is that, irrespective of a significant number of alternative presentations I had considered, the central nature of the research remained highly delicate:

the study of whether established relativity and quantum mechanical laws are applicable or not within a hadron. Neverthless, for the arguments presented in the papers, I believe that this problem must be seriously confronted sooner or later.

I would like also to add that at an initial inspection the content of these papers appear as incompatible with current trends in hadronic physics. In my opinion this is not the case. For instance, one of my central objectives is the study of the generalization of the Weinberg-Salam model of unified theory of weak and electromagnetic interactions with the inclusion of strong interaction as local couplings not derivable from a potential (technically this would be an SU(2)xU(1)-admissible embedding/breaking of the SU(2)xU(1) structure). I should also add that, since I had already worked out (and published) an SU(3)-admissible embedding/breaking of the SU(3) structure in 1966 with very intriguing results, I could have published preliminary studies on the indicated generalization of the SU(2)xU(1) structure a number of years ago. In my opinion, this would have been simply a scientific cheat. The reasons is that the use of local couplings not derivable from a potential either as a generalization or as a symmetry breaking mechanism of existing models has, in my opinon, profound methodological implications which are not transparent in Lie-type tools, but which are directly expressed by Lie-admissible tools. A central objective of the enclosed papers is to identify possible methodological implications of the forces considered even before considereding whether to study the Lie-admissible embedding/breaking of existing unitary gauge models.

Almost needless to say, the amount of efforts I have put in these studies has no bearing on their maturity. I still need a long way to cover to achieve even a preminary maturity.

ERDA support. ERDA has followed me through several years in these efforts. As you know, ERDA is now interested in supporting these studies, provided that I have Harvard sponsorship (an initial consideration for Boston University sponsorship was subsequently declined).

Therefore, I am hereby applying for any position of your choice at your Department which allows me to apply for an ERDA research grant either as principal investigator or, at least, as co-investigator.

In closing I would like to add that, despite my best efforts, no financial support from any other US campus is even conceivable (this is, in essence, an indication of the status of basic research in the USA). Despite my best dedication to this Country, and since I have a family of four to support, the decision which I must take in the near future is whether to continue my studies in the USA or accept an offer from a European institution interested in both the experimental and theoretical implications of my studies. Your solicit action on my request would be gratefully appreciated.

Office No. 495 3212, Home No. 969 3465. Sincerely RM Santilli

c.c: Professors S.R.Coleman, S.L.Glashow, A.M.Jaffe, R.V.Pound and C. Rubbia;
Drs. J.S.Kane, B.Hildebrand, W.A.Wallenmayer and D.C.Peaslee (ERDA).

I have a number of reviews of my studies by collegues. As an indication of their assessement I enclose a review letter by Abner Shimony (now at the Univ. of Geneva).

July 25, 1977

Dear Muggero,

I have spent the last three evenings reading the papers which you sent me, which of course is not time enough to do even partial justice to them. But at least I know much better what your program is, and how the parts of your work which I have examined before fit together. I see, in particular, what the long range goal of your monograph on mechanics was. I can only say that I am immensely impressed by what I have read. You have taken on one of the major physical problems of our times, you have proposed an unconventional but reasonable solution to it, and then -- and this is most impressive -- you have developed the intricate and wide-ranging array of tools needed to explore the consequences of your hypothesis. If your hypothesis turns out to be correct, you will have made a major contribution to human knowledge and you will be a very famous man. But even if your hypothesis does not turn out to be the correct explanation of hadron structure, you will have done great service developing these tools. For example, your proof that dynamical equations in situations where the forces are not derived from a potential require a Lie-admissible structure seems to me to be a great contribution to the understanding of classical mechanics. I am convinced that Lie-admissible algebras will be one of the powerful standard tools of theoretical physics, as your footnote on p.94 of paper VII (on the Gell-Mann-Okubo mass formula indicates). You have succeeded in beautifully unifying algebraic, analytic, and geometrical treatments of mechanics, which also contributes to an understanding of the subject. Incidentally, the amount of mathematical and physical literature which you had to master in order to do this seems to me truly astonishing.

Now let me comment on the physical plausibility of your main idea. The methodology of seeking a covering theory to current quantum mechanics and relativity theory in order to explain the physics within the hadrons seems to me thoroughly plusible. There is no a priori reason why concepts which have been succesful on the atomic and nuclear level should be ~~XXXXXX~~ equally successful within the hadrons.

Is it possible that a complete and detailed explanation of hadron structure requires a microscopic treatment of space-time, and that your hypothesis -- the role of a force not derivable from a potential -- is a kind of phenomenological consequence of the correct microscopic space-time theory? This way of looking at things would throw light upon one of the important considerations in your theory: that invariance under the Poincaré group holds for phenomena involving the hadron as a whole though not for its components. For deviations from Einstein-Minkowski space-time could be expected inside the hadron, but would be rathersmall outside it (except in very condensed states of matter, such as neutron stars).

I am delighted that you will be Weinberg's guest next year. Not only does that help you out for the coming year, but it may open doors for the future. He is knowledgeable enough to appreciate what you are doing, and has enough prestige that a recommendation from him may obtain a permanent position for you. What you have accomplished is so impressive that you deserve a great award.

With best wishes

[redacted]

RESEARCH GRANT PROPOSAL SUBMITTED TO THE NATIONAL SCIENCE FOUNDATION

Proposed Amount: $ 99,212; Proposed Effective date: Jan. 1, 1977
Proposed duration: 24 Months

Title: NECESSARY AND SUFFICIENT CONDITIONS FOR THE EXISTENCE OF A LAGRANGIAN IN NEWTONIAN MECHANICS AND IN FIELD THEORY

Principal Investigator: Ruggero Maria Santilli
Social Sec. No.: 032-46-3855

Submitting Institution: Boston University
Department: Physics
Branch: Graduate School
Address: 111 Cummington St, Boston Mass. 02215

Endorsements:

Principal Investigator

Name : Ruggero Maria Santilli

Signature: [signature]

Title : Associate Professor

Tel. No. : (617) 353 2187

Department of Physics

Name : George O. Zimmerman

Signature: [signature]

Title : Chairman

Tel. No. : (617) 353 2623

B.U. Administrative Official

Name : _____

Signature: _____

Title : _____

Tel. No. : _____

TO: Professors S. Weinberg, M.Tinkham and H. Georgi December 4, 1977
FROM: R.M.Santilli
SUBJECT: Last progress report

As you have eventually noted, the five short papers on my independent studies on the problem of the hadronic structure I gave you with my second report of October 20, 1977, were highly condensed. In essence I prepared them to let you have a full knowledged of my current research activities in the shortest possible time. Since these notes tentatively bear the name of your department, no copy has been circulated besides those as stated in my second report.

What I did circulate rather widely and in a confidential way prior to my arrival at your department was a series of papers (for over 1,000 pages) giving full technical details of my studies. Predictably, the publication of this series turned out to be practical impossible, despite the acceptance of the technical content by referees, simply because their publication costs would have exceeded $ 25,000.

I therefore submitted this series of papers for publication as monographs several months ago. You might be interested to know that these monographs have been accepted for publication (with quite encouraging referee's reports) by Hadronic Press, Inc., Nonantum Massachusetts 02195 (a new press recently organized by local physicists) according to the following reorganization of the material:

"Lie-admissible approach to the Hadronic structure",
Volume I: "Nonapplicability of the Galilei and Einstein relativities", 537 pages. This is a technical treatement, by using the methodology of the inverse problem, of the content of the note # 2 (for 10 pages) in your possession concerning the possible inapplicability of the established relativity ideas to strong interactions in general and the strong hadronic forces in particular when assumed as local but not derivable from a potential.

Volume II: "Covering of the Galilei and Einstein relativities", 487 pages. This is a technical treatement, by using this time the complamentary Lie-admissible problem, of the content of the note # 4 (for 12 pages) in your possession concerning my efforts aiming at the identification of the Lie-admissible covering of established relativity laws for the strong hadronic forces assumed as local but nonderivable from a potential. The volume also contains the basic ideas of the Lie-admissible embedding/breaking-generalization of current symmetry breaking models.

Volume III: "Identification of the hadronic constituents with physical particles", 343 pages. This is a technical treatement of the content of the note # 5 (for 26 pages) in your possession. It deals first with my rudimentary efforts aiming at the quantization of local hadronic forces nonderivable from a potential by using the methodologies of the inverse problem and the Lie-admissible problem. An explicit structure model of the light hadron is then constructed and the predictions compared with the available experimental data. Finally, the volume touches on the problem of the future experimental orientation of high energy physics, with particular reference to the problem of the validity or invalidity of Pauli's exclusion principle and Einstein's special relativity within a hadron, which appears to be needed to provide a physically effective selection among an ever increasing number of models on the hadronic structure.

Volume I will be printed soon. Volume II will be released in spring 1978 and Volume III will be released subsequently. Hadronic Press, Inc. is currently preparing a brochure on the appearance of these volumes for world wide distribution, with comments by a number of scientists.

I have managed to have a copy of Volume I made at my expenses for Howard Georgi. I would be happy to provide copies to any interested collegue but, quite candidly, I do not have the money to do it at my expenses.

It is appropriate here to stress that these monograph, by no means, are intended to indicate that my approach to the hadronic structure is better than others. This comparative study is entirely left to interested independent researchers. Their objective is merely to provide a report on the results of my laborious and solitary journey which, as you know from my report of October 20, lasted for over a decade: the current state of the art on the hadronic structure is such that other approaches are equally conceivable at this time.

I am, of course, fully aware that the content of these monographs is contrary to the rather vast financial enterprise which has been constructed through the years by the governmental-academic complex on the concept of quark and related unitary models. Nevertheless, their finalization is, for me, a question of scientific integrity which is, in this instance, contrary to a career oriented attitude. To be quite candid on this point, I believe that the problem of the hadronic structure must go beyond the interests of currently supported groups of research and, to have a well balanced community of researchers genuinely dedicated to basic studies, all conceivable alternatives must be investigated and subjected to a comparative confrontation with the physical reality. The current restriction of the studies on the hadronic structure to only these trends which are supported by governmental agencies and their referees, in my opinion, is contrary to the spirit of basic research.

I hope that these remarks will not be misrepresented, but simply accepted with benign patience and understanding of my "stato d'animo" due to the extreme difficulties which I have found in the conduction of these studies in this Country.*) I hope, however, that you are by now aware that these difficulties have strenghtened, rather than weakened, my determination in their conduction.

The finalization of my monographs on the Inverse Problem with Springer-Verlag is proceeding on schedule. The first volume will be released on January 1978 and the second volume will be finalized during the summer of 1978..

The remaining period of my stay at your department will be devoted to the finalization of these projects. Therefore, I see no need of further progress reports.

In closing, I would like to note with regret that my application of October 20, 1977 for "any" (not necessarily supported) position which allows me to apply for a research grant, has remained unacknowledged. I am therefore proceeding toward the acceptance of a position from a receptive European Institution.

c.c.: Professors S. Coleman, R. Glauber, S. Glashow, A. Jaffe, R. Pound and C. Rubbia.

*) Up to the current status of being unemployed with a family of four and wife at the graduate school.

STATEMENTS PRINTED BY
HADRONIC PRESS INC
on promotional leaflets regarding
the research monographs

Lie–admissible Approach to the Hadronic Structure

Vol. I: Nonapplicability of the Galilei and Einstein's Relativity? (1978)

Vol. II: Coverings of the Galilei and Einstein relativities? (to appear)

Vol. III: Identification of the Hadronic Constituents with Physical Particles? (to appear)

Ruggero Maria Santilli
Harvard University
Lyman Laboratory of Physics
Cambridge, Massachusetts 02138

"The monographs by Professor Santilli constitute a landmark in the study of the inverse problem in classical mechanics and field theory emphasizing its effectiveness and the significance of the underlying methodology for the problem of interactions."
R. MERTENS, Professor of Theoretical Mechanics and Director, Instituut voor Theoretische Mechanica, Rijksuniversiteit, Gent, Belguim.

"Professor Santilli's undertaking is a courageous quest which demonstrates endless enthusiasm and profound familiarity with a vast and generally neglected literature."
P. ROMAN, Professor of Physics, Department of Physics, Boston University, Boston, MA, USA

"Professor Santilli's monographs systematically and imaginatively explore the hypothesis that the hadronic interactions are not derivable from a potential. In the course of his exploration he makes important investigations on the necessary and sufficient conditions for the existence of a Lagrangian and of the properties of Lie–admissible algebras. He has possibly developed a very fruitful new approach to hadronic structure and he has certainly made great contributions to the mathematical methods of theoretical physics."
A. SHIMONY, Professor of Physics and Phylosophy, Boston University, currently visiting The Département de Physique Théorique of the Université de Géneve, Switzerland.

December 13, 1977

Professor Shlomo Sternberg
Chairman
Department of Mathematics
Harvard University
Cambridge, Massachusetts 02138

Dear Professor Sternberg,

This is to acknowledge my appreciation for your sponsorship of the research proposal with ERDA. I would also like to indicate that in the case that the proposal is funded for a two year period, the Department of Mathematics at Harvard University, you and/or any of your colleagues would in no way be accountable for either a continuation of my stay or my relocation after June 30th, 1979.

Very truly yours,

Ruggero Maria Santilli

RMS/mjm

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

JEFFERSON PHYSICAL LABORATORY
CAMBRIDGE, MASSACHUSETTS 02138

December 14, 1977

Dr. R. M. Santilli
Physics Department
Harvard University

Dear Dr. Santilli:

I am writing to acknowledge receipt of your progress reports and to indicate why I had not made any earlier response.

I had brought your request before our faculty on several occasions. However, none of them felt that your work was close enough to their active research interests to allow them properly to serve as Principal Investigator. And, I am sure that you have had the Harvard rules made clear to you which allow research grants to be requested in Harvard's name only by those holding substantial academic appointments, of a level which we could not offer to you. Such appointments must be reviewed and approved by the Dean of the Faculty, and they are made only for normal academic reasons.

When last I had brought up your case, I was told that there was a good chance the Professor Sternberg of Mathematics might feel able to serve as Principal Investigator. We all thought that that might form a satisfactory resolution of the difficulty, and accordingly I thought it appropriate to defer my response while that possibility was being explored.

Sincerely yours,

M. Tinkham
Chairman

MT/ew
cc: H. Georgi, S. Weinberg, R. V. Pound

January 15, 1978

Professor M. Tinkham
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

I am in the process of releasing the enclosed material related to my monographs with Springer-Verlag and Hadronic Press.

Please inspect the expressions of appreciation for your hospitality I have included in the acknowledgments and feel free to recommend any modification you desire.

Best Personal Regards

Ruggero Maria Santilli

c.c. Professors S. R. Coleman, H. Georgi, S.L.Glashow, R. J. Glauber, A. Jaffe and S. Weinberg.

P. S. I also enclose copy of the announcement which has been recently distributed concerning the organization of the Hadronic Journal.

Acknowledgments

I simply have no words to express my gratitude to A. Shimony. It is a truism to say that, without his encouragement, support, and advice, this work would not have been completed.

I would also like to express my sincere gratitude to A. C. Hurst, S. Shanmugadhasan, and P. L. Huddleston for carefully reading an earlier version of the manuscript and for numerous suggestions.

I am also sincerely grateful to my graduate students J. Eldridge and A. Sen for carefully studying an earlier version. Their various suggestions were very valuable in the overall improvement of the manuscript.

Correspondence on several historical or technical points with H. Corben, P. Dedecker, D. G. B. Edelen, G. M. Ewing, A. Harvey, P. Havas, R. Herman, M. R. Hestenes, C. B. Morrey, L. A. Pars, E. G. Saletan, D. C. Spencer, P. Stehle and K. R. Symon must be acknowledged.

Special thanks must be acknowledged to:

R. Mertens, E. Engels, F. Cantrijn, and W. Sarlet. Their frequent assistance has simply been invaluable for the entire project.

H. Rund and D. Lovelock. Their advice on the use of the calculus of differential forms has been invaluable for the proof of the main theorems.

A. Thellung and J. Kobussen. Their suggestions and comments have also been invaluable.

L. Y. Bahar and R. Brooks. Their critical reading of the manuscript has been invaluable.

C. N. Ktorides. His penetrating critical comments have been equally invaluable.

It is a pleasure to thank F. E. Low, H. Fesbbach, and R. Jackiw for their hospitality at the Center for Theoretical Physics of the Massachusetts Institute of Technology in 1976–1977, where part of this project was conducted.

It is a pleasure to thank S. Weinberg, M. Tinkham, and H. Georgi for their hospitality at the Lyman Laboratory of Physics, where this project was completed. In particular, I would like to express my appreciation for the opportunity of delivering an informal seminar course on the Inverse Problem at the Lyman Laboratory during the fall of 1977, which proved to be invaluable for the finalization of this project.

D. Nordstrom gave generously of his time and experience in assisting with the editorial preparation of the manuscript.

I would like also to express my gratitude to the Editorial Staff of Springer-Verlag for their invaluable assistance in the finalization of this project.

Almost needless to say, I am solely responsible for the content of this volume, including several modifications implemented in the final version of the manuscript.

It is a pleasure to acknowledge support from the U.S. Department of Energy under contract no. ER-78-S-02-4720.A000.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 2, 1978

Dean RICHARD G. LEAHY,

Harvard University

Dear Professor Leahy,

You will be pleased to know that my research grant application to the Department of Energy (formerly ERDA) with Professor Shlomo Sternberg as principal investigator has been accepted by the High Energy Physics Division of DE and sent to the DE Administrative Office with a recommendation for funding. Copy of a letter in this respect is enclosed.

Subsequently, the DE Administrative Office entered in contact with Ms. Cheryl Peyton, ORC, for the finalization of the procedures. A revised front page and budget of the contract has been prepared as per DE specification. A copy is enclosed.

I would appreciate the courtesy of setting up an account number on my name and sending it to Ms. Cheryl Peyton for approval. This is needed so that I can receive a salary beginning from April 1, 1978.

Best Personal Regards

Ruggero Maria Santilli
honorary research fellow

c.c.: Prof. M. Tinkham
Ms. C. Peyton

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

April 6, 1978

Professor M. Tinkham
Chairman
Department of Physics
Harvard University
Cambridge, Ma 02138

Dear Professor Tinkham,

I am here respectfully applying for a change in my current status of honorary research fellow to research associate from March 1, 1978 until May 31, 1979.

As you eventually know, the formalities for the research contract with DOE have either been concluded or are in the process of being concluded, according to the enclosed material. This change in title is apparently needed so that I can receive a salary from this contract (I have been told that a "honorary" position does not allow reception of salary).

On more specific grounds, it appears that I need an appointment as research associate from March 1, 1978 until May 31, 1979 (which is the duration of the contract) at a salary of $ 30,000.00 payable in monthly installements of $ 2,000.00 as per approved budget (copy is enclosed) to be charged to the Harvard code no. 33|966|7131-2 (as per enclosed letter from ORC).

I would appreciate the possibility of keeping my desk in room 437, Lyman Laboratory, for the same period of time. All expenses (xerox, etc.) will however be charged to the DOE contract. Therefore, I do not expect direct expenditures on my behalf. The monthly accounting procedures for non-salary expenses will be administered by the Department of Mathematics.

Almost needless to say, my request is for a temporary, terminal, research appointment. Under no circumstances, either direct or indirect, should Harvard University be responsible for my salary after May 31, 1979 or for my relocation.

Very Truly Yours

Ruggero Maria Santilli

C.C.: Professor S. Sternberg, Dept. of Math.,
Ms. Cheryl Peyton, ORC

HADRONIC JOURNAL
NONANTUM, MASSACHUSETTS 02195, U.S.A.
A Publication of
HADRONIC PRESS, INC.

Ruggero Maria Santilli
Editor In Chief

April 15, 1978

Professor S. COLEMAN,
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Sidney,

The enclosed manuscript has been reviewed or is currently under review by a number of collegues (physicists and mathematicians). The general consensus is quite encouraging for publication. Some collegue is even enthusiastic (e.g. A. Shimony, as you know). I have received numerous critical comments of details for the finalization of the manuscript which I will take in full account in the final version (jointly with the due additional acknowldegments).

Rather than being pleased, I am somewhat dissatisfied (on scientific grounds) because I did not received truly penetrating CRITICAL remarks, that is, candid, open, strong criticism on the conceptual foundations of the conjectured covering of the Galilei relativity.

Owing to this situation, I would be truly grateful whether you can inspect the conceptual lines of the project and let me know your critical remarks in any form you prefer (verbal or otherwise). You can rest assured that your possible assistance will meet with my utmost discretion as well as sincere gratitude. Without doubt, this is one of the most delicate research steps of my academic life for which I need all the qualified advice I can get. The manuscript is supposed to be released for printing on May 1, 1978.

In relation to the stile of presentation, let me indicate that it is the result of numerous draftings and redraftings. By specific intent the paper is written in a form to be provocative in the hope to stimulate a scientific debate (hopefully, also in the interest of the Hadronic Journal). However, I discovered that it is difficult to be provocative in a moderate, well balanced way. Your advice whether the current stile need serious changes would be very much appreciated. I am here referring to Sections 1, 3.9 and 5.

In relation to the content, please take into consideration that one of the objectives of the Hadronic Journal is that of being as far as possible detached from the sea of minute incremental papers or of stuffy research which, in my opinion, has now reached suffocating proportions. To be candid, it appears that the courage, imagination and vision of the founders of contemporary theoretical physics is simply dead these days. What I see instead, with the due exceptions, is a pletora of followers resisting even the consideration of fundamental issues. One of the objectives of the Hadronic Journal is to stimulate new ideas which, as such, are purely conjectural. But this, of course, cum grano salis: the conjecture must have a well defined degree of plausibility. Along these lines, the content of the enclosed paper is purely conjectural. What I would appreciate is some assistance in the evaluation of the degree of plausibility.

Sincerely

Ruggero

RMS|ls

Ruggero Maria Santilli

Rudimentary draft for confidential communications

ON A POSSIBLE LIE-ADMISSIBLE COVERING OF THE GALILEI RELATIVITY IN NEWTONIAN MECHANICS FOR NONCONSERVATIVE AND GALILEI FORM-NONINVARIANT SYSTEMS.*

Ruggero Maria Santilli •
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Submitted on January 16, 1978
Revised version submitted on April 3, 1978
Final version submitted on

ABSTRACT

In order to study the problem of the relativity laws of nonconservative and Galilei form-noninvariant systems, two complementary methodological frameworks are presented. The first belongs to the so-called Inverse Problem of Classical Mechanics and consists of the conventional analytic, algebraic and geometrical formulations which underlay the integrability conditions for the existence of a Lagrangian or, independently, of a Hamiltonian. These methods emerge as possessing considerable effectiveness in the identification of the mechanism of Galilei relativity breaking in Newtonian Mechanics by forces not derivable from a potential. Nevertheless, they do not exhibit a clear constructive capability for a possible covering relativity. For this reason, the second methodological framework is presented. It belongs to the so-called Lie-Admissible Problem in Classical Mechanics and consists of the covering analytic, algebraic and geometrical formulations which are needed for the equations originally conceivedby Lagrange and Hamilton, those with external terms. These formulations are characterized by the Lie-admissible algebras which are known to be genuine algebraic covering of Lie algebras, and which in this paper are identified as possessing (a) a direct applicability in Newtonian Mechanics for the case of forces not derivable from a potential, (b) an analytic origin fully parallel to that of Lie algebras, i. e., via the brackets of the time evolution law, (c) a covering of the conventional canonical formulations as classical realizations, (c) an implementation at a number of levels of Lie's theory, (d) a fundamental realization as universal enveloping nonassociative algebras, (e) a generalization of symplectic and contact geometry as geometrical backing and (f) the capability of recovering conventional formulations identically at the limit of null external forces, here interpreted as relativity breaking forces. A covering of the Galilei relativity, called Galilei-admissible relativity, is then conjectured for independent scrutiny by interested researchers. A number of potential implications, particularly for hadron physics, are then briefly considered for future detailed treatment.

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* To be published in the HADRONIC JOURNAL, Volume 1 (1978).

• Supported in full by the U. S. DEPARTMENT OF ENERGY under contract number ER-78-S-02-4742. A000.

April 24, 1978

TO: Professor M. TINKHAM
FROM: R. M. SANTILLI
SUBJECT: Removal of the word "honorary" from my title as per application of April 6, 1978

This is to inform you that a duly executed copy of the research grant

DOE number ER-78-S-02-4742

has arrived. A copy of the formal notification by the Department of Energy is enclosed for your consideration.

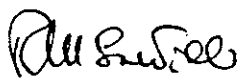
I would be truly grateful whether a decision on my application of April 6, 1978 can be reached in the near future, so that I can receive the salary allocated to me by the DOE grant.

I would like to take the personal liberty of indicating in this respect the expectation by Professor S. STERBNERG that, whether possible, the removal of the term "honorary" from my title be conducted within a physics institution.

You might be interested to know that the topic of the research (which I have sumbitted to numerous collegues as rudimentary drafts for confidential communications) is meeting with a quite encouraging interest which, in any case, is beyond my rather cautious expectation.

If I can be of any assistance, please do not hesitate to contact me.

Thank you for your consideration and time.



c. c.: Professors S. Coleman, H. Georgi, S. Glashow, R. Pound and S. Weinberg.

TO: Professor M. TINKHAM April 26, 1978
FROM: R. M. Santilli
SUBJECT: Application of April 6, 1978, for the removal of the term "honorary" in my title.

It is not without considerable embarassement that, following our phone conversation of this afternoon, I release copies of some of the letters of comments by collegues on my studies related to the grant DOE-ER-78-S-02-4742 (which is now in full effect, to my understanding). Please feel free to contact these individuals, if you so desire. Neverthless, I would appreciate your discretion on the disclosure. A number of additional letters from distinguished collegues is also in my file, but I cannot disclose them prior to the formal authorization by their authors.

As you can see, some of the enclosed letters of comments are excessively optimistic. I hope that you simply consider them as an expression of the genuine enthusiasm which is growing on these studies (the first issue of the Hadronic Journal will publish three papers on Lie-admissible algebras- by three different authors- and truly intriguing papers are forthcoming).

It is my impression that my senior collegues are basically noninformed of my studies. Actually, few physicists are informed at this moment. As you know from my progress reports of the past fall, my studies are presented in two series of monographs, one with Springer-Verlag (now in print) and one with Hadronic Press (which will be printed when I find the courage to release the manuscripts). Any inspection of my studies, to have sufficient value, demands the inspection of all these volumes because they contain the proofs (or, I should say, my efforts at the formulation and proof of) a number of crucial theorems and a tentative elaboration of their possible physical relevance. By today's standard (according to which contributions are assessed in 60 or, maximum, 90 seconds of inspection) the study of these manuscripts is a possibility of few.

These two series of monographs are devoted to two different but complementary, classical and quantum mechanical, possible methods for the treatment of local forces not derivable from a potential (the Inverse Problem and the Lie-admissible Problem). They are basically presented for the arena of their potential direct physical relevance: nonconservative systems in applied physics, mathematics and engineering (you might inspect the enclosed comments by Professor L. Y. Bahar, an engineer, on the applications to energy-related issues). Equivalently, these studies are inspired by the original conception by Lagrange and Hamilton of preserving external forces in their analytic equations to avoid an excessive approximation of physical reality. And indeed, in my teaching of mechanics, I stress that the customary treatment of, say, the spinning top under gravity is noting but a conservative abstraction fully equivalent to the acceptance of the "perpetual motion" (for your amusement, I have enclosed a "vignetta" depicting the operation of truncation of the analytic equations originally conceived by Lagrange and Hamilton which has occurred in recent times).

Neverthless, since my initial letters of application to Steven Weinberg of the spring 1977, I have candidly confessed that the reason which stimulated this laborious and solitary journey is to be able to study, in due time, the old idea that the strong interactions in general, and the strong hadronic forces in particular, are (partially) nonderivable from a potential. You are aware that this is a line of study which was strongly suggested by the founders of contemporary physics (e. g., by Enrico Fermi even before I was born). With an understanding that this approach is substantially unconventional by today's trends, the epistemological argument is quite simple. Unlike the corresponding occurrence at the nuclear level, the charge volume of hadrons does not sensibly increase with mass and it is of the same order of magnitude of any other, physically known, charged particle. If the hadronic constituents are assumed as being non-point-like, that is, of possessing a finite charge volume, the hadronic structure emerges as being substantially different than the atomic and the nuclear structure. In rudimentary words, the dynamical evolution demands a continuous status of penetration of the charge volume of each constituent with that of the others, i. e., a type of motion which likely calls for nonlocal forces. However, local forces not derivable from a potential are known to be a good approximation of these forces. The net effect is that a rudimentary, primitive, Newtonian limit of this hadronic structure can be conceived as a state which is conservative as a whole, but the constituents are in highly nonconservative conditions. The possible direct applicability of Lie-admissible techniques is then consequential.

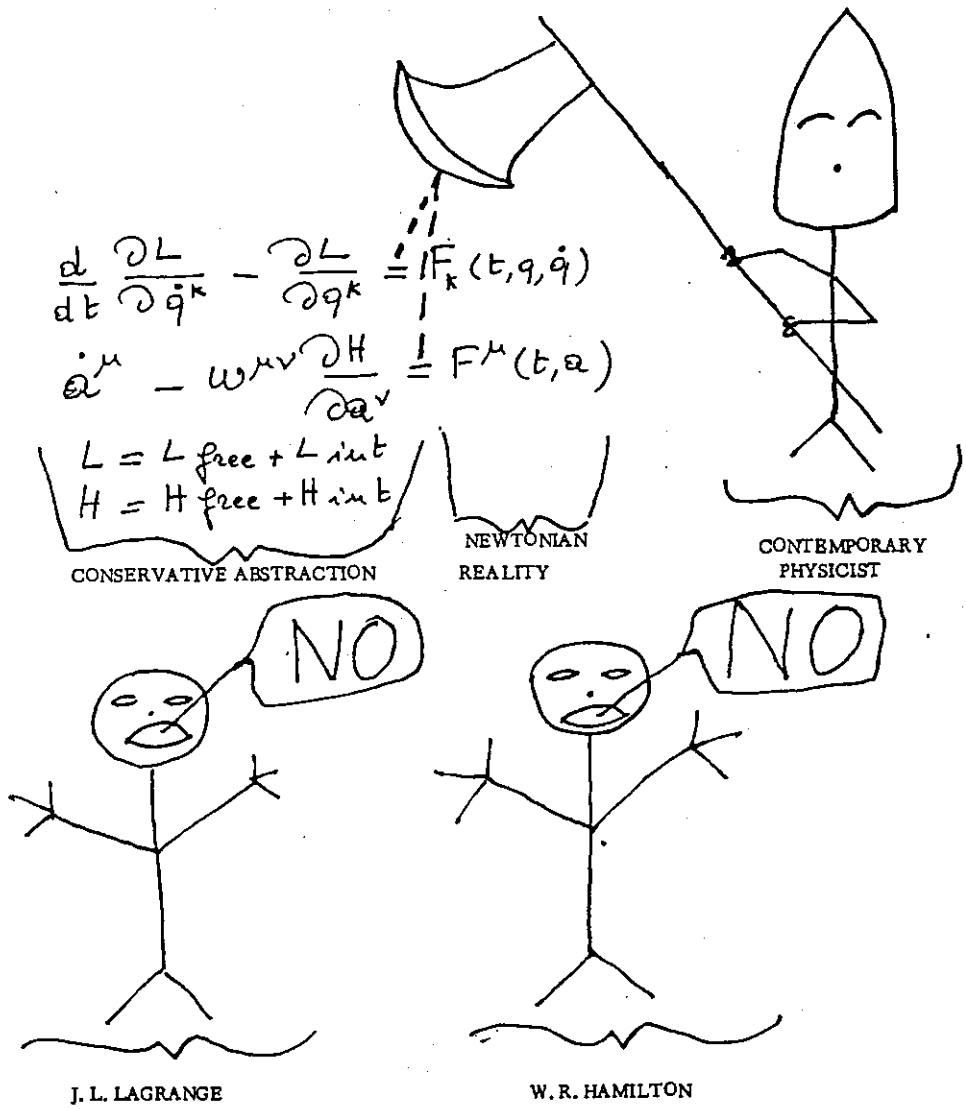
No physicist (and, first of all, myself) can today establish whether this picture of the hadronic structure is correct or grossly erroneous and, in my opinion, the resolution of this issue will perhaps demand generations. The following three aspects, however, deserve a comment.

(A) This line of study MUST be conducted, of course, as a minor complement of the current major stream of studies, in the traditional spirit of unsolved physical problems. Your are aware of the growing severe judgement by an increasing number of physicists on any attempt of restricting the studies on the hadronic structure along only one line. This would be equivalent to scientific loss, because deprived of the primary function in basic research, as well as for human knowledge, of scientific debates. You are also aware of my determination in the continuation of these studies. Finally, you are aware of my eagerness and sincere gratitude toward any scientifically productive criticism or collaboration. I believe I have given proof to all my referees of providing a genuine implementation of all their valuable suggestions. The complexity of the problems to be confronted and, hopefully solved in due time, demands the most serious consideration of any viewpoint. In any case, I have found the courage to release a first paper for publication on Newtonian relativity aspects (this is my first formal release to outsiders-other than confidential reviewers).

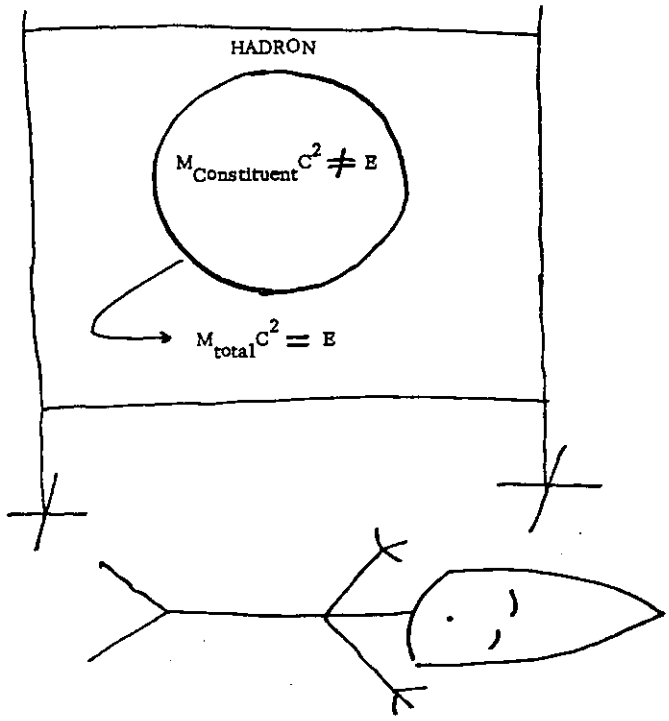
(B) At the risk of jeopardizing my personal reputation and of even being considered demented by superficial collegues, it is my moral and scientific duty, as a researcher, to point out in a way as clear as possible the technical implications of the assumptions that the strong hadronic forces are not derivable from a potential, as they appear to me. Simply stated, it appears that while our current knowledge unequivocally applies to hadrons as a whole (and as experimentally established) the applicability to the hadronic constituents under (and only under) the assumed forces is in doubt. For instance, field (Newton's) equations can apparently be proved to be noncovariant under the Poincaré (Galilei) group; conventional quantization procedures (up to the Wightman's axioms) can apparently be identified as inapplicable; the customary concept of perennal value of quantized spin (and Pauli's exclusion principle) appears undefinable; etc. It is my duty to release my findings on these issues, so that they are reinspected by independent, interested researchers for a possible future resolution of the technical issues. Regrettably, however, these issues appear to generate an understandable emotional behaviour by the average physicist, in which case the profile is basically non-scientific. The reason is that we are here not confuting established trends, but simply studying the possible implications of a conjecture on the nature of the strong hadronic forces (for your amusement I have enclosed a second "vignetta" depicting a true episode-the genuine fainting spells of a relativist that resulted during a "friendly conversation" when he was finally cornered by my technical arguments).

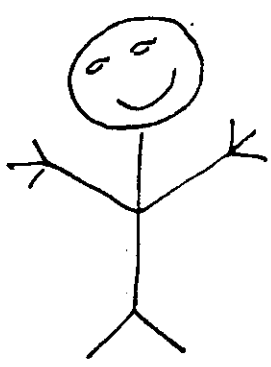
(C) In short, if the old idea that the strong hadronic forces may be (partially)nonderivable from a potential has to be studied to any physical depth, it appears to necessarily demand the attempt at the construction of a new generation of physical theories specifically constructed for the approach and in much the same way as it occurred at the atomic level. This is the aspect which is exiting my collegues and friends beyond my most optimistic expectations and, perhaps, this is the profile for which my enthusiam at such an intriguing physical problem has became contagious. This is also the area of the hopes for covering Lie-admissible formulations (a third vignetta, this time from my forthcoming paper, is enclosed also for your amusement). Of course, this is the most speculative part which will demand time, a long time, for any assessement of any value. What I can safely state as a researcher, as well as a co-editor of the Hadronic Journal, is that this search for possible covering formulations can by now be considered as initiated.

I understand the reservations by my senior collegues on such a delicate undertaking, but I believe that it is only the result of the lack of knowledge of my person and of my study. In any case, I am at their complete disposal for any question. Jointly, I hope that my senior collegues understand the rather severe difficulties I have in being the recipient of the allocated salary via the Department of Mathematics. After all, I have only applied for noting more than the removal of the word "honorary" from my title. Again, please accept the sen timents of my sincere gratitude for your courtesy, consideration and time.

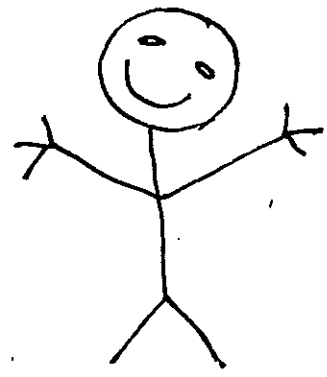


THE TRUNCATION OF LAGRANGE'S AND HAMILTON'S EQUATIONS



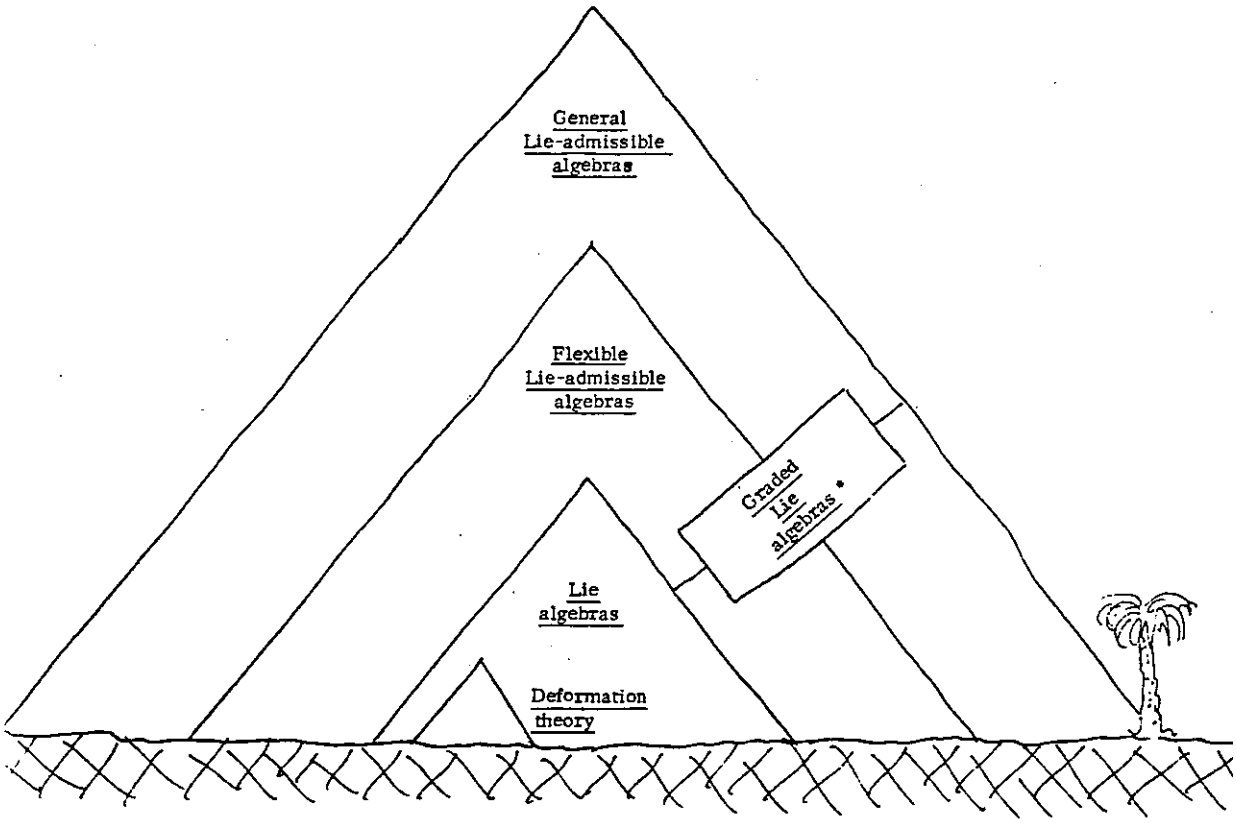


G. GALILEI



A. EINSTEIN

RELATIVISTIC FAINTING SPELLS



*) Secret passage to bigger pyramids

April 27, 1978

Professor S. Coleman
Harvard University

Dear Sidney,

I have no words to express the fact that the manuscript I gave you was truly rudimentary.

I have now implemented numerous technical improvements in virtually all sections, following the help I generously received by quite a few collegues.

I have also provided my best efforts to temp.er the style of presentation, particular in crucial points.

However, please keep into account that this paper has been written during the worst period of my life (without a regular salary since September 1977 with a family of four and with my wife at the graduate school). This is inevitably reflected in the style of presentation.

I do not know whether you will have the time of going through this project. In any case I would like to indicate that it is supposed to be printed on May 1, 1978.

Sincerely

Ruggero

April 29, 1978

Professor M. TINKHAM,
Chairman,
Department of Physics
HARVARD UNIVERSITY

Dear Professor Tinkham,

I have separately put in your mail box a note dated April 26, 1978 elaborating on my current research with copies of some of the letters of comments by collegues. I have taken the liberty of giving copy of this material to each senior collegue. Additional copies are at your disposal.

I would like to report to you an open conversation I recently had with Shlomo Sternberg. In essence, it appears virtually unfeasible that I can receive the salary of the DOE grant via the Department of Mathematics prior to its expiration, owing to the need of searching for mathematicians willing to study my research (an impossible task). In any case, since I am a physicist, I do not think I should even apply for a position at any mathematics department.

In conclusion, a possible negative vote by my senior collegues at your Department implies the inability for me to receive the allocated salary of the DOE grant.

I am sure that, as an administrator, you realize the implications of such a possible occurrence. I trust in your judgment to properly present them to the senior collegues in case needed.

Thank you.

Sincerely

Ruggero Santilli

P.S. I hope you have enjoyed the "vignette" of my note of April 26.

In case needed, I can be reached Monday either in my office (5 3352) or at the printer, 492 5600 (where I will be supervising production of the HADRONIC JOURNAL).

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 5, 1978

Professor M. TINKHAM,
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

I would like to acknowledge your verbal, informal communication of a negative attitude by the senior departmental members toward my appointment as research fellow.

After due consideration, it would be inappropriate for me, being a physicist, to apply for a position at the Department of Mathematics. In any case, since my studies are essentially of conjectural high energy nature (as most of the current high energy research is), a negative decision by the Department of Physics renders simply inconceivable my application at the Department of Mathematics.

The net result is that a negative decision by your senior departmental members on my appointment as research fellow is technically equivalent to the decision that I should not receive the salary allocated by the Department of Energy under contract no. ER-78-S-02-4742 for me.

I am sure you realize the implications of such a rather delicate, possible occurrence.

I am confident that you are aware of my sincere committment toward the pursuit of physical knowledge, as well as of my eagerness and receptive attitude toward any critical review and inspection of my studies.

Therefore, please take into account that I would be simply pleased to submit any paper during the period of the contract to any senior member for inspection and approval prior to any release or submission to outsiders, whether for formal submission for publication or for confidential review. Similarly, I am receptive toward any scientifically productive procedure for the conduction of the studies related to contract DOE-ER-78-S-02-4742 which might be requested by the department.

In consideration of the above I hereby submit my last appeal for a reconsideration of the case, hoping that the wisdom by the senior departmental members will produce a sensible solution.

I look forward to receiving from you a formal communication on the final decision.

Very Truly Yours

Ruggero Maria Samtilli
Honorary research fellow

RMS|cgg
c.c.: Professors S. Coleman,
S. L. Glashow, R. J. Glauber,
A. Jaffe, R. V. Pound and
S. Weinberg.

May 5, 1978

Professor S. GLASHOW,
Harvard University

Dear Shelly,

I believe that a negative decision on my case is substantially against the interest of the department, as well as of the individual senior members. Permit me, respectfully and candidly, to express my viewpoint and propose a solution inspired to the utmost possible moderation.

WHY A NEGATIVE DECISION IS AGAINST THE DEPARTMENTAL INTERESTS.

(1) The DOE grant is now a reality (whether for good or bad reasons). I have no doubt that a negative decision on my case will negatively affect the future relationship between Harvard and the Department of Energy (for this reason I have absteined from communicating to DOE the current difficulties and kept a complete silence until now). I do not know whether you are aware of the pressures on Govenmental Agencies to diversify the topics of research in hadron physics, that is, more explicitly, to begin minor support of quark nonoriented research (to my knowledge, they originate from the highest rank in the Administration and in the Congress). I am sure you realize the difficulties which such a decision would create at the DOE and the consequential implications for Harvard's interests.

(2) The line of study on Lie-admissible algebras is, by now, also a reality. I can assure you that a significant number of physicists and mathematicians are by now conducting active research in this topic (you should not be surprised at some of their names, and keep into account that the names I have disclosed are only part of the complete group). It appears likely that no group of researchers of contrary interest can by now stop this line of study. The current departmental decision literally implies the release of this line of study to outsiders. I am sure you realize the potential implications one to two years from now. Most importantly, Harvard would be simply confronted with articles when they arrive at your library and would be basically outside of the decisional process. With my most sincere respect, permit me to indicate that the number and quality of physicists who do not believe in the quark conjecture (whether colored or not) is considerable. Some of them told me that they have been silent until now because no alternative of any value was on the horizon. I have no word to express my recommendation to you of giving serious consideration to the negative implications for Harvard of the free spinning of these ideas entirely outside of departmental inspection.

(3) The Hadronic Journal is, by now, also a reality, with adequate financing, subscribers all over the world, a regular flow of papers submitted for publication and a very encouraging support from so many collegues. You are aware of the inspiration according to which the journal was born: the presentation of any valuable alternative to the problem of the hadronic structure for the sole pursuit of physical knowledge. The sentiments of my sincere esteem and, if you permit me, consideration in your person, suggest me to recommend to you a careful consideration of this vehicle if you force it completely outside of Harvard's touch.

In conclusion, I sincerely hope that the senior members give full and adequate consideration of the above points prior to reaching a negative decision on my case. They are in Harvard's interest, not my own.

MY PROPOSAL.

I have provided my best efforts (as a researcher by instict) to analyze the difficulties of my case as seen from a departmental viewpoint, even tough I received no formal or informal information. My proposal is therefore inspired by my sincere intent to comply with the departmental rules and general interests. I believe that I have given proof in the past of being a responsible person capable of complying with departmental structures. Also, you can trust that I will follow all my committments ad litteram.

My most serious deficiency, as it appears to me, is that I do not have a senior departmental member as a supervisor. Shlomo is literally burdened by so many duties and responsibilities (as I can testify) that he simple does not have the time at this moment. You know that, since my arrival at Harvard, I have searched for a senior supervisor with my sincere intent of submitting all my scientific production for inspection and approval prior to any release to anybody. But I understand that everybody is quite busy. Lately, I have asked Sidney whether he can act as my supervisor. If he cannot for any reason, I would be very happy whether you can act as supervisor. Almost needless to say, I would be honored to collaborate with any collegue. But wat I believe is most needed is supervision.

In relation to time, please keep into account that I am not prolific (contrary to a different initial impressions). During the year of the grant I intent to write one or maximum two papers. This boils down to hours per one years. My next step is to study a possible relativistic formulation of Lie-admissible algebras. I will need 4-to-5 months just to get oriented.

In essence, I am offering a full departmental supervision and inspection of my studies with the understanding that they should not be released until approved.

In this respect permit me to candidly stress that I offer this strict supervision beginning from the date I receive my first check. My family needs food and shelter, like yours. My lack of regular salary since September 1977 has forced me into a series of decisions and initiatives which I would never have made, if regularly salaried as you are. I verbally told you that the primary reason why I accepted the job of organizing the Hadronic Journal is financial. I also told you that the primary reason why I am in the process to release my manuscripts with the Hadronic Press is because I need the royalties and advance payments. Most of what has happened is strictly motivated by my financial needs. If I were regularly salaried, the Lie-admissible algebras would have remained in my desk for a number of additional years. Further delays will force me into further actions.

In closing, permit me to openly discuss the editorial situation of the Hadronic Journal. You know that we are sincerely committed to scientific values. You also know of our awareness that, to achieve this objective, we need qualified advice by senior members of the scientific community. You also know that senior departmental members received the offer to be editor of the journal. I had numerous possibilities of appointing outstanding physicists for the needed third position of editor. I have abstained from doing so waiting for a more careful consideration by you and your senior collegues.

I would be honored whether you can become the third editor of the Hadronic Journal and, as such, the senior editor (as an incidental note, you know that I have a budget of few thousands dollars for the honorarium, depending from the final number of subscribers).

I understand that you might decide not to officially let your name be printed in the second page as "Editor". In this case, I am also authorized to an informal appointment of a "Silent Editor" (with the same honorarium), in which case you name will not be printed in the second page.

Whatever possibility you select, in case you decide to accept my offer, you can rest assured that I would be happy to submit to you each and every paper prior to formal communication to the authors. Time-wise, we are talking of some 4-to-9 papers (maximum) every two months.

In conclusion, my proposal is inspired by my genuine desire to comply with departmental regulations and general interests. I would of course welcome any scientifically effective alternative solution. My rationale is quite simple: I believe that by doing so the quality of my scientific production and activities would immensely benefit. Jointly, Harvard would avoid a quite risky decision.

Sincerely

Ruggero

c. c.: Professors S. Coleman,
M. Tinkham and S. Weinberg.

Professor S. Coleman
Harvard University

Dear Sidney,

I am sure that, within your own soul, you recognize that the Lie-admissible algebras can indeed write a page of physics, provided that they are properly developed and presented.

I am fully aware that my difficulties in front of the department are also due to the fact that I am alone and, as such, substantially handicapped toward the proper development and presentation of the studies. Regrettably, Shlomo, with his knowledge, could make a real breakthrough. But he is too busy and burdened by some many duties and responsibilities (as I can testify after being his guest for few weeks) that he simply has no time at this moment.

You are the only scientist at Harvard (and, I can safely say, in the Boston area) with the necessary knowledge and vision to understand and techni cally assess the research.

I am therefore asking for your supervision. I would, of course, be delighted and honored to collaborate with you. But if this is incompatible with your current interests, I would be grateful to receive only your occasional supervision, with the strict understanding that I should take a truly minimal amount of your time and that no paper, information, material or otherwise is released to other persons prior to the necessary time for your inspection and approval.

I am making this request in relation to my last proposal to the department related to a solution of the available grant. An additional copy is enclosed. It essentially calls for a supervisor. I believe I have given proof in my life of being a man of honor and you can trust my committments ad litteram.

In practice this means supervising my work for essentially one (or maximum two) papers I can (possibly) write during the period of the contract. You have not seen the final version of my paper with the Hadronic Journal, but the old reference 33 on my forthcoming papers has been reduced to only one, the initial studies of a possible relativistic formulation of Lie-admissible algebras.

In relation to time, I will need 4-5 months only to get oriented at the problem. Thus, during the period of the contract your time can be reduced to the order of hours. Of course, I would be happy di discuss with you alternative topics, if you have any preference (there is so much and so much fascinating work to be done). In relation to the "color-committed" collegues, I am sure that this is a better solution than forcing me angrily loose to fight color and similar conjectures (you know how easy it is to get qualified "anti-color" support).

As an incidental note, I feel obliged to disclose to you on a strictly confidential basis that the copies of letters of support I distributed were only partial. As indicated in my letter to Prof. Tinkham, I have not disclosed the support from "distinguished collegues". You should not be surprised at some of their names.

Hoping to hear from you, I remain, sinceramente tuo

Ruggero

May 9, 1978

Professor R. BOTT
Department of Mathematics
Harvard University

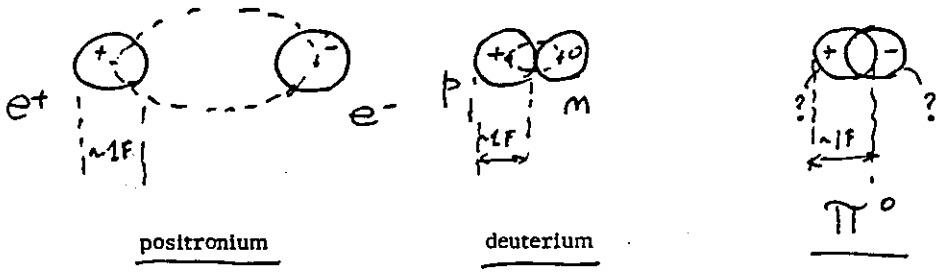
Dear Raoul,

I am in a delicate moment of my academic life. I would appreciate the courtesy of your confidential advice whether I should apply for the position of research fellow (or any other suggested position) at the Department of Mathematics or not. You can rest assured that your consideration and kindness will meet with my utmost discretion.

My situation is rather delicate on a number of technical and academic aspects. Permit me the liberty of presenting an outline as it appears to me and presented in a form as candid as possible. This is only motivated by my desire to provide elements of some possible value for the consideration of the issue.

The conjectural physical nature of my research. I have been involved for some time on the study of the problem of the nature of the forces of the hadronic constituents, the methods for their treatment and the conjectural implications for possible models of structure. In the most rudimentary language possible, the central conceptual argument is as follows. You are aware of the current trends in hadron physics (quarks, color, asymptotic freedom, etc. etc.). They are essentially based on the (for me) abstraction of point-like constituents and the assumption that the strong hadronic forces are derivable from a potential. This is equivelent to say that the hadronic structure can be represented with the conventional structure of a Lagrangian, i.e., $L_{tot} = L_{free} + L_{int}$. This assumption, in turn, implies the direct applicability of conventional analytic formulations (Hamilton's equations, etc.), algebraic formulations (Lie algebras, etc.) and geometrical formulations (symplectic geometry, etc.), as well as conventional quantization procedures in both discrete and continuous cases.

The epistemological argument of my studies is quite simple. Again in rudimentary language, a most intriguing experimental data on hadrons is that their charge volume does not increase with mass (contrary to the corresponding occurrence at the nuclear level) and it is of the same order of magnitude of any other physically established, charged particle ($\sim$1 Fermi). If the hadronic constituents are assumed as physical particles, that is, possessing a finite charge volume, a picture of the hadronic structure substantially different than that of the atomic and nuclear structure emerges. In short, starting from the atomic structure of very large distances as compared to the charge radius of the constituents, we go to the nuclear structure of very close distances and, finally we have a hadronic structure in which, according to this line of thinking, there is a "penetration" of the charge volume of each constituent with that of the others.



positronium deuterium

If such a view of the hadronic structure is plausible, it appears to demand nonlocal forces. This sufficient to indicate the possibility that the complexity of the problem of the hadronic structure might go beyond the most vivid imagination of contemporary physicists.

I am unable to treat, both classically and quantum mechanically, nonlocal forces. I will remain to do so even if I spend the rest of my life on the problem to attempt a rigorous treatment. With full knowledge of my limitations, I have therefore worked on a rudimentary approximation of such a structure which is solely intended as an intermediate step of mainly qualitative nature, prior to possibl future, more adequate treatments.

The idea of this (rather crucial) approximation is again, quite simple. We know from mechanics that local forces not derivable from a potential constitute a good approximation (in a physicist's language) of nonlocal forces. I have therefore concentrated my efforts on the classical and quantum mechanical methods for the treatment of these forces. The emerging systems, at a primitive and rudimentary level, can be conceived as being the local, class C^∞ nonconservative systems of our everyday experience, e.g., the nonconservative (rather than conservative) spinning top with drag torques responsible for the nonconservation of the angular momentum.

After a number of years of isolated labor, I am now in the process of presenting my studies on the treatment of local forces not derivable from a potential in two series of monographs. The first is in print at Springer-Verlag and it is within the context of the Inverse Problem of the CV. The second will be printed by Hadronic Press(when I find the courage to release the manuscripts)and it is within what I have tentatively called the Lie-admissible problem.

I enclose copy of the table of contents of the monographs with Springer-Verlag. I have mailed a copy to Dr. Brooks at Stony Brook who has generously accepted to read the galleys. I also enclose a leaflet on the monograph with the Hadronic Press. Both series of manuscripts have been inspected by numerous collegues, mostly physicists and few mathematicians. I also enclose copy of my articles on the field theoretical extension of the Inverse Problem. Finally, I enclose a copy of the first issue of the Hadronic Journal with a rudimentary presentation of the Lie-admissible problem and some conjectural remarks on possible relativistic implications.

The idea of this dual methodological profile for the treatment of the same systems is again simple.

(1) local, class C^∞ Newtonian systems with forces not derivable from a potential can also be represented with conventional Lagrange's equations (without external terms) under certain integrability conditions. This, however, implies the loss of the conventional structure $L_{tot} = L_{free} + L_{int}$, in favor of an arbitrary functional dependence. The important point is that the knowledge of a Lagrangian or a Hamiltonian renders fully applicable to the broader systems considered conventional analytic, algebraic and geometrical techniques.

(2) The same systems can also be represented in the way originally conceived by Lagrange and Hamilton, that is, by representing the forces not derivable from a potential with external terms in the analytic equations. Apparently, this implies the need of generalizing the conventional formulations by stimulating mathematical problems which, for me, are truly intriguing, as I point out below.

Back to the hadronic structure, if you go to the case of more than two constituents, the structure which emerges is essentially the following, again, in rudimentary language. The state as a whole is conservative and obeys established relativity and quantum mechanical laws, as experimentally established. However, the constituents are in a high degree of nonconservative notion on an individual basis, that is, each constituent exchanges with the other all its physical characteristics (energy, linear momentum, charge, etc.). The intriguing point is that the description of such a structure apparently demands the efforts of generalizing our physical knowledge (essentially based on $L_{tot} = L_{fre} + L_{int}$). To state it explicitly, it appears that this view of the hadronic structure

demands a generalization of available formulations in much the same way as it occurred at the atomic level. For instance, the conventional quantum mechanics, essentially emphasizing the stability of the orbits of the electrons in an atomic structure (for which, after all, was conceived) apparently presents difficulties to describe the motion under consideration because now all the emphasis is in the nonstationarety of the orbits of the constituents. Similarly, conventional relativity ideas appear to exhibit difficulties because, for instance, the equations are not form-invariant under the Galilei transformations by central requirement (to ensure a nonconservative behaviour-I am here referring, again, to the case of the individual constituents and not to the hadron as a whole).

The reaction by collegues (physicists) to this approach to Hadron structure can be divided into two opposite groups with no common grounds. A first group, essentially composed by physicists who do not believe in quarks, color, etc. are strongly in favor for the conduction of this line of study. A second group, essentially composed by physicists academically and financially committed to quarks, is either openly against this line of study (e. g., because of the "style" of presentation of my paper, which, in my opinion, has noting to do with the scientific issue) or is silent.

In my papers I claim no result, while stressing the conjectural nature of my studies. On one point I am firm. I stress the need of subjecting to an experimental verification the validity of the quantum mechanical laws for the hadronic constituents which, in virtually all current papers, are tacitly assumed as valid. I am not trying to convince people that they could be invalid. No. My central objective is to create the awareness in the physics community on the need to verify these laws with experiments prior to claiming that they constitute a scientific truth. Since quark oriented collegues cannot attack me on this angle, this is perhaps the point which renders them most unhappy.

In any case, it appears that a quite intriguing scientific debate is under way. The first issue of the Hadronic Journal, as you can see, already presents three papers on Lie-admissible formulations and truly intriguing papers will be likely published in subsequent issues. If things go as planned, this debate could bring into active role a significant segment of the community. In any case, it cannot be resolved either way by isolated researchers.

The possible mathematical aspects of my studies. Raoul, permit me to candidly confess that, in my view, IT IS EXTREMELY UNLIKE THAT THE PROBLEM OF THE STRUCTURE OF THE HADRONS WILL BE RESOLVED BY PHYSICISTS ALONE WITHOUT THE PARTECIPATION BY QUALIFIED MATHEMATICIANS. My rationale is quite simple. The technical complexities of the problems to be confronted and hopefully solved in due time, simply goes beyond the knowledge and capability of the best groups of best physicists. I have made this appeal to a number of mathematicians and few have positively reacted until now. The best case is that of Prof. H. C. Myung, a mathematician of the University of Northern Iowa and the leading expert on Lie-admissible algebras. He is now fully active and involved in research (he is a member of the Editorial Council of the H. J.). I hope that other mathematicians will undertake active research efforts on some of the many aspects which, to the best of my knowledge, are still open on grounds of pure mathematics. Without this technical contributions by mathematicians, the physicists will not only be unable to districate the issues, but will end up doing noting more than a "grossa confusione".

Unless I am grossly mistaken in my view, it appears that there are truly intriguing, apparently new mathematical problems to be contronted. If you are interested and have the time, please briefly inspect Section 3 of my paper on the Hadronic Journal on the "Lie-admissible Problem". You will see then the apparent need of generalizing Lie's theory into a form of Lie-admissible type, or the need of studying the possible existence of broader geometrical approaches capable of characterizing (in my rudimentary language) the Lie-admissible algebras (I had to discart in my efforts both, the symplectic and the Riemannian geometry).

The role of the Department of Energy. As you know, the DOE grant No. ER-78-S-02-4742.A000 is now arrived, duly executed. The money is sitting in Harvard's bank and I cannot draw the salary allocated by DOE for me because the university regulations prohibit a person with my title, "honorary research fellow", to receive salary.

The DOE is, of course, fully informed of my studies. In turn they had them submitted to several top physicists and (I understand) mathematicians before allocating the money (in these days, no agency gives to money away without due consideration). The net outcome has been that, to the best of my knowledge, DOE appears to be determined to financially support my research provided that I have a qualified university to administer the grant.

Permit me to disclose on a strictly confidential basis that, after this initial grant for two years, DOE appears receptive for a second rather substantial grant. As a final notice, you should also considered that I applied for this grant after two written, sequential, invitations by the DOE. I did not solicit it because I knew I could not apply from Harvard as a principal investigator.

My future research program. After the enclosed first paper in the H. J., I am now writing a second paper of conjectural high energy physics nature (as any theoretical study of this type is today). It is essentially a critical anamnesis of the quark model in light of possible generalization aiming at the stimulation of the awareness of the physics community that, after all, different models could be conveivable.

This paper, which I will release while being still at Lyman, will complete my conjectural papers. After that, I would like to dedicate myself to study and technical papers. Next year I will essentially write two papers with Myung (a mathematician) on Lie-admissible algebras and spend most of the time in studying mathematics. As I indicated to Shlomo, my primary interest for being here next year was to be able to follow a few graduate courses. I am fully aware that my mathematical knowledge is noting but rudimentary.

To summarize, I would appreciate your confidential advice whether it is appropriate for me to apply for any position acceptable by the department of mathematics. My last academic position was that of associate professor of physics. However, any position (of research, nonteaching, nontenured and terminal nature) would be fine for me. I am aware that I am a theoretical physics and, as such, not fully qualified for such an application. Neverthless, I believe that my studies have a possible mathematical significance. Also, the DOE support could be of some value to the Department.

In closing, permit me to stress that, if such a position is granted, the nature of my papers will change, in the sense that I will not write papers of conjectural high energy nature. Instead, I shall provide my best efforts for joint collaborations with mathematicians for technical contributions (this is the reason why I am now hurrying to complete all conjectural papers while being under the name of a physics department).

If you need any further element, please do not hesitate to call me (office 5 3352 and home 969 3465).

Please accept the sentiments of my sincere esteem and gratitude for your courtesy.

Sincerely

Ruggero

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

JEFFERSON PHYSICAL LABORATORY
CAMBRIDGE, MASSACHUSETTS 02138

May 10, 1978

Dr. Ruggero Maria Santilli
Lyman Laboratory of Physics

Dear Dr. Santilli:

I regret to inform you that the Physics Department has declined to change the decision which I had communicated to you earlier and informally. That is, it reconfirmed its view that, since the DOE contract in question has been made with Professor Sternberg in the Mathematics Department, and because your work is in an area of mathematical methods in theoretical physics which is as suitable to that Department as to Physics, therefore your paid appointment should be made through the Mathematics Department. This view is consistent with the fact that no member of the Physics Department felt close enough to your work to be willing to accept the responsibility of serving as Principal Investigator. Moreover, at the time Professor Sternberg was considering whether to agree to serve as Principal Investigator on this contract, our Director of Laboratories, Professor Pound, made it very explicit to him that he expected the Mathematics Department to assume the responsibility for all further appointments related to it if he did agree to serve in that role.

In communicating this decision to you, I feel that I should make it clear that we do not consider removing the "Honorary" from your title to be a trivial technicality. Honorary Research Fellow appointments are made as a courtesy to visitors, with minimum scrutiny, since no financial commitment is involved, and they are for only one year; as a result they carry little institutional responsibility. By contrast, ordinary (i.e., paid) Research Fellow appointments are made only when specifically requested by a senior faculty member who will pay the salary for work related to a contract for which he is fully responsible. Moreover, the supporting documentation for the appointment must be approved by the Dean's office, etc., and it normally must involve comparisons with other candidates turned up in the search mandated by federal Affirmative Action procedures. (This search can be waived only in certain cases where the appointment is limited to a non-renewable term of no more than 12 months.) Harvard University has very restrictive rules governing these matters of appointments and grant solicitation in an attempt to assure appointments of the highest quality and to retain faculty control and responsibility for all research activities. Other universities may be less restrictive, but insofar as they are, their institutional endorsement inevitably carries less weight. In choosing to come to Harvard, you effectively elected to accept the governing standards and principles of the institution.

Sincerely yours,

M. Tinkham
Chairman

MT:ag
cc: S. Sternberg
R. V. Pound

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 15, 1978

Professor M. Tinkham
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

Please let me know at your convenience the date of termination of my honorary appointment (May 31, 1978 or August 31 1978 ?).

Sincerely

Ruggero Maria Santilli

RMS|is

Research Grant Proposal submitted to the
ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION

by

President and Fellows of Harvard College
c/o: Office for Research Contracts
Holyoke Center 458
Cambridge, Massachusetts 02138

entitled

INTEGRABILITY CONDITIONS FOR THE EXISTENCE OF A LAGRANGIAN IN NEWTONIAN MECHANICS AND FIELD THEORY

Principal Investigator

Shlomo Sternberg
Professor of Mathematics
Soc. Sec. No. 090-34-7895
Science Center
One Oxford Street
Cambridge, Massachusetts 02138

Co-Investigator

Ruggero Maria Santilli
Research Fellow
Soc. Sec. No. 032-46-3855
Lyman Laboratory
Harvard University
Cambridge, Massachusetts 02138

Proposed Starting Date: March 1, 1978
Proposed Duration : 15 Months
Amount Requested : $50,000.00

Endorsements

Principal Investigator

Shlomo Sternberg
Professor of Mathematics
(617) 495-2170

Co-Investigator

Ruggero Maria Santilli
Research Fellow
(617) 495-3212 / (617) 969-3465

Departmental Officer

Shlomo Sternberg
Chairman
Department of Mathematics
(617) 495-2170

Institutional Adm. Officer

Mary Marquebreuck
Director
Office for Research Contracts
(617) 495-5501

DEPARTMENT OF PHYSICS

MEMORANDUM

To Ruggero Maria Santilli

DATE May 22, 1978

FROM M. Tinkham

SUBJECT

In reply to your letter of May 15, I can inform you that your honorary appointment runs through June 30, 1978.

July 19, 1978

Dr. M. TINKHAM,
Department of Physics, Harvard University

Dear Dr. Tinkham,

Permit me to express my gratitude for your exhaustive letter of May 10, 1978 communicating the decision by your senior collegues in relation to my affiliation for the DOE grant. I am particularly grateful for elaborating aspects which were basically unknown to me. You can rest assured that I understand and respect this decision in full.

I would like to inform you that I have absteined from even mentioning your consideration of the case to the Department of Energy, as well as to any collegue. You can trust that this silence will be kept in its entirety and that, in case requested to disclose the case by DOE, I will take the liberty of consulting with you to select the most appropriate form of disclosure.

Permit me to comment on my correspondence with you and your senior collegues. You have eventually noticed the use of the conventional academic language in my formal letters with Harvard stationary and the use of a nonacademic, candid language in my personal notes such as this. I understand that you might not be accostumed to nonaccademic languages in accademic matters. Permit me to indicate that, under no circumstance you should identify in such open language my intent of being offensive. If I failed to convey this point, please accept my most sincere apoligies.

In essence, I felt the need of using a candid language because of my high concerns on the grave status of high energy physics, which calls for the attempt of the identification of the current problem in a way as clear as possible. You might inspect in this respect my enclosed letter to Prof. Panofski, Director of the Stanford Linear Accelerator Center.

Also, I was under the erroneous impression that your senior collegues had judged my studies as without physical interest, contrary to an extensive analysis by DOE (no agency gives money these days without extensive consideration) and the support by quite numerous collegues, some of whom outstanding and recipient of the Nobel Laurea. Also, I was under the erroneous impression that Harvard University, after letting me formally file a research grant application with a U.S. Governmental Agency, and after letting this application be formally funded, had decided not to execute the proposal.

But perhaps my state of spirit was the result of a major disappointment I received from Sidney Coleman. In essence, I asked to visit your Department in the hope of receiving criticisms on my delicate studies.On April 4 I visited Sidney indicating that I was in need of qualified advice on an intriguing but truly delicate paper (my second of the enclose formal letter, on the proposal for a generalization of Galilei's relativity). He indicated interest after filing his tax forms. On April 15 I formally submitted the paper for his review via the Hadronic Journal in the form then available, a "rudimentary draft for confidential communications".Most of the critical,technical and linguistic passages of the paper were hand marked for his advice. On April 27 I wrote a note to Sidney in whic I indicated the existence of a revised version incorporating suggestions by numerous collegues and the projected time of printing of the article (May 1). This printing (of the first issue of the Hadronic Journal) was then delayed in the hope that Sidney would kindly help me in the finalization of the pape: On May 5 I discovered that Sidney, while he had kept a complete silence with me, he had been quite generous of criticisms on this paper, as a result of a detailed study he had conducted for departmer use. This was reason for extreme disappointment for me because contrary to centuries of scientific traditions to which I have been educated and contrary to the confidentiality of the formal referee process. My note of May 5 to Shelly was written under the reaction of that moment.

In any case, I want to reassure you that I have no animosity whatsoever with Sidney. Perhaps, he was afraid that I could misuse his help. In this respect permit me to indicate that I have received help for this paper from truly outstanding physicists. I have respected their confidentiality to the point, as you know, of refusing to release their names when requested by some of your collegues during the consideration of my case.

In closing, let me indicate that, being the genuine temperamental Italian you by now know, I am capable of genuine sentiments of respect and gratitude. I have nothing but these sentiments towards you and all your collegues.

Sincerely

Ruggero Maria Santilli

RMS|cgg

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 30, 1978

Professor M. TINKHAM,
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

I complete today my honorary visit at your Department. It is my pleasant duty, as my last act, to express the sentiments of my sincere gratitude to you and all your collegues for the kind hospitality I have received.

For your information, during my visit I have released for printing the following monographs

- Foundations of Theoretical Mechanics, Volume I (with Springer-Verlag); and
- Lie-admissible approach to the hadronic structure, Volumes I and II (with Hadronic Press).

The remaining volumes of these two series will be released at some later time under my new association.

During the same visit I have also released, either for preprint distribution or for printing, the following articles as technical summaries on parts of the above monographs.

- Isotopic breaking of gauge symmetries, preprint HUTP-77|A066;
- On a possible Lie-admissible covering of the Galilei relativity in Newtonian mechanics for nonconservative and Galilei form-noninvariant systems (HADRONIC JOURNAL 1, 223-423 (78
- Need of subjecting to an experimental verification the validity within a hadron of Einstein's special relativity and Pauli's exclusion principle (HADRONIC JOURNAL 1, 574-901 (1978)).

Copies of the latter articles are enclosed for your file. Complimentary copies of my monographs will be mailed to you, in case available. You will recall that I also wrote few additional nontechnica notes. But, as verbally indicated to Steven Weinberg, they were not intended for outside release and simply for internal use.

Permit me to indicate that the above articles are not written in the conventional style for convention topics in conventional journals. Instead, their style of presentation is provocative as a result of a specific and laborious search. In essence, the hope of these articles is that of stimulating the consideration of fundamental issues, according to the traditional priorities of basic research which have been regrettably abandoned in recent times. Also, the style of presentation was selected after consideration of a number of alternatives, in the hope of focusing the current grave situation of basic studies.

As you eventually know, my research grant with the Department of Energy has been executed by Harvard and beginning from tomorrow I will be formally associated with Harvard University Science Center.

Very Truly Yours

Ruggero Maria Santilli
Honorary Research Fellow

RMS|cgg

RUGGERO MARIA SANTILLI
Information for Annual Report

April 14, 1978

A. Summer 1977: Visiting fellow at

- International Centre for Theoretical Physics, Trieste Italy (invited by Prof. P. BUDINI)
- Institut fur Theoretische Physik, Universität Zürich (invited by Prof. A. THELLUNG)
- Instituut voor Theoretische Mechanica, Rijksuniversiteit Gent (invited by Prof. R. MERTENS).

B. Research grant

coinvestigator of a research grant with Professor S. STERNBERG as printipal investigator supported by the U.S. Department of Energy Number ER-78-S-02-4742.A000 and entitled "Integrability conditions for the existence of a Lagrangian in Newtonian Mechanics and field theory". The grant has been approved by the high energy physics division of DOE and recommended to the DOE administration for support. The grant is expected to be executed before May 1, 1978.

C. Lectures outside of Harvard

- "Classical symmetry breakings", talk delivered at the Department of Physics of Northeastern University (invited by Prof. E. J. SALETAN), November 1977;
- "Methodological treatment of nonconservative systems", talk delivered at the Department of Physics, Queens College of the C. U. N. Y. (invited by Prof. K. R. RAFANELLI), October 1977;
- "The Inverse Problem for partial differential equations", informal talk at the International Centre for Theoretical Physics to a group of physicists from Mid-East and Africa; August 1977;
- "The existence theorems of Analytic Mechanics", talk delivered at the Instituut voor theoretisch Mechanica of the Rijksuniversiteit, Gent (invited by Prof. R. MERTENS), July, 1977;
- "The conjecture of a Lie-admissible covering of the Galilei relativity for nonconservative Newtonian systems", talk delivered at the Institut fur Theoretische Physik der Universität Zürich (invited by Prof. A. THELLUNG), July 1977.

D. Lectures at Harvard

- "The Inverse Problem in Newtonian Mechanics and Field Theory", informal seminar course for graduate students in physics and engineering (all from MIT) presented in the fall semester

E. Publications

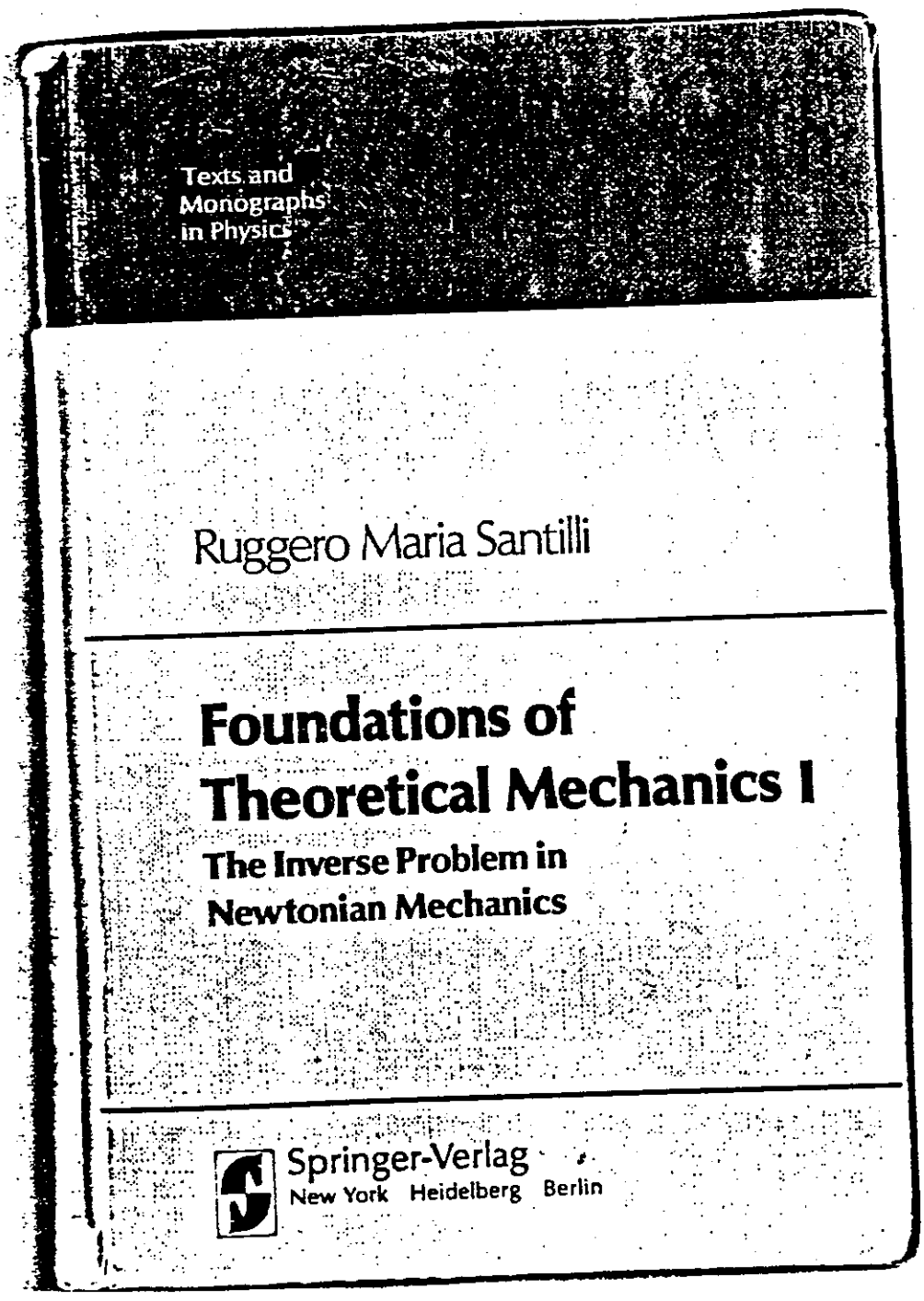
- Foundations of Theoretical Mechanics, Volume I (The Inverse problem in Newtonian Mechanics), currently in press by Springer-Verlag in the series "Textbooks and monographs in physics"
- Foundations of Theoretical Mechanics, Volume II (Generalizations of the Inverse Problem in Newtonian Mechanics), accepted for publication by Springer-Verlag. To be published in 1978|79.
- Lie-admissible approach to the Hadronic Structure, Volume I (Nonapplicability of the Galilei and Einstein Relativities ?) currently in press by Hadronic Press, Nonantum, Ma;
- Lie-admissible approach to the hadronic structure, Volume II (Coverings of the Galilei and Einstein relativities?), currently in press by Hadronic Press, Nonantum, Ma;
- Lie-admissible approach to the hadronic structure, Volume III (Identification of the hadronic constituents with physical particles?), accepted for publication by Hadronic Press. To appear.
- "Isotopic breaking of gauge symmetry" preprint HUTP-77|A066. Submitted for publication.
- "Need of subjecting the validity of Pauli exclusion principle within a hadron to an experimental verification", preprint HUTP-77|A085. Submitted for publication.
- "Need of subjecting the validity of Einstein special relativity within a hadron to an experimental verification" preprint HUTP-77|A086. Submitted for publication.

- "On a possible Lie-admissible covering of the Galilei relativity in Newtonian Mechanics for nonconservative and Galilei form-noninvariant systems", Hadronic Journal 1 (1) (1978), in press.
- Lie-admissible covering of the Galilei relativity (200 pp), Hadronic Press. In press.
- "The conjecture of a Lie-admissible covering of Einstein's special relativity for strong interactions". In preparation.

F. Editorial

Conducted referee work for:
- Physical Review Letters,
- Physical Review D,
- Annals of Physics.

Received the pleasant duty to organize the HADRONIC JOURNAL for the Hadronic Press, Inc. on January 1978. Editorial organization completed by March 30, 1978. The first issue, scheduled for publication on April 1978 is currently in press.


Texts and
Monographs
in Physics
Ruggero Maria Santilli
Foundations of
Theoretical Mechanics I
The Inverse Problem in
Newtonian Mechanics
Springer-Verlag
New York Heidelberg Berlin

Ruggero Maria Santilli
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138
USA

Editors:

Wolf Beiglböck
Institut für Angewandte Mathematik
Universität Heidelberg
Im Neuenheimer Feld 5
D-6900 Heidelberg 1
Federal Republic of Germany

Maurice Goldhaber
Department of Physics
Brookhaven National Laboratory
Associated Universities, Inc.
Upton, NY 11973
USA

Elliott H. Lieb
Department of Physics
Joseph Henry Laboratories
Princeton University
P.O. Box 708
Princeton, NJ 08540
USA

Walter Thirring
Institut für Theoretische Physik
der Universität Wien
Boltzmanngasse 5
A-1090 Wien

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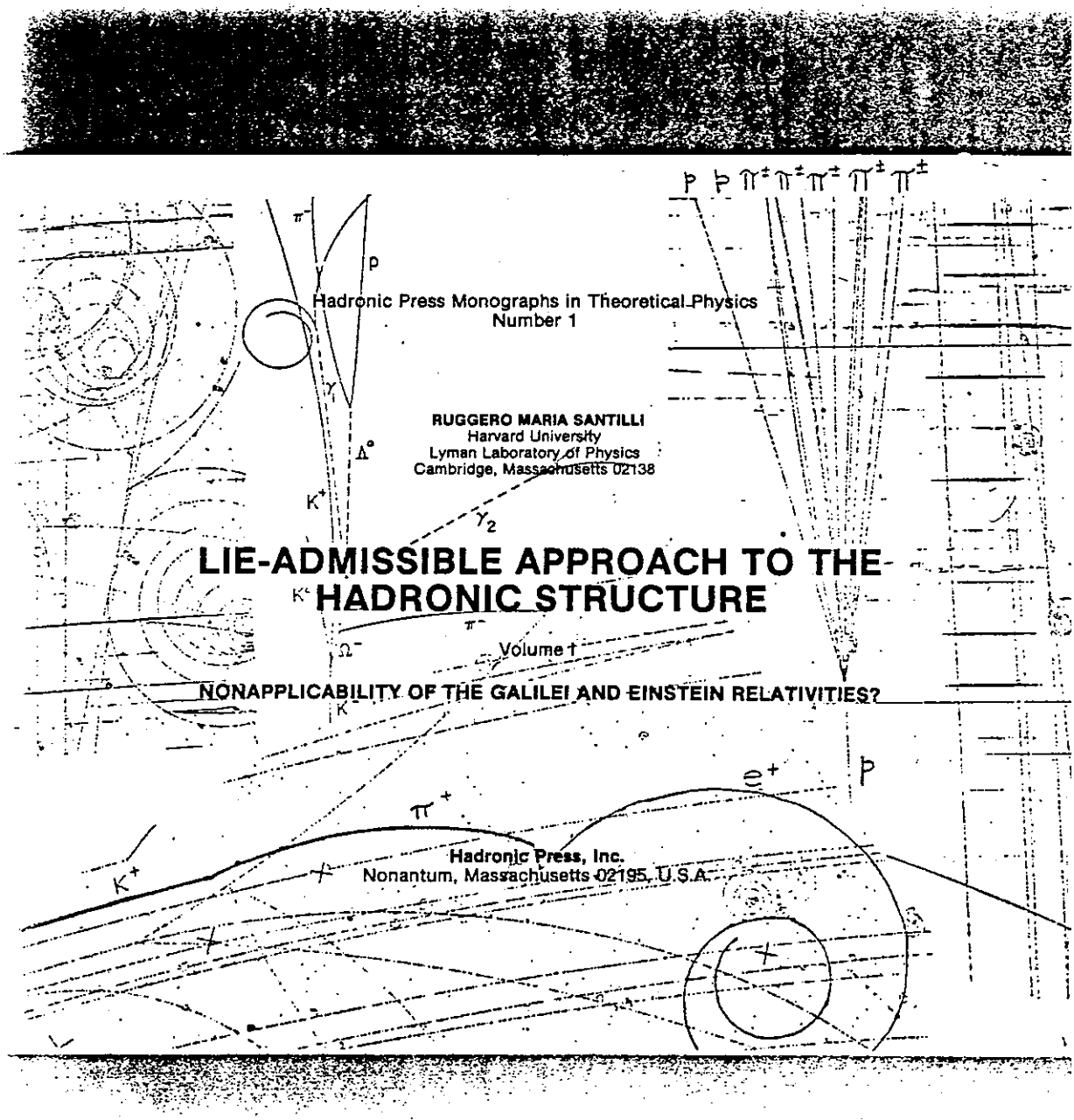
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Hadronic Press Monographs in Theoretical Physics
Number 1

RUGGERO MARIA SANTILLI
Harvard University
Lyman Laboratory of Physics
Cambridge, Massachusetts 02138

LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE

Volume I

NONAPPLICABILITY OF THE GALILEI AND EINSTEIN RELATIVITIES?

Hadronic Press, Inc.
Nonantum, Massachusetts 02195, U.S.A.

HUTP-77/A066

ISOTOPIC BREAKING OF GAUGE SYMMETRY*

Ruggero Maria Santilli
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

ABSTRACT

By using the integrability conditions for the existence of a Lagrangian, a classical gauge symmetry breaking mechanism is introduced whereby: (a) the original gauge Lagrangian is replaced by an equivalent chiral Lagrangian without changing the local variables and conserved currents, (b) the original gauge symmetry is broken by the equivalent Lagrangian and is replaced by a different symmetry which leads to the conservation of the original charge current, and (c) this different symmetry generally results to be a mixture of space-time and internal transformations.

* Research supported in part by the National Science Foundation under Grant No. PHY75-20427.

10/77

In print at Phys. Rev. D

HADRONIC JOURNAL 1, 223-423 (1978)

On a possible Lie-admissible covering of the Galilei relativity in Newtonian Mechanics for nonconservative and Galilei form-noninvariant systems

Ruggero Maria Santilli*
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Received January 16, 1978
Revised version received April 3, 1978
Final version received April 27, 1978

Abstract

In order to study the problem of the relativity laws of nonconservative and Galilei form-noninvariant systems, two complementary methodological frameworks are presented. The first belongs to the so-called Inverse Problem of Classical Mechanics and consists of the conventional analytic, algebraic and geometrical formulations which underlie the integrability conditions for the existence of a Lagrangian or, independently, of a Hamiltonian. These methods emerge as possessing considerable effectiveness in the identification of the mechanism of Galilei relativity breaking in Newtonian Mechanics by forces not derivable from a potential. Nevertheless, they do not exhibit a clear constructive capability for a possible covering relativity. For this reason, the second methodological framework is presented. It belongs to the so-called Lie-Admissible Problem in Classical Mechanics and consists of the covering analytic, algebraic and geometrical formulations which are needed for the equations originally conceived by Lagrange and Hamilton, those with external terms. These formulations are characterized by the Lie-admissible algebras which are known to be genuine algebraic covering of Lie algebras, and which in this paper are identified as possessing (a) a direct applicability in Newtonian Mechanics for the case of forces not derivable from a potential, (b) an analytic origin fully parallel to that of Lie algebras, i.e., via the brackets of the time evolution law, (c) a covering of the conventional canonical formulations as classical realizations, (d) an implementation at a number of levels of Lie's theory, including a fundamental realization as enveloping nonassociative algebras, (e) a generalization of symplectic and contact geometry as geometrical backing and (f) the capability of recovering conventional formulations identically at the limit of null external forces, here interpreted as relativity breaking forces. A covering of the Galilei relativity, called Galilei-admissible relativity, is then conjectured for independent scrutiny by interested researchers. A number of potential implications, particularly for hadron physics, are then briefly considered for future detailed treatment.

*Supported by the U.S. Department of Energy under contract number ER-78-S-02-4742.A000.

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HADRONIC JOURNAL 1, 574-901 (1978)

Need of subjecting to an experimental verification the validity within a hadron of Einstein's special relativity and Pauli's exclusion principle.

Ruggero Maria Santilli*
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Received May 8, 1978
Revised version received June 5, 1978
Final Version received June 19, 1978

Abstract

This paper is a call for theoretical and experimental studies on the problem whether the relativity and quantum mechanical laws which have proved so effective for the atomic as well as the nuclear constituents are truly verified also for the hadronic constituents. For the intent of stimulating these studies, this paper is devoted to the problem whether a violation of the laws considered in the arena considered is conceivable, plausible and quantitatively treatable on grounds of our current knowledge. This problem is studied according to a number of sequential steps.

First, we conduct a critical analysis of the quark models on the hadronic structure to the effect of indicating that, perhaps, their known problematic aspects are only the symptoms of a much more fundamental problem of consistency at the level of the basic laws. By noting that the available unitary models produce a Mendeleev-type classification of hadrons of unequivocal physical effetiveness and of virtually conclusive character, we search for a compatible but fundamentally different model of structure along much of the differentiation between the problem of classification and that of structure which resulted as necessary at the atomic level.

* Supported by the U.S. Department of Energy under contract number ER-78-S-02-4742.A000

We then enter into the study of a conceivable new model of hadronic structure which is capable, on one side, of achieving compatibility with the established models of unitary classification and, on the other side, of resolving the fundamental problematic aspect of available models of structure, the identification of the hadronic constituents with physical particles. According to our priorities, we assume as fundamental the problem of the nature of the forces of the hadronic constituents. The second problem in our priorities is that of the disciplines capable of treating the assumed type of strong hadronic forces. The last problem in our priorities is that of the construction of a structure model of hadrons and of its confrontation with physical reality.

A crucial experimental data of the hadronic phenomenology is that the charge volume of hadrons does not appreciably increase with mass (contrary to the correspondent occurrence at the nuclear level) and it is of the same order of magnitude of that of any other know, massive and charged particle. It then follows that, if the hadronic constituents are massive, charged and physical particles, that is, non-point-like, they are bounded according to a state of penetration of their charge volumes (or wave packets). This yields a realization of the strong hadronic forces as being nonlocal and nonderivable from a potential, that is, a type of force which is beyond our current knowledge at this time for any effective, quantitative treatment. We therefore approximate these forces with local forces nonderivable from a potential. This yields forces which, at the primitive Newtonian level, are the nonconservative forces of the systems of our everyday experience. The fundamental physical character of the assumed strong hadronic forces is therefore that of being nonconservative. These forces essentially constitute the simplest conceivable analytic generalization of the Lorentz force, in the sense that the Lorentz force is linearly dependent on the velocities and derivable from a potential (variationally selfadjoint forces), while our strong hadronic forces are dependent on the velocities in a generally nonlinear way and are non-derivable from a potential (variationally nonselfadjoint forces).

We then enter into the study of the quantization of nonconservative Newtonian forces in general and of strong nonselfadjoint hadronic forces in particular. For this purpose we briefly recall the dual methodologies for the classical treatment of the forces considered, as presented in details by the author in preceding papers and forthcoming monographs, those of the Inverse Problem and of the Lie-Admissible Problem. The paper essentially presents a study for the quantization of these methodologies which results in a proposed dual covering of Schrödinger's and Heisenberg's equations. A central result is that, under the condition that the quantum mechanical algorithms at hand (r, p, H, M, etc.) possess a direct physical significance, the brackets of the time evolution law must violate the Lie algebra identities as the fundamental condition for mathematical and physical consistency for the case of nonselfadjoint forces. Instead, the brackets considered can characterize a covering Lie-admissible algebra, in precisely the same way as it occurred at the classical level. Intriguingly, there is the emergence also of the Jordan algebras, which therefore acquire an apparent fundamental methodological role for the quantum mechanical treatment of nonconservative forces, perhaps equal to that of Lie algebras. Indeed, the brackets of the proposed covering of Heisenberg's equations result to be, jointly, Lie-admissible and Jordan-admissible. The epistemological lines for a possible covering of conventional quantum mechanics, here called hadronic mechanics, are presented. It is then pointed out in details, either via the generalized algebraic structure of the theory or via direct analysis of the dynamical behaviour, that the inflexible laws of quantum mechanics (here called atomic mechanics) for the treatment of selfadjoint forces are fundamentally inapplicable to the broader physical context constituted by strong nonselfadjoint hadronic forces. Instead, the familiar quantum mechanical laws appear to be replaced by covering laws capable of identically recovering the former at the limit of null forces non-derivable from a potential.

As a necessary complement to the above dynamical analysis of the problem, we then study the relativity laws which are applicable in nonconservative quantum mechanics (the hadronic mechanics in our terminology). This objective is achieved by quantizing the Lie-admissible covering of Galilei's relativity for nonconservative Newtonian mechanics proposed by the author in a recent paper. This results in the proposal of a quantum mechanical covering relativity for the hadronic constituents, under the assumed broader forces, which is Lie-admissible in algebraic character and, as such, capable of identically recovering the conventional relativity of atomic mechanics at the limit of null nonselfadjoint forces. It is pointed out that established relativities (Galilei's, Einstein's special and Einstein's general relativity for the interior problem) are inapplicable to the considered more general nature of the strong interactions. In particular, the proposed covering relativity results to be of non-Lie, non-conservative, non-inertial, nonlinear, non-geodesic, non-symplectic and non-Riemannian character to technically characterize physical systems which are non-derivable from a variational principle. In conclusion, our studies on the dynamical profile of the problem of quantization of nonselfadjoint forces result to be in full agreement with the corresponding studies on the relativity profile.

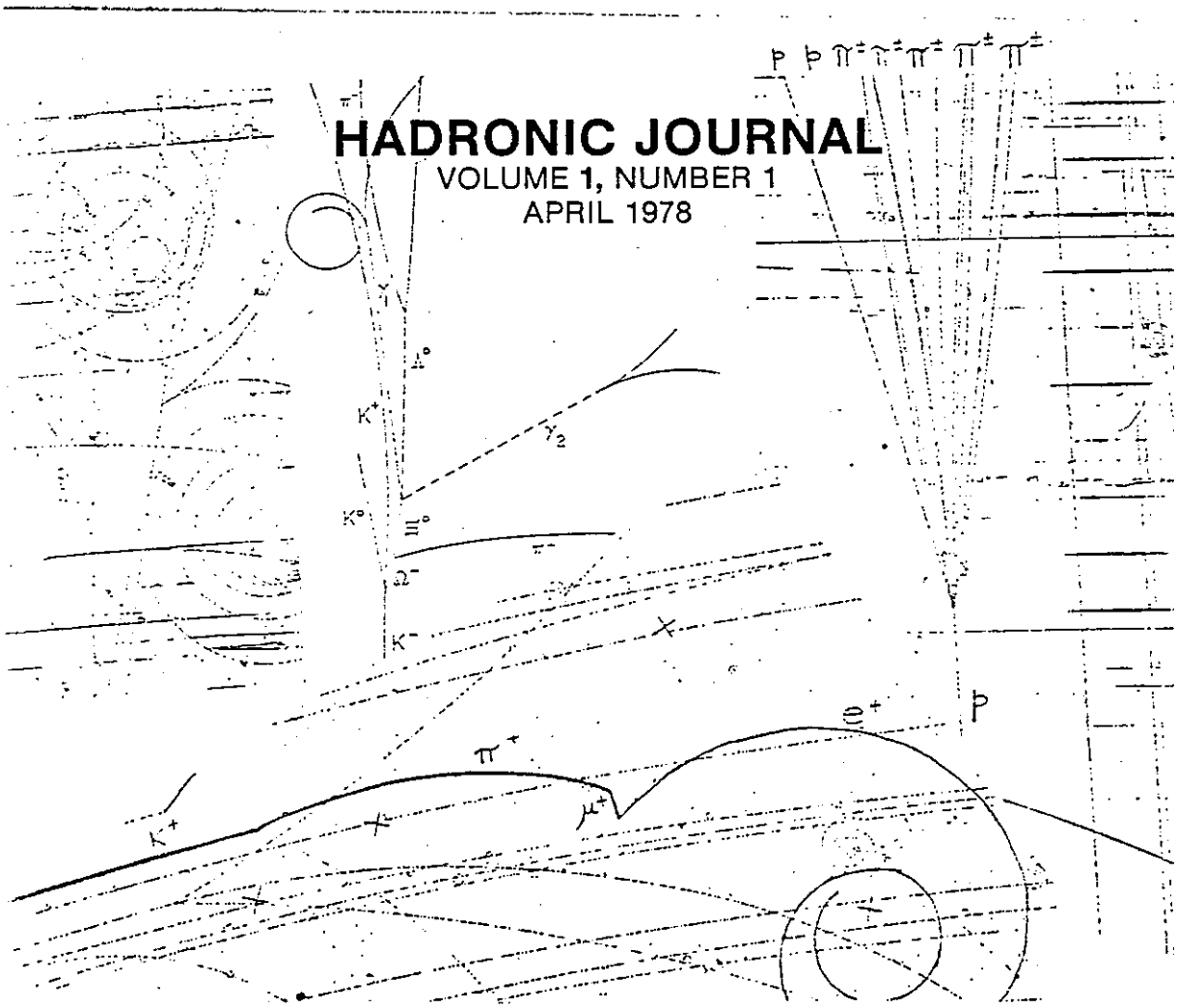
We then enter into the study of an apparent dichotomy of physical laws for the hadronic phenomenology: the unequivocal validity of established laws for the behaviour of a hadron as a whole under ut most electromagnetic interactions and the conceivable applicability of covering laws for the hadronic constituents. This problem is studied via the use of nonintegrable, classical and quantum mechanical subsidiary constraints. In figurative terms, the established laws for the total physical quantities of a hadron are imposed as subsidiary constraints to the covering laws for the individual constituents. The emerging overdetermined systems of differential equations result to be consistent (that is, admitting a physically meaningful solution) under the proper selection of nonselfadjoint forces. It is this property which, in the final analysis, has allowed the presentation of the analysis of this paper. According to this approach, established laws are valid by construction for a hadron as a whole, and possible departures are admitted only for the hadronic constituents. In particular, the violation of these laws at the level of the constituents (only) emerges as a necessary condition for the existence of more general structure forces in order to attempt a real departure of the hadronic from the atomic structure. For instance, the imposition of Galilei's relativity at the structure level would imply conservative strong forces. We then argue that, under these conditions, the atomic and hadronic structures are dynamically equivalent.

As the last step in our priority, we finally consider the problem of the construction of a new structure model of hadrons based on this dichotomy of physical laws, and its confrontation with experimental data. The study essentially indicates that the identification of the constituents of unstable hadrons with suitably selected massive particles produced in their spontaneous decays, while prohibited by the conventional relativity and quantum mechanical laws of strict Lie algebraic character, becomes admissible under the proposed covering, relativity and quantum mechanical laws of joint Lie-admissible and Jordan-admissible algebraic character. The case of mesons is considered in detail and it is indicated that the model is capable of producing a quantitative representation of all the intrinsic characteristics of the particles, while offers some genuine hope for a quantitative interpretation of the decay modes and related fractions. Thus, the proposed new model of hadronic structure appears to resolve the fundamental problematic aspect of the quark models, the identification of the constituents with physical particles, while reaching full compatibility with the established unitary models of classification.

As we all know, our current theoretical knowledge can be interpreted as characterized by suitable implementations of the experimentally established knowledge for the electromagnetic interactions which preserve the underlying basic laws. Pending the verification by interested researchers, our analysis essentially indicates that such knowledge can be considered as applicable, provided that the forces (or couplings) are local and derivable from a potential, that is, the system is represented in its entirety via the simple Lagrangian structure $L_{tot} = L_{free} + L_{int}$. If the strong interactions are assumed as dynamically nonequivalent to the electromagnetic interactions and their forces are realized in a form analytically nonequivalent to the Lorentz force, they demand the abandonment of the virtual entirety of our current theoretical knowledge (such as: Galilei's and Einstein's relativities; Heisenberg's equations and Pauli's exclusion principle; scattering amplitude and Feynman diagrams; canonical field quantization and spin-statistics theorem; etc.). Instead, under the conditions indicated, the courageous construction of covering disciplines must be undertaken for the strong interactions in general and for the hadronic structure in particular, in exactly the same way as it occurred for the electromagnetic interactions in general and for the atomic structure in particular.

The paper concludes with remarks concerning the future orientation of experimental high energy physics which is needed to provide means for a physically effective selection among an ever increasing number of hadronic models. It is argued that, until the experimental efforts are essentially restricted to the identification of new particles, the problem of the hadronic structure will likely remain fundamentally unresolved, because the knowledge of new particles adds informations which are certainly useful for the classification of hadrons, but not necessarily for the structure. It is submitted that, jointly with the continuation of these valuable experiments, the fundamental problem of the validity or invalidity of established relativity and quantum mechanical laws for the hadronic constituents is confronted. In the final analysis, if the laws considered will eventually result to be valid in the arena considered, the quark models are likely to emerge as the only conceivable models at this time. On the contrary, if the laws considered will eventually emerge as being violated in the arena considered, the concept of quark as the constituent of hadrons is likely to be ruled out in a final form.

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HADRONIC JOURNAL
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HADRONIC JOURNAL

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The particle events printed in half tone in the front page of the Journal are derived from 80" bubble chamber photographs kindly provided by BROOKHAVEN NATIONAL LABORATORY

PART IB:

ACADEMIC
YEAR
1978–
1979

HARVARD UNIVERSITY

OFFICE OF THE SECRETARY
17 QUINCY STREET

CAMBRIDGE, MASSACHUSETTS
June 17, 1978

SIR,

I beg to inform you on behalf of the University and the Dean of the Faculty of Arts and Sciences that you are appointed

Research Associate in Mathematics

to serve from March 1, 1978 through May 31, 1979 subject to the Third Statute of the University (*overleaf*).

Your obedient servant,

Secretary to the University

Ruggero Maria Santilli

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 16, 1978

Professor S. STERNBERG,
Chairman
Department of Mathematics
Harvard University

Dear Professor Sternberg,

I am here respectfully applying for any nonteaching, nontenured, terminal position at your Department of your selection from March 1, 1978 until May 31, 1979 which allows me to conduct research as per DOE grant no. ER-78-S-02-4742.A000.

I would also appreciate whether my salary of $ 30,000 for the period March 1, 1978 until May 31, 1979 and payable in monthly installments of $ 2,000 allocated by the DOE grant is formalized and charged to the Harvard code number 33|966|7131-2.

Copies of the formal notification of execution by DOE of the indicated grant and of the Harvard ORC office for the code number should be in your file. Additional copies are at your disposal.

As verbally indicated, you can count on my committment that any possible paper which might originate from my research will reach departmental standards prior to any outside release. Also, I would be happy to comply with any specific guidelline for the conduction of the research as per the DOE grant you might consider appropriate. Almost needless to say, I would be honored to collaborate with any departmental member.

Finally, I would like to stress that my application is only for a terminal appointment. Under no circumstances, either direct or indirect, should Harvard University be responsible for my salary after May 31, 1979 or for my relocation.

Yours, sincerely

Ruggero Maria Santilli
Honorary research fellos

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 2, 1978

Professor SHLOMO STERNBERG
Chairman
Department of Mathematics
Harvard University

Dear Professor Sternberg,

I have just received copy of the execution by Harvard University of the DOE grant no. ER-78-S-02-4742.A000. Permit me to express the sentiments of my sincere appreciation for the possibility of conducting research under this grant at the Science Center of Harvard University.

I would like to take this opportunity to confirm that this is a nonteaching, nontenured and terminal research appointment which, as such, cannot be counted either directly or indirectly, for tenure considerations. Also, permit me to confirm that under no circumstances, either direct or indirect, should Harvard University be responsible for my salary after termination of the appointment on May 31, 1978 or for my relocation. Finally, I expect to have no formal affiliation with Harvard University after the aforementioned date.

I have already initiated the search for a job at another campus and I hope to complete it sometimes by late 1978 or the early part of 1979. Permit me to indicate that, even in case I am unable to find such a job, I do not intend to apply for financial support at Harvard University under any form, at the expiration of the current DOE grant.

Hoping that the research activity of this DOE grant can be scientifically productive, I remain

Very Truly Yours

Ruggero Maria Santilli
Honorary Research Fellow

Professor S. STERNBERG
Tel-Aviv University
Department of Mathematics
RAMAT-AVIV, TEL-AVIV, ISRAEL

July 10, 1978

Dear Shlomo,

I would appreciate your help for the following rather unusual occurrence.

By separate air mail parcel, I have mailed to you a record on the music by the late John BOROS. After listening to this music, if you so desire, I would appreciate whether you can donate this record to some public collection of records in Israel of your choice (that at Tel-Aviv University, if any, would be fine).

John was my truly best and intimate friend. He was Assistant Professor and composer of modern music at the Dept of Music of Brandeis. I was close to him during his laborious composition of this music and I considered him truly promising. Regrettably, as you eventually recall, he died with his wife Emy in a tragic car accident in Waltham during a police chase some time ago. I have provided my best possible help for a fund raising effort organized by his collegues at Brandeis to have John's music performed by professionals and recorded. We have succeeded in this effort by raising all the necessary money and the record has been entirely payed. Therefore, there is no need of additional donations.

I believe that it would be nice if John's music is present in the Country of his Ancestors.

Here everything is quite and peaceful during this summer period. I shall soon air mail you the second issue of the journal. If there is anything I can do in your absence, please let me know.

I wish you a happy stay in Tel-Aviv (and I miss your presence in the department).

Sincerely

Ruggero

CONFIDENTIAL - July 23, 1978

Dear Shlomo,

I have mailed on July 22 (Saturday) a complimentary copy of the Hadronic Journal for your amusement. The issue contains my second article which, in essence, is a direct and open attack at the jugular vein of the quark games: the basic laws. A number of experiments are proposed and others will appear in the subsequent issues of the journal by independent authors. It appears that, in case these experiments result to be negative, the quark conjecture will likely be ruled out in a final form.

I also enclose for your amusement copy of a candid, passionate appeal to Panofsky (director of SLAC) for the conduction of the proposed experiments in due time. As a background information, you should know that experiments on the verification of the validity (or invalidity) of Einstein's special relativity for the hadronic constituents have already been proposed years ago. However, since the mere considerati of these experiments is a profound disturbance to quark people (let me say it, because of money-academi aspects related to research grants), these experiments have been completely ignored.

Owing to this situation, and after due consideration, I decided to use the open, clear and candid language in my letter to Panofsky you can see, as a necessary prerequisite, jointly with its formal submission to officers of Governmental Agencies, to prevent the repetition of the traditional procedure to avoid unwanted lines of study: complete ignorance. Thus, the language of this letter (as well as the provocative language of my papers) is not the result of an emotional state, but instead of a meditated decision. Of course, this could render my guilt even greater. I am therefore left with only one alternative: hope in your benevolent understanding if I misjudged some of these steps.

Please note the letter head and my biographical notes of the letter to Panofsky. As you can see, I have carefully avoided any disclosure whatsoever of my association with the Dept. of Math. and with you. You can rest assured that this procedure will be followed in the future ad litteram.

As I indicated earlier to you, this second paper in the Journal is my last of conjectural, speculative, high energy physics nature. I have no words to express my gratitude to you for the possibility of spending one year at the Science Center. Indeed, I intend to study mathematics as much as possible. Beginning from September, I would like to quietly and silently listen to all your graduate courses and lectures, as well as to others at the Dept. of Math. It is a unique opportunity for me to improve my grossly deficient knowledge of mathematics and I do not intend to miss it.

In case you are interested, my next research objectives are the following. First of all, I am not contemplating to write papers for quite some time. Secondly, I would like to give all my necessary or useful assistance to experimentalists interested in the study or actual conduction of the experiments I have proposed (the most critical one, in my view, is the verification of Pauli's principle for strong interactions restarting at the nuclear level). But this will take a minimal amount of time and it is not expected to result in papers.

My long term theoretical objectives are the following. First, I am interested in studying the possible existence of a covering of Einstein's special relativity in classical field theories with nonlinear derivative couplings (or essentially nonselfadjoint in my language), i.e.,

$$\left\{\left[\left(\Box + \omega^2_{(k)}\right)\phi_k - f\left(\phi; \frac{\partial\phi}{\partial x}\right)\right]_{SA} - F\left(x; \phi; \frac{\partial\phi}{\partial x}\right)\right\}_{NSA} = 0 \qquad (1)$$

The central objective is to see whether it is possible to construct a covering of the conventional Lorentz covariance law

$$\phi'(x') = S(\Lambda)\phi(x) \qquad (2)$$

with an understanding that this law unequivocally applies for essentially selfadjoint field equations, that is, systems with ut most linear derivative couplings (this includes the models of constructive field theories, and of unified gauge theories of weak and electromagnetic interactions ONLY, the inclusion of the strong interactions being highly questionable and a mere opinion, as you can see from my paper, particularly Section 5.20).I am expecting that, if a covering covariance law exists for Eqs. (1), it will be nonlinear. I am also expecting a possible primary constructive role of Lie-admissible algebras, on the basis of their crucial role (to the best of my understanding at this time) for the explicit construction of the strictly nonmanifest transformations for the form-invariance in Newtonian mechanics of the same class of systems (nonconservative, nonlinear in the velocity and explicitly time dependent).

If you are interested, my truly long range theoretical objective is to attempt the construction of a covering of Einstein's general theory of gravitation for the interior problem only (I am a beliver of Einstein's general theory for the exterior problem), along the lines outlined in pages 873-875 of the HJ.The idea is to abandon the use of mass terms in the equations for the interior problem; to confront instead the structure problem at least at a rudimentary classical level; to allow for the existence of more general structuresof the strong interactions (essentially nonselfadjoint strong hadronic forces, in my language which, as such, appear to be incompatible with Einstein's equations for the interior problem only on a number of counts); and finally and most importantly, to construct the gravitational field for the exterior problem, according to exactly the experimentally verified Einstein's approach with the sole use of the structure fields. In this way, the gravitational field is not superimposed as an independent entity to other fields. Instead, it emerges as a direct consequence of the structure fields (electromagnetic and strong) of matter. In different terms, the idea is to attempt the very removal of the problem of a unified theory which, in any case, has escaped so many studies. I have conducted a preliminary study of this approach a number of years ago (Ann. Phys. 83, 108 (1974)) and it appears truly promising and, to me, quite intriguing. Again, the experimentally established Einstein's theory for the Riemannian characterization of the exterior gravitational problem remains in full effect by construction. However, at this moment I see no way of preserving the use of the Riemannian geometry for the interior problem, when the structure of matter is included as the central aspect and essentially nonselfadjoint strong hadronic forces are admitted. Equivalently, the approach is based on the traditional speculation of basic aspects, that via criticism of the experimentally unverified validity of the Riemannian geometry for the interior problem, besides its physical character of providing a first, crude approximation of physical reality for the interior gravitational problem of massive objective (I have seen no experiment conducted in the interior of a star....).

You can easily see that, for even an initial, orientational conduction of these studies, I need a number of years of study of Geometry. I would be truly grateful whether, during next year, you can guide me for an effective study of this discipline. It would take only seconds of your time on an occasional basis to indicate valuable references for me to digest.

Almost needless to say, in case you are personally interested in this delicate attempt to initiate a direct study of conceivable coverings of Einstein's ideas, I would be simply honored to work for joint papers. Also, I would be happy to work for joint projecu with you in any other topic in which I can produce a meaningful contribution.

In closing, you might be interested in few other aspects. The Hadronic Journal has attained financial selfsufficiency owing to the very positive outcome of subscriptions (still growing on a dayly basis). For this objective I have put my previous corporate experience at work in its entirety (I have been Chairman of the Board of a U.S. corporation from 1970 until 1974). In essence, as a condition for my acceptance of the job to organize the Journal I have asked and obtained a post in the board of directors of the publisher, which I obtained. Then, I have asked and obtained the strict implementation of extremely conservative business practices.

In particular, by formal board policy, the printed has no accounts payable at all (a true rarity in U.S. business enterprises). All bills are payed cash on delivery. Most importantly, all production is prepayed and no production is order for stock, a part for few samples. Since the cost of printing the Journal in its current form (mainly offset and few typeset pages) is minimal and the individual runs and reruns of each issue can be for small amounts (150 to 200 copies), the company can effectively and proficiently operate under such conservative practices.

I am happy to report that we have already achieved the smallest period of time between submission and distribution of the trade. Our average now runs around 45 days (again, between arrival of the papers and distribution, that is, mailing, and therefore including refereeing, typesetting, printing and mailing). Notice that this is for papers without regards to length, as you know, and without publication charges, which are also rather unique in the journal trade.

My sole concern is for the quality of the papers. I see no way of achieving the desired quality via only papers in the current hadron physics (which is not a science, like Mathematics, in my view, but the mere expression of opinions by individual physicists). The need of clean papers by mathematicians is therefore a must.

I would appreciate whether you could kindly indicate the HADRONIC JOURNAL to some friend in Israel or abroad. They might be interested because of the rapidity of production, the lack of restrictions on length and the lack of publication charges (reprints can be produced at a charge to the publisher). This, of course, includes also possibly interested friends in Russia or anywhere with potential difficulties, say, of publication charges. The topic, as you know, is any aspect of math and physics.

Almost needless to say, as it was for the first issue, any additional paper of yours you might decide to publish with the Journal, will have utmost priority and will be the first of the issue.

Incidentally, the Journal is now regularly reviewed by Math. Review and Curr. Math. Publ. and this should help mathematicians, jointly with the availability of the Journal in the general libraries.

I have made 750 reprints of my second article in the second issue which are now being mailed. I have made this at my own expenses (an invoice is enclosed). I can afford it because Carla has a summer job and (finally!) I have a regular salary. I have also instructed Donna to let me know any eccess expenditure on our grant on my behalf in excess of the agreed $ 300, because I intend to repay it immediately either personally or via small funds I have from the Journal.

Finally, I enclose copy of my formal thanks to Tinkham and all Lyman collegues for the hospitality they have provided for me during 1977/78.

Sinceramente Tuo

Ruggero

P.S. A good news! just received. It appears that experimentalists are interested in considering my proposed experiments. Even though this is for a long term possible outcome, FINALLY it is the first time that physicists acknowledge the need of subjecting to direct experiments the basic physical laws for strong interactions. It was time indeed! I shall keep you informed of relevant events.

Prof. D. KAZHDAN

July 24, 1978

Dear David,

I enclose for your amusement a complimentary copy of the second issue of the HADRONIC JOURNAL. It contains my second article which, in essence, is an open, direct attach at the jugular vein of the quark games: the basic laws. A number of experiments are proposed and others will appear in subsequent issues by independent authprs. There are indications that, in case these experiments are negative, the quark conjecture could be ruled out in a final form.

I also enclose for your amusement copy of a candid, passionate appear to PANOFSKY (director of SLAC) for the implementation of the proposed experiments. As a background information you should know that experiments on the verification of the validity or invalidity of Einstein's special relativity for the hadronic constituents have been proposed years ago. However, since the mere consideration of these experiments is disturbing to quark people (primarily for money-academic and grant related issues), these proposals have been completely ignored until now. The open, candid and clear language of my letter was therefore a necessary prerequisite to avoid (at least I hope) the repetition of the traditional procedure to bypass unwanted lines of studies: complete ignorance.

It is only with regret that I must acknowledge that Mathematics is a Science. Hadron physics, instead, is an opinion. At least at this time.

I have no words to express my gratitude for the possibility of spending one year at the Science Center. Indeed, I intend to study as much mathematics as possible. Beginning from September, I would like to follow, quitely and silently, graduate course in mathematics. It is a unique opportunity for me to improve my grossly deficient knowledge of mathematics and I do not intend to miss it.

I also enclose copy of my formal thanks to M. Tinkham for the hospitality they have provided for my visit, which expired on June 30, 1978.

Have a happy summer with your familiy.

Sincerely,

P.S. Please notice in the enclosed letter heads and curriculum that, as promised to Shlomo, I indicate my association only with the Science Center and exclude any release whatsoever of my association with the Dept. of Math. as well as with Shlomo. You can rest assured that this procedure will be followed ad litteram.

Prof. R. Bott

July 24, 1978

Dear Raoul,

I enclose for your amusement a complimentary copy of the second issue of the HADRONIC JOURNAL. It contains my second article which is, in essence, a direct, open attack at the jugular vein of the quark games: the basic laws. A number of experiments are proposed and others will appear in subsequent issues by independent authors. There are indications that, in case these experiments will result to be negative, the quark conjecture may be ruled out in a final form.

I also enclose for your amusement copy of a candid, passionate appeal to Panofsky (director of SLAC) for the implementation of the proposed experiments. As a background information, you should know that a number of authors have proposed (years ago) the experimental verification of Einstein's relativity for the hadronic constituents. However, since the mere consideration of such fundamental experiment is disturbing to quark people (primarily for money-academic and grant related aspects), these proposals have been completely ignored until now. The open, candid and clear language of my letter to Panosfky, jointly with its formal release to officers of Governmental Agencies, was a necessary prerequisite to avoid the repetition of the traditional tool (at least I hope) for bypassing unwanted lines of studies: complete ignorance.

Dear Raoul, you see, Mathematics is a Science. Hadron Physics is, instead, an opinion. This is why, as anticipated in my former note to you, the second paper in the Hadronic Journal is my last of conjectural high energy physics nature.

I have no words to express my gratitude for the possibility of spending one year at the Science Center. Indeed, I intend to study as much mathematics as possible. Beginning from September, I would like to follow quitly and silently as many graduate courses in mathematics as possible. It is a unique opportunity to improve my grossly insufficient knowledge of mathematics and I do not intend to miss it.

I enclose copy of the front page and acknowedgments of my first monograph with Springer. Robert Brooks did a simply excellent review job (jointly with a number of other collegues). I have managed to let him have via Springer a check for $ 100 (and probably more at some later time).

Finally, I enclose copy of my formal thanks to M. Tinkham for the hospitality I received at Lyman, which ended on June 30, 1978.

Have a happy summer.

Sincerely

Ruggero

P.S. Please note in the enclosed letter head as well as biographical notes, that I indicate my association only with the Science Center, as promised to Shlomo. No disclosure whatsoever with the Dept. of Math. and with Shlomo is released. You can rest assured that I will follow this procedure ad litteram.

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGIERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

December 27, 1978

Professor DEREK C. BOK, President,
Harvard University, Massachusetts Hall 1.

STRICTLY CONFIDENTIAL

Dear Professor Bok,

I feel ethically and scientifically obliged to bring to your personal attention certain delicate events at the Lyman Laboratory of Physics. According to information available to me, it appears that there exists a realistic risk whereby the Lyman Laboratory of Harvard University might be directly involved in a potential public action for [redacted] by malcontent U.S. physicists.

This letter is motivated by my sincere desire to provide you and Dean LEAHY with all the necessary background information. I would like, of course, leave to you the decision whether a preventive action to keep this risk under control is appropriate at this time or not.

The candid language I have selected for this letter is solely intended to avoid possible misrepresentations, in the sole benefit of clarity. Also, the topic is such to stir up great emotions in the scientific community, as you will see. I would like to beg for your benevolence if I have failed to control my own emotions, or if I have failed to properly interpret the information.

THE SCIENTIFIC SECTOR UNDER CONSIDERATION. The sector of basic research in theoretical and experimental physics under consideration is that of the strongly interacting (subnuclear) particles, called hadrons (e.g., proton, neutron, pion, and a multitude of others). Predictably, the understanding of this subnuclear layer of the physical reality turned out to be substantially more complex than that for the broader nuclear and atomic layers. Among the variety of problems, most notable are the problem of the classification of hadrons into families (the equivalent of the Mendeleev classification of atoms) and the complementary but different problem of the structure of each element of a given family (the equivalent of the Bohr or the Thomas-Fermi model of structure of atoms).

You are aware that this sector of research is nowadays conducted at the cost of billions of dollars per year. Therefore, you should keep into account a considerable financial profile at each and every level under analysis.

NONTECHNICAL OUTLINE OF THE CURRENT STATE OF THE ART. As you know, physics is a discipline with an absolute standard of values: the physical reality. Until theoretical ideas have not been proved via incontrovertible experiments, they constitute conjectures, hypotheses, or beliefs, but not manifestations of the physical veritas. In line with this standard, I can confidently state that the problem of the classification of hadrons of Mendeleev type is nowadays accomplished as the result of brilliant contributions by M. GELL-MANN and G. ZWEIGH OF 1964, as well as numerous others. In line with the same standard, I can with equal confidence state that the problem of the structure of hadrons is, at this moment, fundamentally open. We simply have a pletora of models

each of which is controversial, conjectural and without final experimental backing.

The majority of research has been devoted until now to the study of the conjecture that the so-called quarks are the constituents of hadrons. These efforts are essentially based on the hope that one single model (the so-called SU(3) model) can resolve both, the problem of classification and that of structure, contrary to the corresponding differentiation of these aspects which resulted as necessary at the atomic level.

As you are eventually aware, despite a laborious search and investments of the order of billions, it has been impossible to physically establish the quarks as the actual constituents of hadrons. This has induced quark-believers to attempt the construction of a so-called model of confinement whereby the constituents of hadrons (contrary to all other occurrences in the microscopic world) cannot be produced as free. Despite additional huge investiments in money and human resources, an effective model of confinement which can be accepted by the scientific community at large has not been achieved as of now.

To complicate the problematic aspects, certain technical needs have forced quark-believers to multiply the number of different, unidentified quarks. The net result is that the (unitary) models of hadron structure along the quark conjecture are based on a complex topology of assumptions, each of which is of fundamental physical character, but experimentally unestablished.

I would like to add, for your amusement, that quarks are sometimes called the "Yeti particles", or that the sea of studies along the quark conjecture has been compared to the historical episode of the ether, or that the strong dominance of the scientific scene by the (rather powerful) quark-believers has been compared to the control of science by priests during the times of GALILEO GALILEI. In any case, the very inventor of quarks, M. GELL-MANN, has not yet stated whether he believes that quarks exists or not.

THE INDICATIONS FOR A DANGEROUS LEVEL OF MALCONTENT IN THE PHYSICS COMMUNITY.

To the best of my knowledge and reconstruction, there has been a continuous evolution of the reaction by the scientific community to the financial investements for quarks which, starting from a scientifically effective debate, has evolved up to an apparent dangerous level of malcontent due to a number of social factors.

In essence, the period 1964-1970 (again, to my simplistic and perhaps erroneous understanding) was that of the great hopes for the experimental detection of quarks. Jointly, this was the period in which numerous outstanding scientistis had expressed serious doubt as far as the validity of the quark conjecture is concerned. But they were silenced by vigorous and equally powerful quark-believers.

The period 1970-1975 has seen a marked increase of the concern of valuable physicists and the first timid expression of reservation as far as the amount of money invested in quark conjectes is concerned, as well as the lack of a well balanced condition and conduction of studies in the sector. Subsequent events have moved much faster. In essence, the substantial financial restrictions on the community of basic research have implied the direct consequence that quark-nonbelievers could not benevolently tolerate huge sums of money invested in the quark conjecture, while they were without research support at all, or even unemployed with families to support.

According to information available to me, this level of malcontent has now reached such a proportion to justify the consideration by administrators such as you and Dean LEAHY. The current situation (again, to my perhaps erroneous understanding and reconstruction) can be divided into a number of aspects.

There is first a group of moderate (please read here: tenured,employed,salaried) physicists who are performing a vital moderating function and attempting to implement an orderly adjustment of situations. Then there is a second group of highly malcontent (please read here: without the dream of tenure, or unemployed, etc.) physicists who have lost hopes for an orderly improvement of the dispersal of funds in basic research without grave gestures.

Apparently, the first group has expressed the concern directly and independently to President CARTER. The rationale of this group is the following. The United States is a (beautiful but) technologically oriented Country with a rather cloudy long term future. This future VITALLY depends on our capability to implement NOW a well balanced condition and conduction of basic studies. In particular, as it was the case for the achievement of the structure of the atom and the nucleus, the achievement of the final solution of the problem of the hadronic (subnuclear) structure is expected to play a crucial, if not a vital role for survival, owing to a knowledge of primary relevance for energy related issues. It is therefore essential that any monopoly of funds in this sector is cathegorically avoided and that, instead, ALL conceivable approaches to hadron structure are studied, and subjected to a comparative confrontation with physical veritas, in the true pursuit of human knowledge. On financial grounds, this first group of concerned physicists essentially recommend that studies along quark conjectures are indeed funded, but that jointly, studies along fundamentally different approaches begin to receive support. The proportion of diversification of funds in the sector in subsequent years should then be a clear reflection of a clear interpretation of experimental data. If quarks are experimentally established, they should receive 100 % of the funds If experiments disprove the possible existence of quarks, all funds along these conjectures should be truncated.

I personally share this position in its entirety. Besides ethical aspects, the future of my children is at stake here.

The action of the second group of highly malcontent physicists is, in my perhaps erroneous, personal view, potentially explosive and, as such, deserving the utmost possible consideration. Oddly, this situation has been literally created by the quark-beliebers, owing to an often presumptuous attitude from their status and the methodic realization of their personal beliefs via the use of scientific power (rather than veritas). In essence, a number of physicists is unemployed. This is partially due to the unavailability of tenure and still partially to the rejection of proposals (particularly by the National Science Foundations and apparently) of non-quark-inspiration. What has rendered this situation intolerable to these physicists (again to my understanding and reconstruction) is that in certain instances these technical applications for support have been rejected by quark-believers WITH A FLATLY OFFENSIVE LANGUAGE. I have seen personally some of these reports and, although I am not an attorney, these referee reports are, in my view, an expression of shear irresponsibility by tenured physicists currently under the "quark-money-tree".

As a result of these last events, I have been informed of specific accusations of monopoly of funds by studies along quark conjectures, as well as of scientific corruption, that is, the use of scientific power to prevent the allocation of funds to any study on hadrons other than quark-oriented, as well as to prevent the realization of experiments that might disprove the validity of the quark conjecture.All this allegedly occurring at the academic and not the agency level.

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I feel also obliged to report the (actually uncontrollable) rumor that the submission of a documented petition for a Senate Hearing on alleged [redacted] has been avoided by the partecipation of moderate, concerned physicists (at least temporarily).

MY EDITORIAL ACTIVITY. I have recently organized a new journal in theoretical physics, called the HADRONIC JOURNAL. A number of informative leaflets are enclosed. The undisclosed function of this journal is that of attempting a moderating action in this delicate moment of the research sector, jointly with the conduction of a promotional action for all valuable studies on the hadronic structure, whether quark-oriented or not, because this problem is fundamentally open at this time and (in GALILEI's word) "the authority of a thousand" is not sufficient to establish a scientific truth.

After the first year of operation, with a successful financing and completion of the Volume 1 (for over 1,600 pages) I am happy to report to you that the HADRONIC JOURNAL is already considered in the scientific community as one of best evidences of the LACK of existence of a monopoly by quarks, at least on editorial grounds (the more insidious financial profile is, of course, out of my reach). Indeed, virtually all issues the journal publish papers along quark conjectures jointly with papers along fundamentally different approaches to hadrons, in the genuine pursuit of human knowledge, as well as in substantial disrespect of any financial or academic interests of any specific group of researchers.

My position as Editor in Chief of this "scientifically liberal" journal, as well as the fact that I am the recipient of one of the first research grants of non-quark-inspiration (see below) has put me in a rather peculiar situation, which somewhat indicates the reasons for my being a recipient of the indicated delicate information.

In turn, this has allowed me to perform my intermediary action to the best of my judgment and possibility. For instance, in July 1978 I wrote a letter to Professors PANOFSKY, WILSON and VINEYARD, Directors of the SLAC, FERMILAB and BROOKHAVEN Laboratories, respectively, denouncing the current status of condition and conduction of research and expressing my personal concern. A copy of this letter is at your disposal (also for your amusement). In essence this letter presents itself as a vigorous call for a moment of reflection. In actuality, it was used to calm down excessive malcontent. Even though the treatment of the financial profile was carefully avoided, and the aspects were mainly technical, the ultimate motivation of this letter was the malcontent I am reporting to you here.

HARVARD'S ROLE IN HADRON PHYSICS. S. COLEMAN, S. GLASHOW and S. WEINBERG of the Lyman laboratory of Physics are three of the utmost leading exponents of the quark conjectures. Their scientific status and credibility is quite high, and they have an active involvement in virtually all sectors of hadron physics, from research- editorial to funding functions. In addition, there is a quite valuable experimental group (but I am not an experimentalist and I would like to abstein from commenting on this profile). As a result, the Lyman Laboratory of Physics is considered as one of the leading centers of studies along quark lines on a world wide basis.

These occurrences should be solely reason of pride under normal circumstances. Regrettably, they should also be seen as reason for concern, particularly from an administrative profile. This is due to the total absence of any hadron study whatsoever at Lyman Laboratory other than quark oriented. The net effect is that the Lyman Laboratory has been depicted to me by outsiders as a typical

representative of the monopolistic condition and conduction of research for hadrons, as a clear hint for a potential direct implication of Harvard in possible public actions for alleged scientific corruption.
I urge you and Dean LEAHY to conduct an independent verification of these personal remarks. Again, I present them in good faith, but they can be erroneous.

THE EPISODE OF MY VISIT AT LYMAN. I joined the Lyman Laboratory on September 1, 1977 with a one-year appointment as "honorary research fellow". On December 1977 I was authorized to file a research grant application to the DEPARTMENT OF ENERGY (DOE) jointly with Professor S. STERNBERG of Harvard's Department of Mathematics, with Lyman Laboratory being my affiliation of the application. On March 1978 the application was funded and the contract was sent by DOE to Harvard for signature.

I discovered at that time that I could not draw a salary at Harvard under my grant because of the term "honorary" in my title. I therefore submitted a formal application to Lyman Laboratory for (A) the removal of the term "honorary" from my title, so that I could at least draw a salary until the termination of my appointment on June 30, 1978, and (B) the extension of such a "research associate" position for a non-tenured, non-teaching, terminal, additional year , with all expenses supported by my grant.

After an elaborate and rather long departmental consideration (rather oddly restricted to only the senior members, despite its research character) my applications for points (A) and (B) was REJECTED with a final vote of the senior members in early June 1978.

This rejection is, per sé, innocuous. It is the way in which my case was handled and the subsequent events, that are reason for concern.

In essence, with the assistance of the Lyman Laboratory I could have moved easily this grant to another Department at Harvard or, in case impossible, to another university. This assistance was clearly crucial because my application had been filed and funded with my affiliation with the Lyman Laboratory.

Instead, S. COLEMAN, S. GLASHOW and S. WEINBERG made it a point in indicating to a number of collegues that my application had been rejected because the research under my grant has "no physical value". This directly created a number of predictable technical and nontechnical problems, including self-evident legal implications. The return of the grant contract to the DOE, unsigned, was avoided thanks to the invaluable partecipation by Professor STERNBERG and Dean LEAHY. I received a one-year research appointment at the Department of Mathematics, although I qualify myself as a member of the "Science Center", as you can see from my letterhead. This is due to the fact that I am not a mathematician and I do not intend to become a mathematician. I am a theoretical physicist involved in conjectural studies on hadrons.

The episode was aggravated by a number of side elements. For instance, in the occasion of my first and most delicate paper with the DOE grant, I consuted S. COLEMAN expressing to him my need for qualified advice. He expressed interest to help, but after the IRS returns. On April 15 I formally submitted this paper to S. COLEMAN (as ed.itor of the HADRONIC JOURNAL) for his confidential review. I subsequently kept him informed via written notes of the progress of this paper. Finally, I was later told that, while S. COLEMAN had kept a total silence with me, he had been very generous of criticisms on this paper during the numerous meetings on my case. Quite frankly, I find this behaviour in strict violation of centuries of scientific tradition, as

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well as in violation of the confidentiality of the formal referee process. The judgment of the ethical aspect is here left to you.

Similarly, I did attempt to express the potential implications of this negative attitude to the senior members, but my action was misinterpreted as "threats". I was left with no other conceivable route than that of truncating any contact with the senior members at Lyman. Copy of the correspondence is at your disposal.

To understand the meaning of the judgment of "no physical value" by S. COLEMAN, S. GLASHOW and S. WEINBERG you should know that one of the primary objectives of my research under the DOE support is that of conducting a critical analysis of quark models in the traditional spirit of unsolved physical problems. Actually, my grant appears to represent (to my understanding) an indication of the desire by the DOE Officers (Drs. HILDEBRAND, PEASLEE and WALLENMAYER,HEP, as well as Dr. KANE, Director of the Division of Physics and Dr.DEUTCH, Director of the Division of Energy) to implement the well balanced condition and conduction of research at Harvard University on hadron structure, as indicated earlier (or, in my language, to avoid a monopolistic restriction of research on hadrons at Harvard along only quark conjectures).

As a result, the judgment of "no physical value" by the indicated quark committed collegues was referred to studies of strict non-quark inspiration. Also, and more insidiously, it was referred as a form of opposition (or it can easily be interpreted as such) to the DOE against the initiation at Harvard of a nonmonopolistic conduction of studies on hadrons.

In defense of my studies permit me to indicate that the central (novel) methods I have identified for my own treatment of the strong interactions have been accepted for publication(since November 1977, and, thus, much before the consideration of my case at Lyman) in one of the most prestigious series of monographs in physics, that by SPRINGER-VERLAG of Heidelberg, West Germany, under the title: FOUNDATIONS OF THEORETICAL MECHANICS, Volumes I and II. The first volume under my DOE grant has already been distributed and I enclose exerpts for your consideration. The second volume is scheduled for the second year of my grant. My papers, also under this DOE grant, have apparently stirred up a considerable form of interest and I have,for your inspection, a rather voluminous file of letters of support for my studies by outstanding physicists from all over the world.A few abstracts of my papers are enclosed.

In case you are interested to inquire for an independent assessement of the physical value of my studies, I recommend you to consult [redacted] Professors [redacted] and [redacted], as well as all the distinguished members of the EDITORIAL COUNCIL of the HADRONIC JOURNAL. A list of additional outstanding scientists of proved ethical standard is at your disposal.

HARVARD'S CURRENT RISK. The rejection of my application at Lyman for the conduction of my DOE grant, the breaking of the confidentiality for the referee process by S. COLEMAN, the judgment of "no physical value" of my research by S. COLEMAN, S. GLASHOW and S. WEINBERG are all INNOCUOUS occurrences for the simple reason that, owing to the current delicate moment of our community, I have kept a complete silence and I have cathegorically absteined from even alluding their existence to outsiders.

The risk for a potential implication of the Lyman Laboratory in a possible public action by malcontent physicists originates from a much more substantial, delicate, and clear occurrence.

The true, ultimate objective of my DOE grant, as well as of my editorial involvement with the HADRONIC JOURNAL, is the following. The most distressing aspect of the current status of hadron physics IS NOT, in my view, the pletora of models and our inability to select the right one. It is due, instead, to the complete lack of experimental verification of the basic physical laws in an incontrovertible form. Specifically, all studies along quark conjectures are based on the supplementary, much more fundamental conjecture that Einstein's special relativity applies under strong interactions in general and within a hadron in particular. The point is that this relativity has been experimentally established only for the electromagnetic interactions and its validity under the strong is a mere BELIEF as of now.

My primary objective is therefore that of studying and promoting the experimental verification of basic physical laws, as currently used in hadron physics. Only after these laws have been verified, the question of quark or non-quark models may acquire a scientific value beyond that of exercises of curiosity.

I should here indicate that I am not the first to stress the need of verifying basic, conventional, physical laws for the strong interactions. Actually, the Founders of Quantum Mechanics suggested it already for nuclear physics. Lately there have been initial proposals of specific tests and even initial attempts of experiments, although with insufficient energies. As of now, it appears that we have indeed achieved the technology to test Einstein's special relativity at extremely small distances, within a subnuclear particle, and I enclose an article by Professor D.Y. KIM from Cambridge-England I have (proudly) published in the HADRONIC JOURNAL. It is understood that our current knowledge for these crucial experiments is at the very beginning and a long labor of trial and error, presentation of ideas and their constructively critical examination by independent researcher, is expected.

Permit me to confess that the aspect of the current status of our community of basic studies which distresses me most is a caparbious, emotional, at times violent OPPOSITION BY QUARK-BELIEVERS ON THE CONDUCTION OF THESE EXPERIMENTS OF SUCH FUNDAMENTAL CHARACTER FOR HUMAN KNOWLEDGE. To understand this aspect you should first know that quark models are fundamentally incapable of resolving the problem of the validity or invalidity of basic laws within a hadron (owing to the complex topology of assumptions in which they are based, none of which experimentally established). Secondly, you should know that these quark conjectures are vitally dependent on Einstein's special relativity. To be specific on this truly crucial point, and as elaborated in my papers in all necessary details, if Einstein special relativity is invalid in the interior of a hadron (as it is already known to be in the interior of a star .....) the quark conjectures are inconsistent in their very formulation , let alone their treatment and intended physical content.

In conclusion, if the experiments studied under my DOE grants are actually conducted in due time and establish the invalidity of Einstein special relativity within a hadron, THE COMPLETE SCIENTIFIC PRODUCTION ON STRONG INTERACTIONS CONDUCTED AT LYMAN LABORATORY FOR HADRON STRUCTURE IS INVALID.

This is the legacy of Einstein's ideas in the first centennial of his birth. These ideas are truly fundamental in contemporary theoretical physics, but they have been proved only for the electromagnetic interactions (for the special case). I can understand the personal emotions by S. COLEMAN, S. GLASHOW and S. WEINBERG at the even remote possibility that their numerous papers on hadron structure could ALL be invalid.But this is a direct consequence of their assumption of Einstein's ideas in an area in which they have not been established experimentally, without a critical analysis of the profile. A possible invalidity of quark-oriented studies is than nothing

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but a fact of scientific life.

I am now, finally, in a position to express to you in good faith the true risk that Harvard University is currently exposed to, as it appears to me.

In essence, it appears that a number of physicists from the U.S.A. and from abroad have contacted the senior faculty members of the Lyman Laboratory in relation to my papers and my studies on hadrons to the effect of inquiring their view on the feasibility of the indicated experiments. This is quite predictable because all my papers in the subject bear the name of "Lyman Laboratory". It is only with extreme concern that I have to report the indication that these senior collegues have dismissed the studies and experiments under consideration with the routine statement of "no physical value".

I am sure you are are aware of the fact that your senior members at Lyman have many outside admirers, but an equally numerous number of enemies.... Some of the latters are quite friendly on the surface. As a result, the statement of "no physical value" made by your quark committed faculty at Lyman, on fundamental experiments which could invalidate the quark conjecture, apparently has been a too easy an opening for nontrivial attack by outsiders.

In conclusion, until my episode, my studies and Lyman's opposition have remained within our Yard, I saw no reason for concern. I have now felt obliged to inform you because of the release to potentially dangerous outsiders of potentially dangerous statements by the senior members of Lyman Laboratory.

MY RECOMMENDATION. The problem of the experimental verification of the validity or invalidity for the strong interactions of Einstein's special relativity (as well as other quantum mechanical laws) is of such fundamental character for human knowledge to go beyond the personal interests of individual researchers, whether those by S. COLEMAN. S. GLASHOW and S. WEINBERG or mine. I am sure you are by now aware of my extreme determination to pursuit this scientific objective at whatever personal price or humiliation.

I beg you for your help in avoiding, whether possible, unnecessary aggravations in the conduction of this function, as well as unnecessary, risky, exposure of Harvard in a rather delicate moment of our community of basic studies.

In particular , I would like to respectfully submit the following options.

(1) To identify sensible but effective means of recommendation to S. COLEMAN, S. GLASHOW and S. WEINBERG to use their invaluable knowledge and scientific status in favor of these fundamental tests, possibly, in a openly stated form.As of this moment, the totality of papers by these physicists assume in a TACIT FORM the validity for the strong interactions of the basic physical laws experimentally established for the electromagnetic interactions only, without any mention of the need of their experimental study. If, for any reason, this constructive attitude is not realizable,

(2) To identify sensible but effective means of recommendation to S. COLEMAN, S.GLASHOW and S. WEINBERG to use extreme scientific caution when consulted by outsiders on these fundamental experimental tests (or, better, be silent). This is clearly essential to avoid unnecessary risks of accusation by outsiders of scientific corruption. And, finally,

(3) To implement at Harvard University a well balanced condition and conduction

of research in hadron structure, in such a way that not only quark conjectures, but also all valuable conjectures are comparatively studied, in the genuine pursuit of fundamental human knowledge. This is not only essential to avoid accusations of monopolistic restrictions of research to only quark oriented studies, but also to continue the fulfilment of a by know historical, primary contribution by Harvard to human knowledge.

In this latter respect, permit me to frankly express my personal opinion that the implementation of this well balanced condition of research is impossible at this moment at Lyman Laboratory, owing to the personality of the theoretical physicists currently in control of the sector of research, as well as their personal research interests. Neverthless, I do see certainly possible the implementation of these conditions of research at Harvard, via the joint use of Lyman Laboratory, as well as other divisions, or via the study of other alternatives (the formation of a Center for Hadron Physics as a subnuclear complement of the existing Center for Astrophysics ??)

In closing this letter, permit me to clarify a few points. First of all, permit me to reassure you that I have no animosity whatsoever toward S. COLEMAN, S. GLASHOW and S. WEINBERG. As a matter of fact, I have only sincere gratitude for their hospitality, as I have formally stated in my monographs with SPRINGER-VERLAG as well as all my papers (see the enclosures). I simply understand their emotions at the mere thought that Einstein might be violated within a hadron .....

Secondly, permit me to stress that I have dedicated my life to basic studies and, as such, I am committed to their orderly conduction. As a result, you can rest assured that I am committed to continue my intermediary or preventive action to avoid even the risk of unnecessary crises.

Thirdly, you have my formal committment that, until I am a member of Harvard University, I shall provide my utmost loyalty to this campus. I am therefore at your disposal for any assistance you might need.

In relation to my future, you might be interested to know that I do not intend to apply for a renewal of my research appointment at the Department of Mathematics for the conduction of the second year of my DOE grant. This is due to a number of reasons, including my utmost consideration and respect for the ethical and scientific status of Professor S. STERNBERG, as well as to avoid even the risk of a repetition of the indirect humiliations he had to tolerate from the Lyman collegues.

Neverthless, I shall attempt to identify a non-tenured, non-teaching, terminal, one-year position at some other department, completely supported by my grant. In principle, the experimental high energy group at Jefferson is the ideal candidate. Neverthless, this group has operated in the past in a sort of symbiotic condition with quark-oriented studies. Therefore, I do not know whether they are interested in being even indirectly exposed to the study of experiments of truly fundamental physical character. Owing to this situation, I intend to avoid any formal contact with the experimental high energy group and abstein from applying for the position indicated (of course, in case I am invited to join, I would be simply honored).

After due consideration of other alternatives, I would like to submit an application for the position indicated to Professor G.B.FIELD at the Center for Astrophysics. Jointly, I intend to consult with Professor MARTIN at the Division of Applied Sciences to inquire about the advisability of my submitting the application indicated. Hoping not to be of any inconvenience, I shall send you copy of the related correspondence.

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If none of these possibilities materializes, my formal association with Harvard will terminate, by contract, on June 30, 1979. I only hope that S. COLEMAN, S. GLASHOW and S. WEINBERG, as well as the other senior members of Lyman Laboratory, WILL NOT INTERFERE with these contacts with their, by now, routine statement of "no physical value".

In closing, permit me to confess my deep sadness, after numerous years of sincere dedication to basic studies at a substantial personal and family sacrifice. I first came to realize that, being an independent theoretical physicist, the mere idea of applying for a regular academic job in the U.S.A. is sadly laughable. These jobs simply do not exist, at least not at my level (nontenured associate professor). They have to be created. But then, they are created at the highest possible human price: your brain. Now I have come to realize that, even with full financial support by a U.S. Governmental Agency, it is extremely difficult to resolve the academic entanglements created by preexisting, vested, academic and financial interests of the type reported in this letter. WHAT WILL BE NEXT ? Where is the creativity of this Country leading to, without the intervention of academic administrators with a genuine vision ? Are we already engulfed in a pattern of "scientific suicide" as far as the genuine pursuit of fundamental human knowledge is concerned ? Will the historians of, say, the 29th century be in a position to express the easy judgment that the (now feared) collapse of the U.S. economy was due to the failure by the academic administrators to implement in time an effective conduction of basic studies ?

In relation to the latter question, permit me the liberty of expressing a vigorous form of support for the Governmental administrators. I can personally testify of their genuine committment and mature scientific vision for the needs of this Country. In my humble view, the current shadows exist ONLY at the academic level. In any case, it is for me inconceivable that they have to be faced with academic entanglements of the type reported in this letter during the actuation of their functions.

I am confident that you will treat this letter and its content with utmost confidentiality. I would be particularly grateful whether its existence can be kept confidential as much as possible. On my part, I have even avoided secretarial partecipation by typing it on my own (as I do, after all, for all my papers and manuscripts). You can trust my utmost discretion.

Please do not feel obliged to acknowledge this letter. I am confident in your wisdom to differentiate its good part from the bag, to put the former to good use and benevolently ignore the latter. Whatever your decision will be, you have my gratitude for your consideration and my sincere esteem.

V E R I

T A S

Ruggero Maria Santilli

c.c. Dean RICHARD G. LEAHY, Faculty of Arts and Sciences
University Hall 20

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

January 11, 1979

Professor DEREK C. BOK, President,
Harvard University, Massachusetts Hall 1,

STRICTLY CONFIDENTIAL

Dear Professor Bok,

Dr. DAVID C. PEASLEE of the Department of Energy, Division of High Energy Physics (tel. 301 - 353 3624) has informed me in a phone conversation of January 10, 1979 of the existence of considerable difficulties in his search for a possible accomodation for the second year of my grant No. ER-78-S-02-4742.A000.

These difficulties were predicted in my report to you of December 27, 1979. Almost needless to say, no member of the Department of Energy has been informed of this report nor of this letter.

After due consideration of the situation, I would like to humbly recommend you the study of the formation of an independent division for the conduction of studies such as mine under the suggested title of

"Center for Hadron Physics"

Owing to preexisting, vested interests in other divisions, this appears to be the most effective way to implement at Harvard the well balanced conductic of studies on hadron structure I indicated to you in my report of Dec. 27.

I enclose for your consideration a "rudimentary draft" of this project. As you can see, I recommend that the position of Director for this possible Center be assumed by a professional administrator (and not by a physicist)

I am here formally asking your authorization to proceed for a feasibility study for the organization of this Center. In case you are in a position to grant such an authorization, I would need the indication of the administrator for me to contact, possibly, with the understanding that the same administrator might become the first Director of the Center (initially on a part-time basis).

I have no word to express to you the human and scientific value for the formation, at this time, of such a Center. I also am firmly convinced that such a Center would be a scientific and financial success for Harvard, because I am fully confident of the implementation of the "mature synthesis between genuine scientific vision and sound administrative practice" I recommended in my draft.

Looking forward to hearing from you, I remain

Very Truly Yours

Ruggero Maria Santilli

C.C.: Dean R. C. LEAHY, Univ. Hall 20

Rudimentary draft for the possible organization of the

C E N T E R F O R H A D R O N P H Y S I C S

by RUGGERO MARIA SANTILLI, Science Center Room 435, ext. 5 3352

FUNCTION. The Center should conduct a well balanced theoretical and experimental study on the problem of the strongly interacting elementary particles (hadrons), with particular reference to the problem of the experimental verification of the fundamental physical laws currently used in this sector of research (e.g., Einstein's special relativity, Pauli's exclusion principle and the spin-statistics theorem). This objective should be pursued by coordinating the efforts by leading mathematicians, theoretical and experimental physicists and engineers.

ORGANIZATION. The Center should be constituted by a Director, a Board of Advisors and the staff members under the position of Research Assistant, Associate and full professors. No member of the Center, including the Director, should be tenured and all members should be on a contractual, research, terminal basis. The Director should be appointed by the President or any administrative body suggested by the President. It is strongly recommended that the position of Director should not be taken by a physicist, and be taken instead by a professional administrator. The Research Professors of the Center should be appointed by the Director, following the recommendation by the Board of Advisors. This latter Board should be composed by senior scientists of proved scientific vision by different Institutions.

FUNDING. Rather than costing money, the Center should bring money to the University. This objective can be realistically attempted via a sound administration, and also in view of the energy related potential of the primary function of the Center which is expected to grow in the near future. All activities should be funded via research grants of governmental or corporate nature. Applications should be filed jointly by the Center and by individual scientists. It is recommended that any scientist, from all over the world, should have the possibility of filing an application via the Center, of course, upon review and approval by the Board of Advisors, and that possibility be advertised in The Physics Today as well as in other scientific news media to fulfill an increasing need of such a function in the scientific community. Initial funding of the Center may be provided by the grant of R.M.SANTILLI from the Department of Energy, under the assumption that such a relocation is possible.

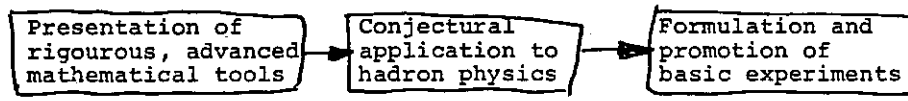
AFFILIATION. It is suggested that the Center be affiliated to the Department of Mathematics, in such an administrative form to create the minimum possible burden, if any, to such Department. This affiliation appears recommendable on the basis of the primary function of advanced mathematical knowledge for the activities of the Center. All members of the Department of Mathematics (as well as of any other Department with the necessary technical qualification) should be encouraged to spend part of their time at the Center, under the Center's support. Initial housing should consists of a few rooms (1-or-2) made available at the Science Center for close contacts with the Dept. of Math.

GROWTH FORECAST. The first year of operation should be mainly orientational and operated under minimum conditions (part time Director, and one or two full time and/or part time research members). Subsequent growth should be the result of a mature synthesis between genuine scientific vision and sound administrative practice.

January 3, 1979

TO: Professors BOTT, HIRONAKA, KAZHDAN, MACKEY and STERNBERG
FROM: Ruggero M.Santilli

We have closed today Vol. 1, 1978 of the HADRONIC JOURNAL for a total of 1,598 pages,plus the Index. I would like to thank you (as well as all the members of the Department of Mathematics) for the logistic hospitality, whithout which this Journal could not have been possible. Also, the possibility of being in contact with you has been simply invaluable for me, as well as for the Journal, owing to the central objective:



More specifically(as you know) the central objective of the Journal is to appeal to all possible mathematical resources to formulate, in due time, the experimental verification for the strong interactions in general and for the hadron structure in particular of the BASIC physical laws (Einstein's special relativity, Pauli's exclusion principle, the spin-statistics theorem) which are experimentally established until now ONLY for the electromagnetic interactions. The idea is that only after such experimental resolution, the problem of the structure of hadron can be truly confronted in a final form.

You will be amused to know that this attitude has stirred up a genuine form of interest by outstanding physicists, as well as considerable "pain" by others (I understand that the sale of ALKA SELTZER has considebrably increased near quark-centers....).

Outstanding physicists such as A.SALAM (Director of the ICTP in Trieste, K.S.THORNE (Director of the Center for Astrophysics at CALTEC), B.DeWITT (Director of the Center for Relativity at the Univ. of Texas at Austin) as well as the Nobel Laureates YANG and PRIGOGINE and all members of the Editorial Council of the Journal and numerous other physicists have openly supported the study of the experiments in question.

Governmental Agencies (both the DOE and NSF) have also formally expressed their interest to support these studies. To my knowledge, a number of research proposals have already been filed and other are intended for filing in 1979. This support is apparently intended for the entire line, from the pure mathematical studies to the experimental proposals, and I tought you might be interested to know it. As far as myself is concerned, DOE appears to be serious in continuing (actually increasing) the support of my studies.

I enclose for your amusement copies of my recent two papers (written for The Phys. Rev. D, which means, in a non-provocative style). I also enclose the table of contents of my first volume with SPRINGER-VERLAG which is now in regular distribution. Regrettably, I do not have a complymentary copy for all of you (my personal copy is however at your disposal).

Please accept the sentiments of my sincere gratitude and my best wishes for a VERY GOOD 1979.

Ruggero

HARVARD UNIVERSITY
UNIVERSITY HALL 20, CAMBRIDGE, MASSACHUSETTS 02138

FACULTY OF ARTS AND SCIENCES
OFFICE OF THE ASSOCIATE DEAN

January 24, 1979

Dr. Ruggero Maria Santilli
Mathematics Department
Science Center
1 Oxford Street

Dear Dr. Santilli:

I am writing with regard to your recent correspondence to President Bok. He has forwarded this to Dean Rosovsky and the Dean and I have had an opportunity to review the issues you raised.

Unfortunately, the matters you discuss in your long memorandum of December 27, 1978, and in the more recent letter, are questions which, in the first instance, must be resolved at the departmental level. Thus, should you wish to pursue your proposal, you should ascertain whether or not there is any support for it within the members of the faculty of the appropriate academic department. It would then be up to that department to formally endorse and forward that proposal to the Dean for further consideration.

Sincerely yours,

Richard G. Leahy

RGL:bec

cc: President Derek Bok
Dean Henry Rosovsky

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

January 25, 1979

Professor HEISUKE HIRONAKA, Chairman
Department of Mathematics, Harvard University

Dear Heisuke,

After due consideration of the current status of basic research in physics, I have recently taken the liberty of proposing to President BOK, Dean ROSOVSKY and Associate Dean LEAHY the consideration for the possible setting of a new center of research at Harvard called CENTER FOR HADRON PHYSICS. Associate Dean LEAHY has recently informed me that I should ascertain whether ot not there is any support for such a proposal within the members of the faculty of the appropriate academic department. It would then be up to that department to formally endorse and forward the proposal to the Dean for further consideration.

I enclose copy of the rudimentary proposal, as originally submitted. As you can see, I recommend that the Center be affiliated to the Department of Mathematics. I would therefore appreciate the courtesy of your consideration of this proposal.

My recommendation to affiliate this possible Center to your department is based on the need of advanced mathematical knowledge for the treatment of rather fundamental physical problems which is only available at your department. Also, because I believe that an effective dialysis between mathematics and physics is much needed and would constitute a valuable scientific service.

The proposal itself was motivated by my sincere concern of the current status of basic research in physics, with regard to both, its short term and long term impact. I am here referring, in particular, to the experimental verification of the validity or invalidity of basic physical laws for strong interactions, and a number of related aspects, some of which predictably related to energy issues. In short, I believe that these experimental verifications are of such complexity as well as physical relevance, to warrant the organization of a Center specifically conceived for that function. In any case, these problems have been rather vigorously posed to the scientific community in 1978 and I believe that they will not be ignored by other campuses. The long term economic forecasts for this Country are, in my view, so cloudly that we simply cannot afford the luxury of overlooking fundamental issues in basic research.

In case you consider this proposal of any value, I would be happy to discuss it with you in more detail. As you can see, I had in mind a quite modest beginning, possibly funded via my DOE grant with Shlomo, if at all possible, and then a controlled growth as scientifically and financially appropriate.

I am sending a copy of this letter to Shlomo, but I have abstained from informing other collegues. I am also taking the liberty of informing Dr. DAVID C. PEASLEE of the Department of Energy (tel. 301 353 3624) of this initiative. I am sure that, in case you desire to consult him, he will be most cooperative.

Sincerely

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

February 14, 1979

Professor H. HIRONAKA, Chairman,

Department of Mathematics, Harvard University

Dear Professor Hironaka,

My function as editor of the HADRONIC JOURNAL calls that I clarify as soon as possible my address upon the expiration of my current appointment as research associate at your department on June 1979.

Pending a study of my case, I would be truly grateful whether you could kindly write me a letter indicating that I can be your personal guest with the understanding that

- such guest status will carry no salary and can be terminated at any time of your election;
- I shall pay on my own all logistic expenses (telephone, xerox, etc.); and
- I shall continue the use of my current stationary (as per this letter) as well as my formal address at the"Science Center" to avoid any unnecessary connection of your department with the editorial activities of the Journal.

Such a guest status would be fully sufficient to clarify my whereabouts after June 1979 with the administration of the HADRONIC JOURNAL, as well as with the distinguished scientists of its EDITORIAL COUNCIL.

Subsequently, I would appreciate your courtesy of exploring with Dr. D. C. PEASLEE of the Department of Energy, during your meeting at the end of this month, the possibility that my research associate position at your department be renewed for 1979/80 with my salary and expenses fully supported by the second year of my grant.

I have no words to express my appreciation and gratitude.

Sincerely Yours

Ruggero Maria Santilli

RMS/se

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 15, 1979

Professor R. GIACCONI,
Dept. of Astronomy

Caro Giacconi,

vorrei prendermi la liberta' di informarti delle attivita' dell' HADRONIC JOURNAL, che ho organizzato qui ad Harvard con HOWARD GEORGI (del dipartimento di fisica). L'indice del Volume 1 e' accluso come una prima informazione, mentre mi tengo a tua disposizione per copie dei numeri e per ogni altra informazione puoi desiderare.

Il "piccolo vento nuovo" che abbiamo tentato e' la riconsiderazione della vecchia idea che le interazioni forti non sono della struttura triviale $f = \partial V/\partial r$, ma bensi' sono strutturalmente piu' complesse, e plausibilmente di typo locale non-derivable da potenziale, come una approximazione di una realta' fisica di typo non-locale (questa pare fosse l'opinione anche di Fermi).

Per questo obiettivo, abbiamo promosso lo studio, come primo passo, di tutti i metodi per lo studio delle forze considerate (variationally non-self-adjoint) ed a livello sia classico che quantistico che della teoria dei campi quantizzati.

Allo stato attuale (leggi rudimentale) questi metodi si possono classificare nei seguenti due gruppi.

"Inverse Problem", che consiste nella riduzione delle equazioni ad una forma trattabile mediante algebre di Lie, equazioni di Hamilton ed Heisenberg ecc.;

"Lie-admissible problem" che consiste nel tentative di generalizare la struttura analitica delle formulazioni teoriche attuali, senza trasformare le equazioni di studio, e mediante una generalizzazione delle algebre di Lie chiamata algebre Lie-ammissibili. Infatti, le transformazioni di equivalenza per ridurre un sistema non-derivabile da potenziale ad uno che lo sia, spesso implicano l'impossibilita' di usare le coordinate usate nell'esperimento.... o l'uso delle algebre di Lie implica un sistema altamente non-inerziale, ecc. Questi problemi vengono a mancare nell'uso delle tecniche Lie-ammissibili che sono formulate per le coordinate di uso pratico, ma il prezzo da pagare e' abbastanza alto: le algebre Lie-ammissibili sono incompatibili con molta della conoscenza teorica attuale.

Un quadro abbastanza inquetante per alcuni e stimolante per altri e' quindi emerso da questa azione del 1978: l'ipotesi che le interazioni forti non sono derivabili da potenziale sembra essere incompatibile con le idee di Einstein, sia per la relativita' ristretta che per quella generale per il solo problema interno.

Come tu ben sai, al livello della fisica delle particelle elementari, oltre mezzo secolo di studi delle interazioni forti e' fondamentalmente basato sulla relativita' ristretta. Ma tu sei ben al corrente dell'esito di questi ingenti sforzi e finanziamenti: la congettura che i quarks sono i costituenti degli hadroni e la QCD (fondamentalmente dipendente dall'ipotesi che $f_{strong} = \partial V / \partial r$).

Il punto rimane che la validita' della relativita' ristretta per le interazioni forti e' solo un vango sentore a questo momento, senza nessuna verifica sperimentale sia pure parziale (a meno che la congettura dei quarchi, complementata dalla congettura che confinano, complementata dalla congettura della liberta' asimptotica, ecc. ecc., costituisce prova della validita' della relativita' ristretta per la struttura degli adroni....).

In aggiunta, questi studi sui quarchi sono cosi' dipendenti dalla relativita' di Einstein speciale che, se questa e' invalida per la structura adronica, l'ipotesi dei quarchi e' inconsistente nella sua formulazione, lasciamo stare il trattamento (tecnicamente. le forze forti non-autoaggiunte implicano una rottura del concetto di spinore e molte delle leggi di base; l'aspetto della carica frazionaria e irrilevante).

Lascio a te immaginare le implicazioni di una situazione del genere, inclusi gli investimenti attuali nei quarchi da parte delle agenzie governative che sono entrati d'un tratto in una condizione di limbo.

In ogni case, il dado e' stato tratto. Ti accludo copia di un volantino per due volumi di reprints su queste cose che e' stato stampato e spedito in circa 100.000 campioni (sic), in tutti i dipartimenti di matematica, fisica ed ingegneria. Ti accludo anche copia di altro materiale di tuo possibile interesse.

Se sei interessato ad essere messo al corrente di piu' dettagli, ti prego di farmelo sapere e faro' del mio meglio per farti avere copie dei volumi.

Ti fara' piacere sapere che tutto questo programmd e' stato lanciato con l'assistenza del Dipartimento dell'Energia (DOE) sotto il mio grant No. ER-78-S-02-4742.A.000. Attualmente sto decidendo il Dipartimento di Harvard a cui fare domanda per il rinnovo del mio secondo anno del contratto (i fondi sono gia' stati allocati, secondo mia conoscenza).

Ti sarei grato se potessi considerare la cosa. Nonostante una opposizione prevedibile (e strenua) da parte dei "quark-believers", la agenzie governative e molto dell'ambiente scientifico "limpido" sono pienamente a favore della verifica sperimentale della relativita' ristretta per le interazioni forti , per cui la cosa appare inarrestabile ora.

In linea di principio, un programma del genere dovrebbe essere di interesse diretto per il Center of Astrophysics. Comunque, mi sono astenuto dal fare una domanda, e questo e' il mio primissimo contatto, perche' non conosco affatto l'ambiente. Nota che non mi riferisco ad una richiesta di salario. No. Anzi, e' il contrario, nel senso che il mio salario piu' le overheads sono interamente pagate dal mio grant, come puoi verificare contattando direttamente l'ufficiale del DOE in carica del mio grant, Dr. D. C. PEASLEE, 301 353 3624.

Per concludere questa lunga lettera, ti prego innanzitutto di scusare ogni disturbo. Se sei interessato come collega a quello che sta succedendo nell'ambiente scientifico in forma confidenziale, ti prego di farmelo sapere. Puoi contare nel mio pienc rispetto della confidenzialita'. Infine, se credi che il programma in corso e' di interesse per il Center of Astrophysics ed un mio appointment a questo Center sia pieno che"joint" sia di vantaggio per questo centro, ti prego di farmelo sapere. Da parte mia sarei felicissimo di una associazione del genere.

Sperando di avere il piacere di conoscerti, rimango

Center for Astrophysics
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

February 23, 1979

Professor Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street

UNIVERSITY MAIL

Dear Professor Santilli:

I was most interested to read your letter and consider your thoughtful remarks about the desirability of applying the most advanced mathematical techniques to the study of physics. Unfortunately, much of the discussion is beyond my specific expertise and therefore I will have to remain a sympathetic bystander.

Sincerely yours,

Riccardo Giacconi

/s

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 28, 1979

Professor RICCARDO GIACCONI
Center for Astrophysics
60 Garden Street

UNIVERSITY MAIL

Dear Professor Giacconi,

I would like to express my appreciation for the courtesy of your letter of February 23, 1979. I hope you did appreciate my effort to be as candid as possible.

You might be interested to know that the initial reaction to our reprint series on Lie-admissible formulations has been rather encouraging. As a Government Officer recently put it to me, the problem of the experimental verification of the validity or invalidity of Einstein's special relativity for the strong interactions "simply cannot be ignored".

It will be a pleasure to keep you occasionally informed of most salient research developments at the HADRONIC JOURNAL, of course, on a fully informal basis.

Also, it would be a pleasure for me to meet you sometime.

Sincerely

Ruggero Maria Santilli

RMS/se

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

February 28, 1979

Professor RICHARD G. LEAHY, Associate Dean
Faculty of Arts and Sciences

UNIVERSITY MAIL

Dear Professor Leahy,

Following your letter of January 24, 1979, I have submitted the proposal for a feasibility study on the setting of a new center of basic research to Professor HIRONAKA, Chairman of the Department of Mathematics.

Subsequently, I arranged for a meeting between Professor HIRONAKA and Dr. DAVID C. PEASLEE of the High Energy Physics Division of the U.S. DEPARTMENT OF ENERGY on February 27. I am confident that Professor HIRONAKA will report to you the outcome of this meeting. On my part, I would like to reassure you that all the necessary precaution was taken to stress the informal, noncommittal nature of this feasibility study, as well as the need for confidentiality.

I also had two meetings with Dr. PEASLEE on this subject. Their outcome is essentially the following. Understandably, DOE intends to avoid any participation whatsoever in the decisional process at Harvard, whether to proceed with this feasibility study or not and eventually set the center or not. Nevertheless, in case Harvard decides favorably for the project, DOE appears genuinely interested to provide all possible assistance. It is understood that possible initial operations should be minimal, so that DOE has the necessary time to budget the potential research support without affecting current engagements. It is also understood that all possible efforts would be provided to avoid conflicts with other divisions at Harvard. For instance, the names suggested are "Center for basic research" or "Center for applied mathematics", rather than "center for Hadron physics".

My personal impression is that DOE is quite receptive to the projected primary function of the center. I am here referring to a coordinated, effective dialysis between advanced, pure mathematics and physics, with priorities on energy related issues. DOE also appears to be quite receptive to the needed, advanced mathematical knowledge for the theoretical and experimental study of the validity or invalidity of basic physical laws for the strong interactions (Einstein's special relativity, Pauli's exclusion principle, etc.). After all, these laws are currently assumed as valid in all funded research on the controlled fusion. You can easily figure out the administrative and scientific implications in case these laws, in the ultimate analysis, need implementations for the strong interactions, as by now believed by numerous qualified physicists. To put my personal impression in a vivid language, DOE (as well as other governmental agencies) simply cannot ignore these issues.

page 2.

During his visit at Harvard, Dr. PEASLEE has also explored the possibility for the continuation of my research under full DOE support for a second year. I understand that Professor HIRONAKA will explore the possibility of a renewal for a second year of my nonteaching, nontenured, terminal appointment. As a result, I have absteined from submitting an application to other divisions at Harvard. I am confident that Professor HIRONAKA will report to you on this matter too in due time.

In closing, you might be interested to know the recent scientific activities at the HADRONIC JOURNAL. Volume 1, 1978 has been completed on schedule for a total of 1,603 pages. An informative leaflet is enclosed. Volume 2, number 1 (February) 1979 has also been completed (today) on schedule. The Table of Contents is enclosed. As you can see, the Journal continues the nonmonopolistic presentation of qualified research on hadron by quark believers as well as quark nonbelievers.

The call for a moment of reflection on the basic physical laws for the strong interactions launched via Volume 1, 1978 of the Journal has seen a truly encouraging response. All papers on these topics are being reprinted in two first volumes of a yearly series entitled "Applications of Lie-Admissible Algebras in Physics", Edited by Professor H.C. MYUNG (the current leading mathematician on Lie-admissible algebras), S. OKUBO (an outstanding authority on hadron physics) and myself.

A blue leaflet on these reprint volumes is enclosed. You might be amused to know that this leaflet has been printed and distributed in 60,000 copies (sic) to the world wide mathematics and physics communities.

Very Truly Yours

Ruggero Maria Santilli

RMS/se
c.c.: President DEREK BOK
Dean HENRY ROSOVSKY
encls.

TO: THE SENIOR MEMBERS OF THE DEPT OF MATH April 29, 1979
FROM: R.M.SANTILLI

As you are eventually aware, the U.S.DEPARTMENT OF ENERGY has formally approved the renewal of the grant for the support of my salary during 1979-80 with my affiliation to your department, under the new grant number AS02-78ER04742. I understand that there are difficulties for the renewal of my research associate position at your department for an additional terminal year.

I believe that these difficulties are due primarily to my lack of keeping you sufficiently informed of my research activities, as well as of their implications for the promotion of the development of mathematics. I have therefore interrupted my research, to prepare for you a review paper on the rather intriguing status of strong interactions. A copy of this paper is enclosed with the understanding that it is the result of a few days of work and, as such, of preliminary character.

I hope that, among your many duties and activities, you will find the time to look at this paper. You may then see why so many scientists all over the world (including Nobel laureates) consider as of final character the Mendeleev-type classification of hadrons via unitary groups. Yet they question the conjecture that quarks are the constituents of hadrons. More importantly, I hope that this paper will indicate to you that the current search for a more adequate treatment of the strong interactions has all the ingredients to constitute a genuine thrust for the development of numerous branches of mathematics.

I also hope that you will see with benevolence my intemperances with your Lyman collegues. The disagreements in our community of basic research are not of minute technical character. Instead, they are related to fundamental issues. As such, they stir up great emotions on all sides. The roots of my disagreement with your Lyman collegues are quite simple. On my part, I respect studies on quarks and, as an editor, I routinely accept them for publication. However, the doubts on quarks are too numerous and too substantial to justify the restriction of research on the fundamental problem of hadron structure to quark conjectures only. As a researcher and editor, I therefore favor a more balanced and mature conduction of studies on the sector in which quark lines are considered jointly with all other conceivable lines of clear scientific promise. As such, I vigorously oppose any more restrictive view, whether of quark or nonquark inspiration.

In any case, I hope you understand and respect my scientific convictions and determination. I decided long time ago to pursue knowledge in physics, rather than a career in physics, with full cognizance that this is often unrewarding. In essence, I simply cannot compromise with my scientific ethics, at whatever personal cost.

You should be informed that the DOE is interested in having me spending one additional year at your department. The reason is due to the fact that my research calls for applications of advanced mathematical tools. In turn, these tools are expected to play a crucial function for the formulation and execution, in due time, of experiments of truly crucial physical character: the experimental resolution of the validity or invalidity for the strong interactions of currently used laws and principles (Einstein's special relativity

and Pauli's exclusion principle, in particular), which are experimentally established until now only for the electromagnetic interactions.

Permit me to be candid on this issue. Tenured physicists can afford the luxury of expressing personal beliefs. Governmental officers cannot. They are responsible to the taxpayer. I am confident you will see that the U.S. Department of Energy cannot continue to invest indefinitely truly large amounts of money on hadron physics, all based on the mere belief of the validity of the basic laws, without jointly promoting their experimental verification.

I would like to stress here that the sole objective of my grant with DOE is the study and promotion of these basic experimental verifications. On my part, I would be honored to collaborate with your Lyman collegues on these crucial physical issues, and it is regrettable that this collaboration did not materialize. It is also regrettable that there exist a number of physicists opposing these crucial tests. But they are a minority by now and, in any case, they cannot arrest a scientific program of the United States Government already in motion.

Please take all the necessary time to reach your final decision. I am at the disposal of all of you for any additional information you might desire. Nevertheless, please take into consideration that I have not applied for a position elsewhere and, more importantly, that I cannot apply without the prior consultation with the Department of Energy.

I would like to suggest to you most warmly that you communicate directly to the Department of Energy your final decision on my appointment. This is recommendable to avoid an unnecessary repetition of the Lyman episode of April-June 1978 (The DOE had funded my proposal with my affiliation to the Lyman Laboratory, but Lyman failed to report the negative decision on my appointment to the DOE and created unnecessary aggravations).

I am confident that your final decision will be a reflection of your scientific ethics, as well as of the traditional scientific freedom at Harvard. If your final decision will be favorable, I will be honored to spend another year with you. If your final decision will be negative for whatever reason, I would like to confirm what I indicated verbally to Eisuke, that you can count on my best collaboration for an orderly transfer of the grant.

I am and I will be always grateful to Shlomo, because what I have learned from his lectures I treasure most, to Heisuke, because of his genuine committment to science and mature conduction of my case, and to all of you, for the hospitality during the current academic year.

Sincerely

Ruggero

c.c.: President DEREK BOK, Dean HENRY ROSOVSKY and Associate Dean RICHARD LEAHY.

April 19, 1979
Revised May 15, 1979

Preliminary draft

Any critical comment by interested colleagues for the finalization of this paper would be gratefully appreciated

AN INTRIGUING LEGACY BY ALBERT EINSTEIN:

THE EXPECTED INVALIDATION OF QUARK CONJECTURES

Ruggero Maria Santilli*

Science Center
Harvard University
Cambridge, Massachusetts 02138

* Supported by the U.S.DEPARTMENT OF ENERGY under contract number ER-78-S-02-4742.A000

This preprint has been printed and distributed to the scientific community by the Hadronic Press in 15,000 samples.

To be submitted for publication.

ABSTRACT

The objective of this paper is to present an outline of the rather numerous criticisms on the quark models of hadron structure which have been lingering in our community of basic research for some time.The main line of the paper consists of the presentation of the various arguments according to which the unitary models provide a Mendeleev-type classification of hadrons of unequivocal physical value and of virtually conclusive character. Nevertheless, the quark models are not expected to provide a joint model of structure of each individual element of a unitary multiplet, nor quarks are expected to exist as physical particles. The hope of this paper is that quark supporters will inspect these criticisms, and present possible counter-arguments, for a scientifically effective resolution of these issues in due time. In particular, this paper is an appeal to experimenters in high energy physics to provide means for an effective selection among an ever increasing number of hadronic models. It is submitted that the problem whether quarks exist or not as physical particles necessarily calls for the prior experimental verification of the validity or invalidity of the basic physical laws used in quark models, with particular reference to Einstein's special relativity and Pauli's exclusion principle, which are experimentally established until now only for the electromagnetic interactions. A number of proposals of specific tests exist in the literature, although in a predictable preliminary form, and numerous other tests are conceivable, all apparently feasible with the current technology. In any case, it is unlike that research in the sector can be effectively continued without these basic experimental resolutions. But, most of all, this paper is an appeal to young minds of all ages. The possible invalidity of conventional physical laws for the strong interactions, rather than constituting a scientific drawback, represents instead an invaluable thrust toward the search for covering laws specifically conceived for the strong interactions, which has already been initiated by a number of researchers. In turn, this situation has all the ingredients for a scientific renaissance, that is, the pursuit of genuine non-incremental advancements in mathematics and physics.

CONTENTS

1. THE QUARK MODELS

2. THE FUNDAMENTAL ASSUMPTIONS OF THE QUARK MODELS

3. THE PROBLEM OF THE ARENAS OF VALIDITY AND INVALIDITY OF EINSTEIN'S SPECIAL RELATIVITY IN PARTICLE PHYSICS

4. THE PROBLEM OF THE ARENAS OF VALIDITY AND INVALIDITY OF QUANTUM MECHANICS IN PARTICLE PHYSICS

5. THE EXPECTED INVALIDATION OF QUARK CONJECTURES

6. THE EXPERIMENTAL PROFILE

7. A SCIENTIFIC RENAISSANCE STIMULATED BY STRONG INTERACTIONS?

- REFERENCES AND FOOTNOTES

o o o

May 3, 1979

Professor H. HIRONAKA
Dept. of Math.

Dear Heisuke,

I am taking the liberty of enclosing copies of personal, confidential letters (from my file) on my work, in case they may be of some assistance for the proceedings of my possible reappointment.

Please notice that these letters are unsolicited and, as such, are a spontaneous expressions by their authors on my work. You will also notice that some of them are eccessively genererous on my grossly incomplete work. Please consider them nothing more than an expression of the genuine enthusiasm around the "scientific rainaissance" which has been touched in the paper you received hyesterday.

Finally, I have selected the letters enclosed from my personal file, to give you an indication of the reaction to my work by mathematicians, physicists and engineers. In case I can be of any further assistance, please do not hesitate to let me know.

I simply have no words to express my gratitude for your consideration.

Sincerely



President Bok,
Best and sincere regards,

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

President DEREK BOK,
Harvard University

May 6, 1979

Dear Mr. President,

A number of regrettable circumstances has forced me to write the enclosed paper of review on the rather numerous criticisms on quark models in hadron physics moved by distinguished scientists all over the world, including Nobel laureates. The wide distribution of this paper (15,000 copies via Hadronic Press) is intended to indicated to quark-committed physicists that a critical process of examination of quark conjectures is in motion on a world wide scale. Therefore, their corridor-type of opposition to these studies has little scientific value. If they have technical arguments to disprove these criticisms, they should present them in scientific papers.

This paper is the result of indications in my possession that the situation in this crucial sector of research has deteriorated considerably, both at Harvard and outside, since the time or my confidential report to you of December 27, 1978.

Here at Harvard, the Department of Energy has formally approved the renewal of my grant with my affiliation to the Department of Mathematics, as filed, for the study of the experimental verification of the validity or invalidity for the strong interactions of the currently used, basic, physical laws (with particular reference to Einstein's special relativity and Pauli's exclusion principle), which are experimentally established until now only for the electromagnetic interactions. But considerable difficulties have lately emerged for the renewal of my research associate position at the Department of Mathematics for one terminal year under this grant. I heard rumors that these difficulties are due to lobbying by senior, quark-committed physicists at Lyman. But I am not interested to know whether this is indeed the case or not. Copy of my recent letter to the senior members of the Department of Mathematics, as well as copy of confidential letters of comments on my work by collegues, are enclosed for your file.

In any case, it appears that there is a tense moment on campus which calls for defusing. In my perhaps erroneous, personal view, it is essentially due to the lack of graceful acceptance by quark-committed physicists of critical studies on quark conjectures. I would like to stress here again that I have no animosity whatsoever against these physicists. Yet, I consider essential the conduction of critical studies on current trends on the fundamental problem of hadron structure and their assumed laws. Without this scientific process we are pursuing power, trather than truth.

page 2.

Outside Harvard, there has been an apparent increase, to my knowledge, of the number of physicists who believe that the truncation of the alleged monopoly of funds and human resources in the sector by quark conjectures is a matter of primary national interest. Again, these are responsible scientists. They favor the continuation of funding of quark conjectures. Nevertheless, they vigorously oppose the restriction of research in the sector to quark conjectures only. What has deteriorated the situation is again the lack of graceful acceptance of these sound requests by quark committed physicists, currently in control of the sector. There is noting personal against them. It is a mere fact of scientific life. After a decade of undisputed control of the scientific scene and investments of the order of billions of dollars, the quark models on hadron structure are more controversial than ever. Any scientist with a genuine committment to the pursuit of knowledge can see that it is time now to search, promote and fund studies alone new ideas (promising ones exists in the literature), jointly with the continuation of studies along old ideas.

The point of potential rupture is that indicated in my report to you of December 27, 1979, the apparent opposition by some (but not all) quark-committed physicists against the conduction of experiments on the validity or invalidity of the basic physical laws used in quark conjectures. Indeed, such an alleged attitude can only invite a crisis.

Permit me to say that, whatever the future will bring, I am in peace with my soul. With my report to you of December 27, 1978 I have accomplished my moral duty: to provide Harvard administrators with all necessary elements of judgement, as seen from one side of this scientific dispute. The idea of the delineation of a scientific crisis without any cognizance at all by Harvard administrators was contrary to my ethical code. On my part, you can rest assured that I have kept utmost confidentiality on this report, to the point of avciding the indication of its existence to my wife, let alone collegues and outsiders.

Permit me the liberty of indicating again that, in my view, the only way to defuse this deteriorating situation is via the intervention of administrators. I simply do not believe that physicists can effectively administer themselves under the current circumstances.

If I can be of any assistance, please do not hesitate to contact me.

Sincerely

Ruggero Maria Santilli

c.c.: Dean H. ROVOVSKY and Associate
Dean R. LEAHY.

Encls.

May 21, 1979

Dear Heisuke,

Shlomo has difficulties for my keeping the current office (room 435). Please consider the possibility that I occupy room 515 (Shlomo secretarial office), as soon as he makes it available for his leave. Perhaps, this office is considerably less exposed than my current one and could solve this (rather unusual) problem.

In case difficulties persist, please advice me whether it is appropriate that I apply for a joint position at the Center for Astrophysics in the intent of moving there my Editorial Office of the HJ, in case accepted.

I am sorry to be of any inconvenience to you. Unfortunately, I live in a two bedroom flat with a family of four. It is absolutely impossible for me to work at home.

In relation to the DOE grant budget I would like to confirm to you what verbally indicated to Shlomo, that I shall accept and endorse whatever he decides.

Again, please accept the sentiments of my sincere gratitude,

Sincerely



HARVARD UNIVERSITY

OFFICE OF THE PRESIDENT

MASSACHUSETTS HALL
CAMBRIDGE, MASSACHUSETTS 02138

May 23, 1979

Dear Mr. Santilli:

This is just a note to thank you for sending me a copy of your preliminary draft on quark models in hadron physics.

With best wishes,

Sincerely,

Derek C. Bok

Mr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street

PART IC:

ACADEMIC
YEAR
1979–
1980

HARVARD UNIVERSITY

OFFICE OF THE SECRETARY
17 QUINCY STREET

CAMBRIDGE, MASSACHUSETTS

September 7, 1979

SIR,

I beg to inform you on behalf of the University and the Dean of the Faculty of Arts and Sciences that you are appointed

Research Associate in Mathematics

to serve for one year from June 1, 1979 subject to the Third Statute of the University (*overleaf*).

Your obedient servant,

Secretary to the University

Ruggero Maria Santilli

HARVARD UNIVERSITY
FACULTY OF ARTS AND SCIENCES

OFFICE OF THE DEAN

5 UNIVERSITY HALL
CAMBRIDGE, MASSACHUSETTS 02138
(617) 495-1566

June 6, 1979

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street

Dear Dr. Santilli:

Thank you for your letter of June 4th. I will be leaving for England shortly after Commencement and consequently I really don't have any free time before I go. However, I appreciate your courteous note and trust you will understand why I can't now schedule an appointment for you.

Yours sincerely,

Henry Rosovsky

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

June 26, 1979

Professor FRED L. WHIPPLE
Harvard University
Department of Astronomy

University Mail

Dear Professor Whipple,

Following a suggestion by THORNTON PAGE (see enclosed letter), I am contacting you for assistance in the technical and editorial finalization of a recent paper of mine entitled "An intriguing legacy by Albert Einstein, etc". I am enclosing the copy of the paper with the corrections and comments by Dr. Page up to p. 14. As you can see, my knowledge of the English language is truly limited, and an editorial control is much needed. A technical control or criticism would be much appreciated too. I am referring in particular to Section 3, on the problematic aspects of the special relativity for the strong interactions as well as of the general relativity for the interior problem(only). Your candid criticisms of these speculations would meet with my sincere gratitude.

Almost needless to say, you can count on my confidentiality for any possible assistance. In particular, I shall include your name in a possible acknowledgment section only upon your inspection and autorization. But, if you do not have the time to look at this paper, simply return it to me. I will be equally obliged for your consideration.

I have not yet decided the Journal for submission, pending the preparation of a more mature version. In any case, I would like to abstein from publishing this paper in the HADRONIC JOURNAL, owing to my position of editor.

Permit me to confess that this has not been an easy paper for me to write. The primary objectives were: (1) to stimulate a moment of reflection on quarks; (2) to promote the experimental verification of the basic physical laws currently used in strong interactions; and (3) to stimulate the conduction of nonincremental research, jointly with more conventional studies.

A difficult part was to minimize the expected negative reaction by quark committed colleagues. For this purpose I have presented only criticisms on the topic at large, and I have avoided the presentation of additional criticisms which would have called for specific reference to specific papers. Nevertheless, quite candidly, I do not know whether I did indeed achieve my objectives in a scientifically balanced way.

page 2.

Also, I do not know whether I properly conveyed my respect and favor for the continuation of studies along quark lines. As a matter of fact, I routinely accept (and often invite) for publication in the HADRONIC JOURNAL papers along these lines. My paper was simple an expression of my personal uneasiness and concern on the virtual complete restriction of studies on strong interactions along quark lines which has occurred in recent times, with considerable stagnation (also in my view) of genuine advancements in mathematical and physical knowledge in the sector. In essence, I favor a more balanced conduction of research in the sector in which all valuable lines, of both quark and non-quark inspiration, are pursued.

Finally, your council on Section 5 on Einstein's criticism for quantum mechanics would also be appreciated. I have received mixed comments on it. A first group favors the emphasis on this "legacy" in a specific section and in the title, because it has been only academically discussed until now, that is, without the actual conduction of research. A second group (mainly composed by quark committed friends) is against this emphasis, and suggests only a mention, because the strong interactions were unknown when Einstein's conceived his special relativity (see the remark of p.25, last line). I personally favor some form of emphasis on this "legacy" because I believe it is time to consider it seriously. But, again, I do not know whether I did achieve a well balanced presentation.

You might be intrigued to know that a rather feverish research activity is going on on the issues touched in this paper. As you can see in the enclosed copies of the content of the HADRONIC JOURNAL, virtually every issue presents papers either directly or indirectly related to the problem. Additional papers are appearing in other Journals and in conference proceedings. The contributions in 1978 have been reprinted in two volumes (see the enclosed flier) and a complimentary copy is at your disposal upon request. A copy of my monographs on the classical profile (with Springer-Verlag) and on the quantum mechanical profile (with Hadronic Press) is also at your disposal upon request.

Most importantly, a number of experimentalists in high energy physics, following the distribution of the preliminary draft of my paper, have expressed the awareness of the need to initiate a serious experimental study on the validity (according to some) or the invalidity (according to others) of conventional atomic laws for the hadronic structure.

Hoping that I did not abuse of your courtesy and time, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml
encls.
P.S. Please feel free to call me, if you so desire, I would be happy to visit you at any time of your convenience.

June 26, 1979

From the desk of:
FRED L. WHIPPLE
Director, Smithsonian Astrophysical Observatory
Professor of Astronomy, Harvard University
Emeritus.

Dear Mr. Santilli:

I do not consider myself able to be of help on the theoretical aspects of this paper. Since I am starting immediately on vacation, I could do little on other aspects of the paper until September anyhow.

So I return the paper to you

Regretfully,

Fred L. Whipple

[redacted]

[redacted]

14 June 1979

Reply to Attn of Code SN

Dr. R.M. Santilli
Science Center
Harvard Univ.
Cambridge, Mass. 02138

Dear Dr. Santilli:

Your goals for this paper are good but your use of the English language needs improvement. I have marked suggested changes in red.

I would question your doubt of Special Relativity for strong interactions on p. 12.

After p. 13, I gave up editing your English. You should get help from some of your colleagues at the Science Center. Fred Whipple at the Center for Astrophysics 160 Garden St. would surely be willing to help. Tell him that I know his English is good! Dave Layzer would also help.

I am not competent to judge your arguments against quarks in hadrons, but I hope your paper will straighten out the pro-quark and anti-quark groups.

Sincerely,

[redacted]

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

July 26, 1979

Professor G.B.FIELD, Director,
Center for Astrophysics
Harvard University

University Mail

Dear Professor Field,

I am here applying for a position at your Center commensurate to my qualifications. I enclose for your consideration my resume, as well as informative material on my recent research, teaching and editorial activities. I am also enlcosing copies of my monograph "Foundations of Theoretical Mechanics", Volume I, with Springer-Verlag, as well as copies of the reprint volumes "Applications of Lie-admissible algebras in physics", I and II with Hadronic Press. The courtesy of returning these volumes to me at your convenience would be appreciated. Finally, I enclose my list of references. I would appreciate the courtesy of indicating whether you prefer to contact directly these colleagues, or I should solicit letters of recommendations.

I am currently the corecipient of a research grant with the Department of Energy with Shlomo Sternberg of the Dept. of Mathematics as principal investigator. Apparently, DOE is interested in the continuation of this support. Please feel free to contact Dr. D. E. Peaslee of the DOE, tel. 301 353 3624, if you so desire. I would like to have the opportunity, in case I join your Center, of applying for the continuation of this grant, either as principal investigator, or as coinvestigator, jointly with interested colleagues. Since the current grant expires on June 1, 1980, the new application should be filed by September 1979. Your consideration of this aspect during the consideration of my application would be appreciated.

I am currently the editor in chief of the Hadronic Journal and I would appreciate the possibility of continuing this editorial function. The journal has completed Volume 1, 1978 for some 1,602 pages, and volume 2, 1979 is now at an advanced stage. As you can see in the enclosed informative material, our journal is attempting a dialysis between advanced mathematics and open physical problems in strong interactions. The Hadronic Journal, in particular, has initiated the treatment by mathematicians and physicists of a generalization of Lie algebras known under the name of Lie-admissible algebras, because of their applicability for the classical and quantum mechanical treatment of forces nonderivable from a potential. You are eventually aware that the strong interactions have been long suspected as being of this type by the founders of contemporary physics (Enrico Fermi and others).

page 2.

I am also the co-organizer, jointly with a mathematician, of a yearly meeting called "Workshop on Lie-admissible formulations". The meeting of this year will be held here at the Science Center from August 1 until 4. Participation is restricted to a few mathematicians and to a few physicists working in the field. This workshop is organized in the intent of conducting research without prejudices, as much as humanly possible. Also, the workshop is devoted to the treatment of fundamental, open, mathematical and physical problems (studies of minute incremental character are gently diverted to other meetings). In case I join your Center, I would appreciate the opportunity of continuing this meeting. No expense on behalf of your Center or of Harvard University is expected (the direct costs for the 1980 meeting will likely be paied by the DOE).

I am currently a nontenured member of the Department of Mathematics. Nevertheless, as you can see, I do not disclose this association in my letter head owing to my editorial function. In case I join your Center, you can rest assured of my best cooperation in regards to this issue.

The primary objective of my research is to study the problem of the experimental verification of the validity (according to some) or invalidity (according to others) of conventional physical laws for the strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

The search for maturity of formulation of this problem is conducted via (1) my series of monographs with Springer-Verlag and the Hadronic Press (see my curriculum); (2) research papers I am currently writing with mathematicians and physicists; (3) the coordination of the efforts by independent researchers via the Hadronic Journal; (4) contacts with qualified colleagues (experimentalists in particular); and (5) the yearly workshop on Lie-admissible formulations. All valuable contributions in the study of the problem considered are reprinted in yearly volumes, the first two of which have been included. Two additional volumes are under preparation.

This project has now passed the predictably nebulous,orientational phase and is now entered in the second phase of technical treatments of specific issues, thanks also to an increasing participation by mathematicians. Almost needless to say, the complexity of the topics to be confronted is such that a considerable way remains to be covered to achieve maturity of formulation of actual experiments. Nevertheless, a number of experiments have already been proposed, and a rather feverish research activity is now going on, as a result of the efforts initiated at the Hadronic Journal.

I believe that these studies have a rather direct astrophysical implication. My application to join your Center has been submitted in this spirit.

RMS/ml
encls.
c.c.: President Bok, Dean Rosovsky and Associate Dean Leahy (to fulfill an old promise).

Yours, Very Truly
Ruggero Maria Santilli

Center for Astrophysics
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

1 August 1979

Dr. Ruggero Maria Santilli
Harvard University
Science Center, Room 331
One Oxford Street
Cambridge, Mass. 02138

Dear Dr. Santilli,

Thank you for your letter of July 26. Clearly you are very active in your chosen field. As regards your possible association with the Center for Astrophysics, I must respond that such an association would not be appropriate. It is our policy to encourage research at the Center which is directly related to astrophysics and to resist the temptation to go into fields which are not directly related.

Sincerely,

George Field

P.S. I return your published materials herewith.

GF/dr

cc: D. Bok
H. Rosovsky
R. Leahy
H. Hironaka

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

August 10, 1979

Dr. GEORGE FIELD,
Center for Astrophysics

Dear Dr. Field,

Following your letter of August 1, 1979, I would like to reassure you that I understand and respect your position and that, on my part, I do not intend to pursue any more my association with your Center. Unfortunately, since I was expecting a normal consideration jointly with other candidates, I did solicite letters of recommendation on my behalf, and you might receive them in the near future. I would appreciate the courtesy whether you can simply keep these letters in your file without returning them to their authors.

You might also be interested to know that, unless suggested by senior members at Harvard, I do not intend to pursue my association with other divisions of this campus. I am grateful to the Department of Mathematics for the possibility of receiving my DOE grant for two years. Nevertheless, I am not a mathematician , nor I intend to become a mathematician. As a result, I do not intend to abuse of the hospitality of this department by asking for a further extension.

I sincerely have no intention of disagreeing with you. Nevertheless, permit me the liberty of indicating that the studies I am conducting with a growing number of mathematicians and physicists are indeed directly related to astrophysics. As director of a center for astrophysics, I believe that you should be informed of their existence, for whatever their value is.

I am taking the liberty of outlining here a few points in nontechnical language. The technical profile is now treated in too numerous papers in different languages and techniques to be effectively summarized.

The root of the current situation in theoretical physics which has propagated directly into astrophysics (in my view) is the fact that the virtual totality of classical, quantum mechanical and quantum field theoretical methods were conceived for and remain capable of treating only point-like approximations of particles, whether the peripheral electron of a hydrogen atom or a proton created in the core of a neutron star.

This is, after all, a consequence of the strict locality of the theories available. My monographs with Springer-Verlag inspect this situation at its conceptual foundations, Newtonian Mechanics. In particular, they provide a technical treatment of the fact that variationally selfadjoint physical models, whether in Newtonian Mechanics or in Riemannian geometry can only characterize action-at-a-distance interactions among point-like approximations of objects. You should keep in mind that these studies have identified as being variationally selfadjoint all current models used in astrophysics, with the possible exception of only a few cases in which technical difficulties (e.g., singularities) render debatable the computation of the adjoint systems.

We believe that these selfadjoint models have a clear place in physics of clear physical value, such as, for the exterior problem of test particles in astrophysical objects. Nevertheless, these models are not believed to be of terminal character, nor that astrophysics will stop at the current views, irrespective of the number and scientific authority of the current supporters. Indeed, the same models, when applied to the interior problem of astrophysical objects, and are truly identified in their physical foundations, provide a model, say, of the interior of a neutron star as composed of point-like constituents with action-at-a-distance forces only. This is contrary to experimental evidence that hadrons have an extended size ($\sim$1F).

Even though pursued by only a few, gaunt researchers at this time, and against all sorts of oppositions (sometimes strictly intended to prevent the conduction of these studies), what we are attempting is the reinspection of physics in the intent of achieving an initial, simple, but genuine representation of the extended character of particles in interation, whenever such a representation is physically appropriate (e.g., the dynamics of a proton within a neutron star). The methods we are searching are subjected to what we call an "uncompromisable condition" of being a covering of conventional formulations for point-like abstractions (that is, capable of recovering the latter at the limit of point-like objects). To avoid the human error of most of our colleagues, we do not claim that our results are of terminal character. They are intended only as a first step in a problem which will likely call for generations of researchers.

This objective is now pursued at a number of methodological levels, with particular reference to the algebraic, geometrical, quantum mechanical and quantum field theoretical levels.

The astrophysical, direct implications of these studies have clearly transpared at the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, that we have just completed. The proceedings (by mathematicians and physicists) will be published sometime in December 1979. A number of the articles are specifically devoted to initial steps in astrophysical problems (mainly the algebraic, geometrical and quantum mechanical treatment of

extended hadrons within the core of astrophysical objects).The idea is that we should first try to understand the behaviour of one such extended particle , before attempting the construction of models for a collection of such particles. Initial attempts for this transition have been set for the THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, scheduled for August 1980.

Particularly intriguing has been the following outcome of the workshop. It consists of a possible link between the vexing problem of quantization of conventional gravitational models and the van Hove no go theorem of (pre) symplectic quantization, jointly with genuinely new possibilities of bypassing this impass via our broader tools (Lie-admissible algebras and symplectic-admissible geometry). Of course, this topic is under study at this moment, and it will take some predictable time before it will appear in print.

It is for me difficult to indicate all the rather numerous aspects of strict astrophysical inspiration.This is mostly due to the novelty of our mathematical methods. For instance, the Lie-admissible generalization of the Lie algebra is virtually unknown in the mathematical community at large, let alone the physical community. *

Perhaps, a representative episode (which will amuse you) occurred recently within a Newtonian context. While Skylab was falling, NASA was somewhat embarassed because of the inability to predict with even a minimum of indication the location of impact (a few days before the fall, NASA was unable to make any prediction, that is, it could have fallen everywhere on the projection of the trajectory on the entire Earth). Owing to this situation, as well as the known potential political implications, the virtual entirety of our theoretical knowledge was feverishly inspected in the hope of gaining some insight. In turn, this episode provided a clear identification of the limitations of current theoretical views.

I was contacted by NASA because they had just learned of my studies for forces more general than $f = -\partial V/\partial r$, as well as of the rigorous analytic techniques of the Inverse Problem for the treatement of these more general forces (my monographs with Springer-Verlag). I was unable to make any contribution, understandably, because of only a couple days notice... Yet my contact with NASA established the following point.

The virtual totality of current theoretical tools in classical mechanics resulted applicable only to point-like abstractions and action-at-a-distance forces (variationally selfadjoint methods).On the contrary, the dynamics of Skylab was strictly nonselfadjoint, because of drag forces necessarily nonderivable from a potential, as well as the necessary extended character of the object, as well as its orientation.

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* In case you are interested, you may inspect some of the papers I wrote in astrophysics and general relativity, as per my curriculum.

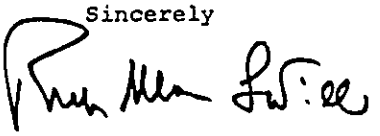
page 4.

As one NASA man vividly put it to me, in case a theoretical physicist comes here proposing to treat SKYLAB with his conventional relativities and inherent point-like approximations, "he would be likely chased out of NASA premises".

In my humble view, the current level of knowledge in astrophysics for the treatment of the interior problem is fundamentally equivalent to the point-like abstraction of the motion of Skylab in Earth athmosphere. The differences are merely technical (curvature, quantization, etc.). Indeed, once the physical reality is finally confronted, the motion of an extended hadron within superdense hadronic matter (the core of a star) is indeed conceptually equivalent to the the motion of the extended Skylab in dense Earth's atmosphere. But then the need for a severe inspection of current theoretical views in astrophysics is simply unavoidable. The difficulties for attempting a genuine advancement in astrophysics, which cannot be disjoint (in my view) from strong interactions, are of human and not of technical character: the desire by the majority of physicists (with numerous exceptions and, I believe, you included) to remain attached as much as possible to old ideas.

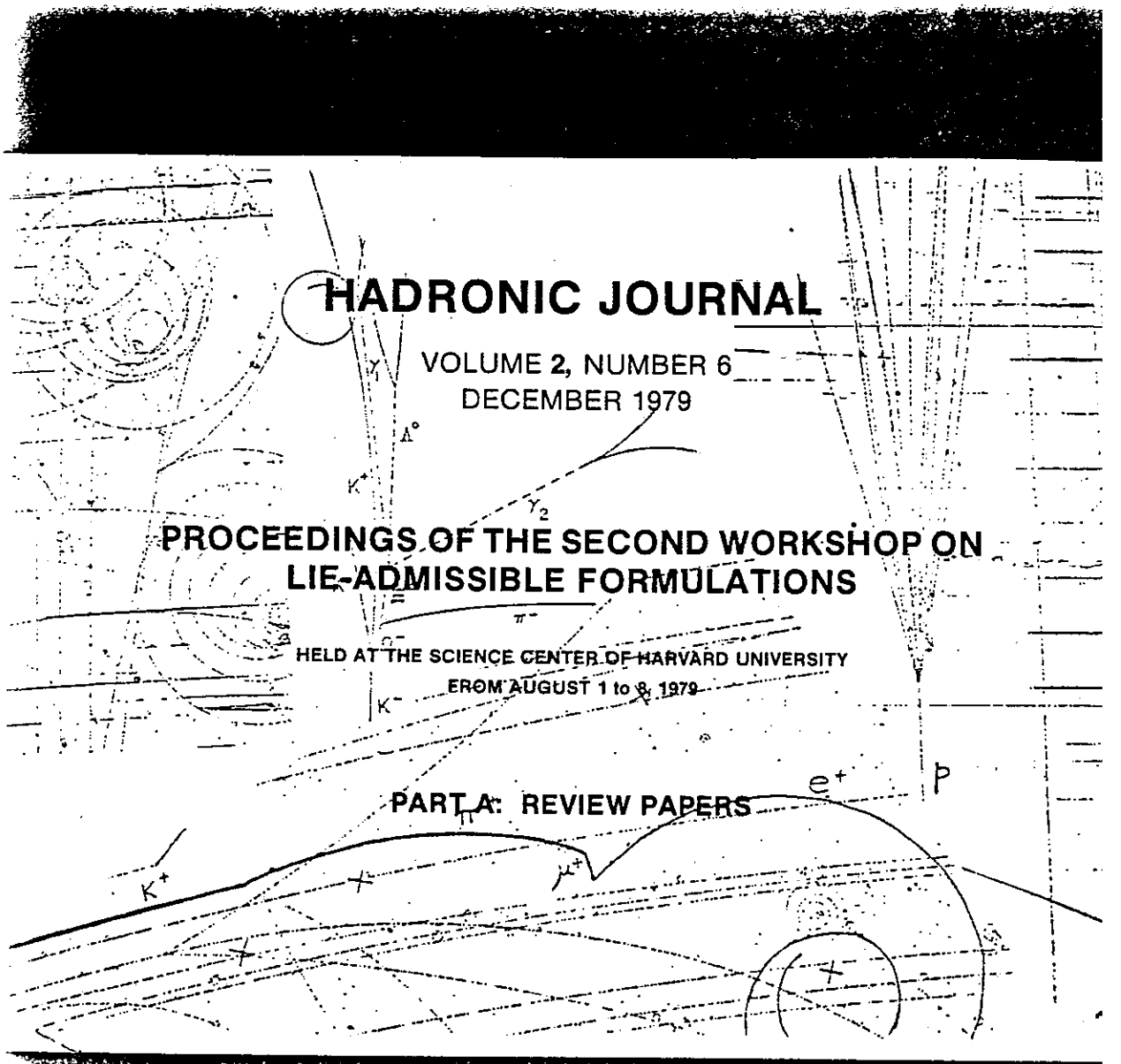
As I indicated earlier, this letter terminates my search for a possible continuation of my studies at Harvard. I would like to take this opportunity to stress that my loyalty to Harvard will remain untamed, as the only expression of gratitude for the hospitality received which is compatible with my ethics. Jointly, however, benevolently allow me to express my sadness for the apparent lack of interest at Harvard in the studies of these fundamental physical problems, as well as the rather strong opposition I have encountered here during their conduction.

Sincerely



Ruggero Maria Santilli

c.c.: Drs. D.BOK, H.ROSOVSKY,
R. LEAHY and H. HIRONAKA



HADRONIC JOURNAL

VOLUME 2, NUMBER 6
DECEMBER 1979

PROCEEDINGS OF THE SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS

HELD AT THE SCIENCE CENTER OF HARVARD UNIVERSITY
FROM AUGUST 1 to 8, 1979

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HADRONIC JOURNAL

Volume 2, Number 6, December 1979

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held at the Science Center of Harvard University
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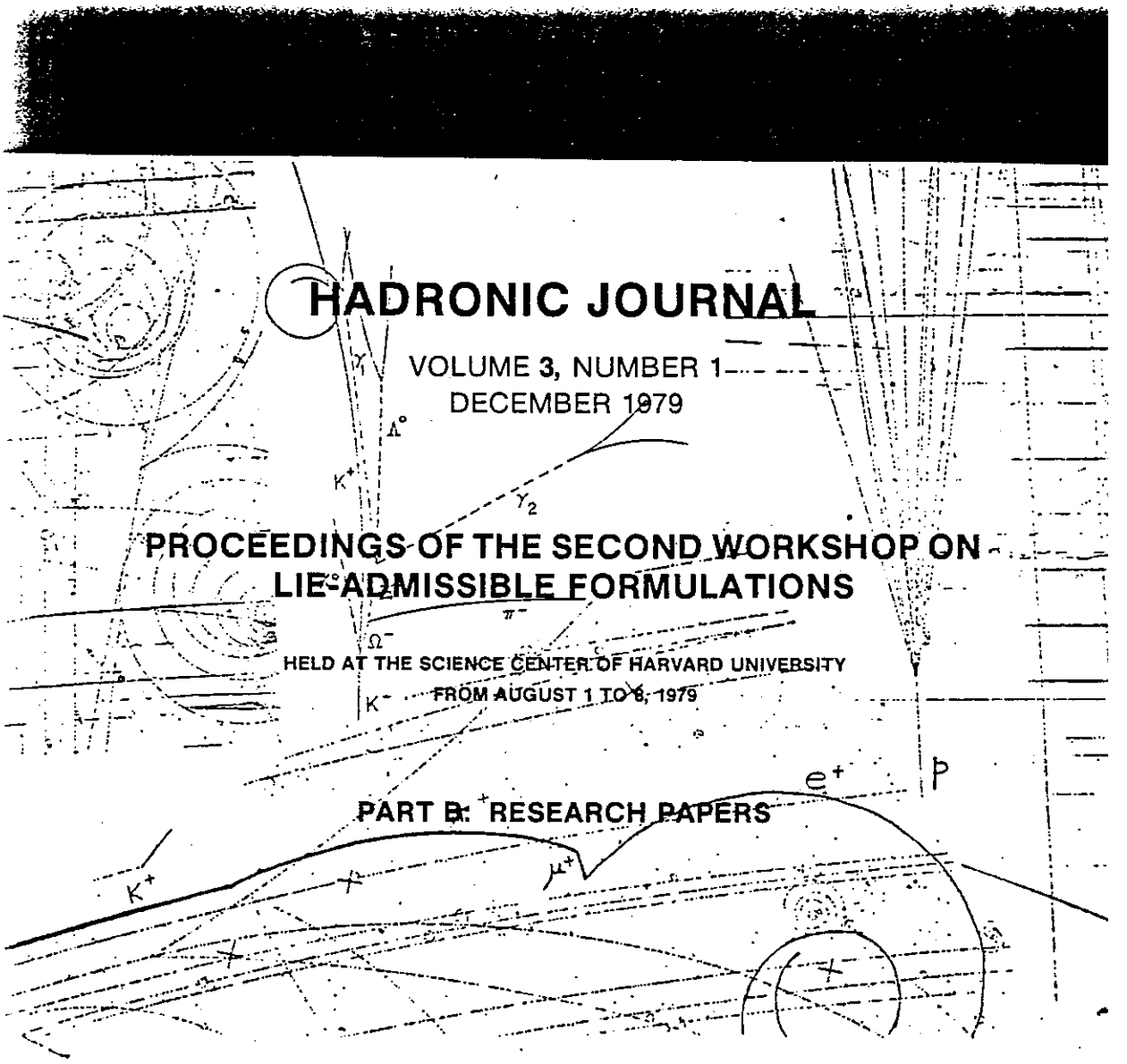
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HADRONIC JOURNAL

Volume 3, Number 1, December 1979

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HARVARD UNIVERSITY

OFFICE OF THE PRESIDENT

MASSACHUSETTS HALL
CAMBRIDGE, MASSACHUSETTS 02138

August 13, 1979

Dear Mr. Santilli:

This note is simply to acknowledge the copy of your letter to Professor Field and to return the supporting materials.

Best wishes,

Sincerely,

Derek C. Bok

Mr. Ruggero M. Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

CONFIDENTIAL

Sept. 4, 1979

Prof. H. HIRONAKA

Dear Heisuke,

I am in a delicate moment of my academic life. Your advice and, whether possible, help, would meet with my sincere gratitude and confidentiality.

As you know, my DOE grant expires on June 1, 1980. The DOE has verbally indicated to me the decision to continue the support of my studies. To keep continuity, I must apply sometime during this semester. In turn, this situation calls for the identification of my new institution within the next two months or so.

I have therefore initiate the search of an outside institution willing to administer my grant. However, this has immediately met with problems. For instance, a colleague put it to me: "If your salary is covered by your grant, why Harvard does not want to continue its administration?".

In conclusion, the transfer of my grant from Harvard to a new institution is resulting to be much more difficult than I expected.

Owing to this situation, I am also looking at the possibility whether there is any way according to which Harvard can administer my grant for the next two years (the second term). It is in this respect that your council would be appreciated. But, if you cannot advice me for any reason, simply ignore this letter.

I shall keep my promise of non-applying to the Dept. of Math. I am sincerely grateful for the hospitality I received for two years, but I am not a mathematician, nor I intend to become a mathematician. Besides, my studies are now converging not only on applied physical problems, but actually in the formulation of experiments. I do not see much of a compatibility with a Dept. of Math. and I shall, therefore, not apply (unless some senior member recommends me the contrary). It remains without saying that in these experimental studies there is considerable new mathematics (Lie-admissible algebras), yet unexplored at the mathematical level.

I did apply to the Center for Astrophysics on July 26, 1979. On August 1 I received the enclosed letter by Dr. Field in which, as you can see, my application was denied even consideration on grounds that the policy is

"to encourage research at the Center which is directly related to astrophysics and to resist the temptation to go into fields which are not directly related".

Permit me to express my great surprise when I received this letter. I simply could not believe to my eyes. My application was for what I am now doing, that is, theoretical and experimental studies on the problem of the validity or invalidity of conventional relativities within a hadron and, thus, within the interior problem of astrophysical objects.

page 2.

This research program, not only is "directly related to astrophysics", but, in the opinion of a growing number of physicists, verts on THE fundamental open problem of contemporary astrophysics. To be specific on this rather crucial point, my studies vert on the future, experimental resolution whether conventional geometries of current use in physics (i.e., of non-integral type) apply or not within hadronic matter, whether a hadron or a star. Until this problem is experimentally resolved, decades of studies at Field's Center, and millions of taxpayer's money risk to be spent in scientific hand-wavings.

Owing to this situation, I was expecting a normal consideration, jointly with other candidates, and then the customary rejection at the end of these formal processes already decided at the beginning .... I am confident you will now see the reason of my surprise. After all, since my application was in astrophysics,I have written papers in astrophysics, and I do qualify as an astrophysicist, the rejection by Field to even consider my application jointly with others might be, at the extreme, considered even against the Act for Equal Opportunity/Affirmative Action Employer.

Almost needless to say, I have absteined from bringing these aspects to Field's attention and I shall continue to do so. I answered with a moderate letter of August 10 in which I reassure him that "I understand and respect your position and that, on my part, I do not intend to pursue any more my association with your Center".

Despite this, my application to Field's Center for a fundamental open problem in astrophysics supported by the DOE, and Field's refusal to even consider the application,remain.

Could you kindly consider the possibility of contacting Dr. Field and finding the proper way of bringing to his attention the delicate aspects of the current situation?

Another possibility for me is to apply to

Professor Peter J. Huber, Department of Statistics, Science Center room 609

according to the opening announced in the August issue of the Notices of the AMS (see ecnlosed copy).

Indeed, part of my studies that will be intensified in the next two years consists of the possible construction of a generalization of the conventional statistics of current use in physics (based on Lie algebras) for the case of the strong interactions, via the Lie-admissible coverings of the Lie algebras. I believe that this too is a rather fundamental problem of contemporary statistics and, therefore, I believe that I do qualify for this opening as far as consideration is concerned.

page 3.

Nevertheless, I have absteined from applying, waiting your return from Japan, in the hope that I can consult with you and, jointly we can avoid the duplication of Field's episode.

Could you kindly consider the possibility of contacting Dr. Huber? Could you kindly advice me whether it is appropriate to apply to this opening ?

Again, I am not asking for money from Harvard. On the contrary, I would like to bring money, as I did in the past two years. I am simply looking for the possibility that Harvard administers my DOE grant for the next two years. This essentially calls for a nontenured, nonsalaried, terminal, nonteaching appointment at a division which is compatible with my research. The Center for Astrophysics is THE most compatible. The Department of Statistics is equally compatible, and so are others (e.g., Applied Sciences).

The primary reason why I have been looking for this possibility is related to the difficulties and questions of various nature I have encountered in my recent attempts to transfer this grant to a new institution. Also, I still hope that the opposition at Harvard against my studies will one day terminate.

I enclose for your convenience copies of my curriculum, as well as of informative material on my past teaching-research-editorial activities.

Hoping that I did not abuse of your courtesy, time and patience, I would like to take this opportunity to express again the sentiments of my sincere esteem and gratitude.

Yours, Sincerely

Ruggero

P.S. On the first week of August we had an excellent SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS. Half of the participants were pure mathematicians and half physicists. A number of participants were from abroad (Switzerland, France, Israel, W. Germany, Italy, Australia and the USSR, this including corresponding members who could not attend because of lack of funds for travel). I believe that we made rather crucial advances on both mathematical and physical grounds. The PROCEEDINGS of this workshop will be published in the December 1979 issue of the Hadronic Journal. I have asked to the publisher for a number of complimentary copies of this issue and I shall do my best to let you have a copy. It is expected to be available in late January 1980 and it will comprise some 12-14 articles.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

September 23, 1979

President DEREK BOK,
Harvard University

Dear Mr. President,

I am concerned for the implications of a recent administrative decision on my case and, until I am a member of this community, I feel obliged to express this concern to you. I am also under the impression that this decision is due to lack of adequate information. Therefore, I am taking the liberty of outlining here the most salient aspects, while I remain at your disposal for any additional information you might need.

As you know, I am a research associate at the Department of Mathematics and the recipient of a research grant from the Department of Energy which supports my salary as well as all my research and logistic expenses (including telephone, xeroxing, etc.). My appointment and grant expire on June 1, 1980. The DOE is apparently pleased with the scientific outcome of my studies and has the best intention to continue my grant. Please feel free to contact, if you so desire, the DOE officer in charge of my case, Dr. D. C. PEASLEE, tel. 301 353 3624.

To keep continuity of grant, I have to identify a qualified research institution willing to administer my grant, and in such a way that I can apply for a renewal before the end of this calendar year. Beginning with this past summer, I have therefore applied to numerous institutions in fields related to my studies, which are: High Energy Physics, Astrophysics, and Statistics. I am also considering institutions in Engineering, because of certain engineering applications of my methods that are already under way by independent researchers.

Here at Harvard I have applied only to Dr. FIELD with an application of July 26, 1979, for a position at the Center for Astrophysics. I have abstained from applying to other Divisions of your campus, such as for the opening in the Department of Statistics announced by Dr. P.J.HUBER in the August issue of the Notices of the American Mathematical Society, lacking advice by senior members.

A copy of my letter of application to Dr. FIELD was sent to you. As you can see, my application essentially consists of theoretical and experimental studies on the problem of the validity (according to some) or the invalidity (according to others) of basic, conventional, physical laws, geometries, relativities and statistics for the interior of a strongly interacting particle (a hadron) or, much equivalently, for the interior of a star. It is almost pointless to indicate that the validity of these conventional settings for the exterior problem (outside a hadron or a star) is unquestionable. The studies are strictly referred to the interior problem (inside a hadron or a star) where no meaningful experimental information is available at this time.

With my considerable surprise, on August 1, 1979 I received a letter from Dr. FIELD essentially indicating that my application had not been submitted to the customary faculty consideration, because it is the policy of the Center to consider only research which "is directly related to astrophysics". Since Dr. FIELD was informed that copy of my application had been sent to you, Dean ROSOVSKY and Associate Dean LEAHY, I am lead to conclude that the decision not to consider my application has been essentially reached by Harvard's administrators.

The purpose of this letter is to provide you with information related to this decision.

MY APPLICATIONS ELSEWHERE. Permit me to stress that I have applied and I am continuing to apply to several institutions. With a full research support and with a research program of rather fundamental physical character, I have no major problem in finding a receptive Institution, despite the typical aspects involved in any grant relocation. As a matter of fact, I have already received an offer. On my part, I therefore have no need to remain necessarily at Harvard. With full candor, permit me to say that the issue is whether it is in Harvard's interest to keep my research program or not, as I shall elaborate below.

THE DIRECT RELATIONSHOP OF MY STUDIES TO ASTROPHYSICS. As communicated to Dr. FIELD in my answer of August 10, 1979, I understand and respect his decision. However, I cannot accept his view that my studies are not directly related to astrophysics. But, again, I believe that this view is only the result of lack of adequate information.

In terms as simple as possible, all currently available astrophysical models, irrespective of their most advanced implementations (e.g., gauge) are based on the conjecture for the interior problem that the geometry is always locally Lorentz (that is, Einstein's special relativity applies in the neighborhood of a point of the local variables). My studies are essentially intended to subject this conjecture to experimental verification within a hadron. If the special relativity is invalid within a hadron, it will be consequentially invalidated in the interior of astrophysical bodies, because they are nothing but a collection of hadrons. In turn, this would call for a genuine, nonincremental, advancement in astrophysics, that is, the construction of coverings of current astrophysical models for the in terior problem, which has already been initiated by a number of researchers(see below).

Irrespective of personal theoretical views by individual researchers, I stressed in my answer to Dr. FIELD that a failure of the current theoretical views in astrophysics of the interior problem has been recently identified in rather visible terms and for one of the simplest possible interior systems: the motion of SKYLAB while within Earth's atmosphere. In essence, SKYLAB had rather complex forces (nonlocal forces approximated via polynomial expansions in the velocities) which cannot be all "geometrized" via current tools. In particular, the use of conventional geometries yielded only a point-like approximation of SKYLAB with only part of the actual forces.

I would like to stress that this view is not new. In the final analysis, this view is nothing but Cartan's point that the Riemannian geometry cannot recover the entirety of Newtonian Mechanics, but only that part compatible with Galilei's relativity (again, only geometrizable forces).

But there is a further aspect in which I disagree with Dr. FIELD, the separation between astrophysics and hadron physics implied in his decision. The gravitational field, in my view, literally originates in the interior of astrophysical bodies.At a deeper analysis, the interior of these bodies is not the conventionally used notion of mass (which is a technical expedient to avoid our ignorance on the problem of structure), but instead, a collection of fields. Among them, the strong fields (interactions) are expected to play a crucial role for the very initiation of the study of the "origin" of the gravitational field. For technical details, you might consult my article in Ann. Phys. 83, 108 (1974), where a model consisting of the (conventional) geometrical "identification" of the gravitational field in the interior problem with the structure fields is proposed (as a first step, prior to the availability of more suitable geometries), jointly with an explicit experimental proposal.

You should also be informed that I have proposed a generalization of Galilei's relativity for Newtonian systems with forces nonderivable from a potential (i.e., precisely those forces that are outside of Riemannian geometry). This proposal, presen ted in the Hadronic J. 1, 223 (1978) was the result of about a decade of prior studies devoted to the methods for the treatment of the broader forces considered. Even though predictably under examination by independent researchers, this generalized relativity is the first that provides a form-invariant description of Newtonian systems with arbitrary forces, while jointly characterizing the time rate of variation of physical quantities (par contre, Galilei's relativity provides a form-invariant description of only systems with conservative forces, and the characterization of the subcase of conserved quantities).

I subsequently used this generalized relativity, after working out an initial quantization, for the problem of structure of hadrons, resulting in the proposal of a new model of structure which is now under study by a number of researchers. I enclose to this effect a paper by Dr. Jiang from the People's Republic of China. The key point is, again, the realization of strong interactions with forces more general than those $f = \partial V/\partial r$ of current use in high energy physics and astrophysics. For technical details, you may consult my paper Hadronic J. 1, 574 (1978).

Currently, I am working on a relativistic extension of this relativity or, equivalently, a generalization of Einstein's special relativity for physical conditions of particles not treated in the books, e.g., the motion of the extended proton (radius $\cong 10^{-13}$ cm) within dense hadronic matter, such as the core of a neutron star, and expected, consequential, nongeometrizable forces. The gravitational implications of this step are crucial. I am, therefore, working in astrophysics.

In conclusion, and against the view of numerous colleagues, I do not see a qualitative difference between the problem of structure of the lighest known hadron, the π^0, and that of the most massive star. The only difference I see is a matter of size. This is the reason why I cannot accept a separation between hadron physics and astrophysics. In any case, an interplay between these two profiles of the same physical reality can only be beneficial to both.

THE HISTORICAL REASONS OF DOUBTS. By no means my doubts on the validity of conventional theoretical tools of contemporary physics for the interior of hadronic matter are new. On the contrary, I have simply followed the teaching, not of my contemporary colleagues, but of the Founding Fathers of contemporary physics. I quoted earlier Cartan. Permit me to recall that are numerous, additional, authoritative, historical voices of doubts.

(1) Fermi made it quite clear in his "Lectures in Nuclear Physics" in relation to the strong interactions and their short range that "there are doubts as to whether the usual concepts of geometry hold for such small regions of space". Notice that doubts on conventional geometry imply doubts on conventional relativity and quantum mechanical laws. After a considerable search and inquiry among his colleagues, I have come to the reconstruction that Fermi expected the strong interactions to be nonderivable from a potential (exactly the nongeometrizable forces of Cartan's point), as one way to differentiate them from the electromagnetic interactions, by therefore implyin g the possible existence of generalized formulations. On my part, I have taken seriously Fermi's teaching, and my studies on the problem of the structure of hadrons indicated earlier are based on this point by Fermi. Par contre, quark believers realize the strong forces via the simplistic structure $f = \partial V/\partial r$ up to their most advanced formulations (e.g. quantumchromodynamics), without a genuine critical inspection, and in the apparent disregard of Fermi teaching.

(2) Pauli clearly indicated in his historical lectures and papers that his exclusion principle (a pillar of contemporary quantum mechanics) had been conceived for the lack of overlapping of the wave packets (the atomic structure). Again, I have taken seriously this teaching by Pauli. Indeed, most of my efforts are centered on the experimental verification of the validity or invalidity of Pauli's principle under the conditions of overlapping of the wave packets (see below) which are necessary for the constituents of hadrons. Par contre, quark believers apply rather lightly Pauli's principle as a pillar of their views, without a genuine critical examination, and in the apparent disregard of Pauli's teaching.

(3) Einstein's doubts on quantum mechanics, and Heisenberg's undeterminacy principle (another pillar of quantum mechanics) in particular, are famuous. He believed in the good approximation that this mechanics provides for the description of the electron's orbits in atoms. But he refused to believe up to his death in the final physical character of this theory. Incidentally, Einstein's doubts are, in my view, most touchingly and effectively reported in Heisenberg's memoires "Physics and Beyond". On my part, I have taken seriously Einstein's doubts. Indeed, my efforts

are also verted toward a theoretical proof that Heisenberg's undeterminacy principle is inapplicable under the conditions of overlapping of the wave packets, as well as toward the experimental resolution of the issue. Par contre, quark believers rather lightly assume Heisenberg's principle without a genuine critical inspection for the necessary conditions of overlapping of the wave packets of the hadronic constituents, and in the by now known disregard of Einstein's teaching.

(4) Jordan's doubts on the lack of terminal character of quantum mechanics and quantum statistics are also well known. These doubts resulted in the creation of a new (nonassociative) algebra known as Jordan algebra. In his joint celebrated paper of 1931 with von Neumann and Wigner, he put it quite clearly that "the need of such a generalization arises from the (probably) fundamental difficulties resulting when one attempts to apply quantum mechanics to questions in relativistic and nuclear phenomena". I have taken quite seriously the teaching by these three Founders of contemporary physics. Indeed, on algebraic grounds, my efforts are based on Jordan's view of generalizing the algebraic structure of quantum mechanics. In fact, my generalization of Galilei's relativity and my methods of quantization are based on more general algebras which are jointly Lie-admissible, as well as Jordan-admissible (thus, including Jordan's approach), in order to accomodate forces more general than $f = -\partial V/\partial r$. Par contre, quark believers preserve in its entirety the conventional algebraic structure of quantum mechanics, without a genuine critical inspection, and in apparent disregard of the teaching of Masters such as Jordan, von Neumann and Wigner, with very few exceptions (I am referring here to recent uses of the M_3^8 Jordan algebras for quark oriented models that have received rather moderate receptions within the same circle of quark believers).

(5) Wigner himself has clearly expressed in his memoir "Symmetry and reflections", and for different technical reasons, the statement: "let us not forget, the problem of interactions is still a mistery."

My list of historical reasons of doubts by authoritative voices could continue....

THE ROLE OF THE HADRONIC JOURNAL. When I decided to organize this new Journal in January of 1978 the situation was essentially the following. We had numerous authoritative and seemingly uncorrelated doubts on the validity of conventional views for the strong interactions, but we had:
(a) no comprehensive analysis on the reasons for such authoritative doubts;
(b) no organized initiation of the studies for their experimental resolution ; and
(c) no effective beginning of the studies aiming at the construction of possible generalized formulations, specifically conceived for the strong interactions.

You should be informed that, beginning from the very first issue of April 1978, and thanks to the participation of scientists with genuine vision from virtually all developed Countries, the HADRONIC JOURNAL has made genuine contributions on these rather fundamental physical problems.

Today we do have an initial, but comprehensive analysis, linking rather deeply the seemingly independent doubts by Fermi, Pauli, Einstein, Jordan, von Neumann, Wigner and others. In essence, we have identified the property that, under the conditions of overlapping of the wave packets, we have nonlocal forces nonderivable from a potential which imply the inapplicability of conventional (symplectic and Riemannian) geometries (FERMI POINT). In turn, these forces, even under local approximation nonderivable from a potential, imply the breaking of the spin symmetry of particles and, thus, the inapplicability of Pauli's principle (PAULI POINT). Still in turn, these broader forces imply a departure from Heisenberg's principle (EINSTEIN POINT). Still in turn, in order to be algebraically treated, these broader forces call for a generalization of the associative enveloping algebra of quantum mechanics into a nonassociative form (JORDAN POINT). Still in turn, these broader forces imply rather fundamental, open problems for the strong interactions (WIGNER POINT). And so on. In conclusions, all the doubts by the Founding Fathers of contemporary physics turned out to be deeply and intriguingly related.

Today we do have initial formulations of experiments. I am referring here to:

- My proposal in the Hadronic J., 1, 574 (1978), subsequently elaborated in a number of articles, to test Pauli's exclusion principle in nuclear physics. At this level, we have experimentally established, statistically small conditions of overlapping of the wave packets of the nucleons. Our methods then predict a statistically small deviation from the exact validity of Pauli's principle in atomic physics, which may have well escaped existing inspection, much along the (quantitatively similar) historical case of the discovery of the violation of discrete symmetries by LEE and YANG. The following aspects of this experimental proposal may have a relevance for your consideration of my case. First, the mechanism of inapplicability of Pauli's principle which is conjectured in nuclear physics is based on the breaking of the central part of Einstein's special relativity, the so-called SU(2)-spin part. Second, a small deviation from Pauli's principle in nuclear physics necessarily implies a bigger departure at the level of the structure of the hadrons (because of the higher state of overlapping of the wave packets), and an even bigger departure at the astrophysical level (because of the extremely high pressures and densities of hadronic matter). You should therefore be fully informed that this experiment is the first step toward the proof or disproof of a fundamental conjecture of contemporary astrophysics, that the geometries for the interior problem are Lorentz in local character.
- Kim's proposal in the Hadronic J. 1, 1343 (1978) to measure more accurately the mean life of unstable hadrons in flight. If the structure forces are more general than those currently believed, we have a deviation of the quantities predicted by Einstein' special relativity. This experiment too is, therefore, of direct astrophysical relevance and can be useful for the future resolution of the fundamental assumption of contemporary astrophysics indicated earlier, although from a complementary profile.

You should also be informed that numerous other experimental tests are under study.

Finally, we do have today a coordinated effort aiming at the construction of generalized formulations specifically conceived for the strong interactions, ranking the participation of senior mathematicians of the caliber of Tomber, as well as senior physicists of the caliber of Okubo, including Nobel Laureate Prigogine who received the Nobel price for his statistical studies on forces nonderivable from a potential. In particular, I am conducting my search of maturity in these studies via

(1) two series of research monographs on the methods for the treatment of forces nonderivable from a potential, one with Springer-Verlag under the title "Foundations of Theoretical Physics" (Volume I was published in 1978, Vol. II is in press, and Vol. III is scheduled for 1982), and one with the Hadronic Press under the title "Lie-Admissible Approach to the Hadronic Structure" (Vol. I was published in 1978, Vol. II is in press, and Vol. III is scheduled for 1980).

(2) A series of articles I have written or are under consideration in various Journals (Physical Review D, Foundations of Physics and the Hadronic Journal), either alone, or in collaboration with Professors Myung (from the USA), Kobussen (from the Institut für Theoretische Physik of Zürich), Ktorides (from Greece), Fronteau and Tellez-Arenas (from France) and Eliezer and his group (from Australia).

(3) An intensive effort of consultation with receptive colleagues of either mathematical or physical or experimental orientation in the various aspects of the problem.

(4) The coordination of efforts by independent researchers for articles in the Hadronic Journal; and

(5) A yearly congress, specifically devoted to these studies, called "Workshop on Lie-admissible formulations". The first was held in August 1978 and resulted in a number of papers. The second was recently held in August 1979 with the participation of mathematicians and physicists from the USA, France, Switzerland, Belgium, and Israel, as well as contributing participants (who could not attend for various reasons, but are sending in their contributions) from the USSR, and the People's Republic of China. The Proceedings will be published in January, 1980.

You are aware from preceding contacts of my determination to pursue these studies, at whatever personal sacrifice. In essence, I decided long ago to pursue knowledge in physics, rather than a career in physics, by being fully aware that this is more than often unrewarding.

THE SCIENTIFIC IMPLICATIONS. I am under the impression that Harvard administrators, again, I believe, primarily for lack of adequate information on my studies, are not actually aware of the implications of these studies. Permit me the liberty to candidly stress that we are here facing a coordinated and organized effort by numerous mathematicians and physicists to generalize the very foundations of contemporary physics.

In High Energy Physics we are feverishly working at the generalization of the fundamental tools, such as the relativity and the quantum mechanics, for the forces nonderivable from a potential, representative, as indicated earlier, of conditions of overlapping of the wave packets of particles. Even though available generalized tools are still at a rudimentary level (after all, Galilei's relativity was conceived circa 1638!), they have already been useful to identify the next technical problems to be solved, and they have already produced an alternative model to hadrons structure which, even though ignored by quark-committed physicists, is under active study by numerous young minds in various Countries (see the enclosed paper by Dr. Jiang from China).

In Statistics we have already identified a generalization of conventional settings (technically possessing a Lie-admissible algebraic structure, rather than the conventionally used Lie algebra structure) of course, under the presence of forces more general than $f = -\partial V/\partial r$. Also, a potential inapplicability of Pauli's principle under strong interactions has already stimulated the study of a generalization (of Lie-admissible character) of the fundamental statistics of contemporary particle physics, the Fermi-Dirac and Einstein-Bose statistics. A paper by an independent researcher is forthcoming in the Hadronic Journal on this crucial topic, and I will be glad to publish it whenever it reaches maturity.

In Astrophysics the possible inapplicability of the local Lorentz character of the geometries for the interior problem has already stimulated the study of generalized geometries by independent mathematicians (it is a geometry tentatively called symplectic-admissible and which is capable of accomodating the most general forces known at this time, the integrodifferential-nonlocal forces, rather than the limited capability of the Riemannian geometry of accomodating physical forces of nature). A critical inspection and assessement of Einstein's theory for the interior problem is under way by an independent researcher , expert in the field, and I will be glad to publish it in the Hadronic Journal whenever it achieves maturity.

We do not claim that our efforts are necessarily on the right track. All advances in physics are the result of a historically laborious process of trial and error. Also, in case our direction will be eventually proved via experiments to be valid, we do not claim that our approach has a terminal character. I firmly believe that Theoretical Physics will never achieve terminal descriptions. We are only particdpants and spectators (in Heisenberg's words) of a continuing scientific process.

Nevertheless, I believe that studies of such fundamental physical character should deserve a serious consideration, not only at the scientific level, but also at the administrative level. You are aware of the rather strong opposition I have found here at Harvard by quark-committed, senior colleagues, against the conduction of these studies. What I would like to add here is that these studies are now in motion on a world wide scale; the participation by mathematicians and physicists with a genuine vision is increasing litterally on a monthly basis; and these studies by now will be continued irrespective of whether I remain at Harvard or not, and whether I continue my contribution or not. What I am attempting to say here is that you should be fully informed of the scientific drive and momentum toward the experimental resolutions of theoretical beliefs, whether mine or those by others.

THE POLITICAL IMPLICATIONS. A final aspect I feel obliged to bring to your personal attention is that, jointly with the scientific drive indicated earlier, a political drive at a number of administrative levels has been initiated, and it is now in motion. I would like to stress that this drive is substantially out of my control, and I try to abstein myself as much as I possibly can. I am simply not a politician, as proved by the candor of my writings. I am only interested in doing physics, and it is for me reason of particular regret when forced into the consideration of this profile.

The passwords of this political action are: SCIENTIFIC ACCOUNTABILITY.

B illions of dollars of taxpayers money are annually spent in this Country in research either directly or indirectly related to the strong interactions. Most of these public funds are nowaday spent in research conducted and based on the tacit, unquestioned, belief of the validity of conventional laws, geometries and statistics for the physical arena considered. This is certainly admissible on scientific grounds, and I personally favor the condution of these studies, as explicitly stated in my papers.

Nevertheless, the political action under consideration calls for
- the continuation of the study and funding of research in strong interactions based on the validity of conventional theoretical views, but
- jointly, the conduction and funding of theoretical and experimental studies on their possible invalidity, as well as generalization, to achieve a well balanced use of public funds.

If you meditate a little on the scientific scene emerging from this report, you will find the consideration of this point inessential. During the past two years we had a rather intensive promotion of a moment of reflection on the validity of conventional, basic laws for the strong interactions, backed by numerous, historical, authoritative voices. This action had to imply, sooner or later, a call on the scientific accountability.

Also, you should be informed that this political action has been and is being aggravated by the rather caparbi ous opposition by physicists academically and financially committed to quarks. Most regrettable is the complete silence, to my best knowledge, in the quark literature on the very existence of these problems at the level of the fundamental laws.

You should be informed also that, to my knowledge, this political action has been aggravated by the following additional aspect. The problem of the basic physical laws for the strong interactions is expected to have a deep (if not crucial) impact on energy related issues, with particular reference to the problem of the controlled fusion. After all, this fusion is nothing but a laboratory construction of bound states of nucleons. As such, the basic laws do have a primary role. Regrettably, this energy connection has been dismissed as non-sense by a number of quark-committed physists, to the best of my understanding and reconstruction. But this simply invites a process to the scientific accountability of these physicists, to the detriment of their supporting institutions.

THE IMPLICATIONS OF YOUR DECISION. At this moment, and in the view of outside observers, the scientific situation at Harvard in the sector is well balanced. It is true that the majority of funds and research is being devoted to the use of conventional laws, geometries and statistics. Nevertheless, you house the originator of the studies on their experimental finalization.

Your decision not to allow the continuation of my research here has the direct effect of producing, in my view, a significant scientific unbalance in your campus.

This letter will achieve its objective if Harvard's administrators will reach a final decision on my case, first of all, according to standard practices (an in depth assessement of the scientific merits of my studies), but also with due consideration of the implications for a possible unsubstantiated negative decision.

On my part, I can only confirm what indicated previously to you, Dr. Field and others, that whatever your final decision will be, you can count on my best understanding, as well as my best loyalty, as an expression of my appreciation for the hospitality received.

Very Truly Yours

Ruggero Maria Santilli

c.c.: Drs. ROSOVSKY, LEAHY and FIELD.
enclosures.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 1, 1979

Professor R. M. Santilli
Department of Mathematics
Harvard University

Dear Professor Santilli:

After having read your letter of September 4, 1979 and talking with you, I now feel that I should make clear my opinion about your trying to continue your association with Harvard University. My opinion is that you can conduct your research much more efficiently and comfortably at some other institution than Harvard. I do not believe the transfer of your grant from Harvard to a new institution will create any difficulty either from the point of view of formality or from the point of view of the continuation of your own research activity. You asked me to contact Dr. Field at the Center for Astrophysics, but I can not believe that I can be of any service in persuading him or his colleagues to change his or their decision. The reason is simply that my understanding of your work is completely inadequate to make any reliable recommendation. The same applies to the Department of Statistics at Harvard. Simply, I am making neither any recommendation to either one of the two nor to any other department at Harvard.

I'm glad that you are keeping your promise of not applying to the Department of Mathematics. You should be aware of the departmental decision made during the last academic year that your appointment as Research Associate in our Department shall terminate at the end of May, 1980, irrespective of whether your D.O.E. grant is to be renewed or not.

I understand that you have an offer from some outside institution. I'm glad that you're making maximum effort to find a new institution in which to continue your line of research and I wish you the best success in your future career wherever you go.

Sincerely yours,

Heisuke Hironaka

Heisuke Hironaka
Chairman

HH/mjm

Center for Astrophysics
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

October 2, 1979

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli,

Thank you for your letter of September 26. I read it with interest.

Sincerely,

George B. Field

GBF/ljc

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

October 2, 1979

Professor HEISUKE HIRONAKA,
Department of Mathematics

Dear Heisuke,

I would like to thank you for your letter of October 1, and reassure you that I am in full agreement with its contents. In particular, I am in full agreement with your view that I can conduct my research "much more efficiently and comfortably at some other institution than Harvard". Permit me to express my reasons for such a view (which may be different than yours).

You are familiar with my episode at Lyman. When, on September 1, 1977 I joined the Lyman Laboratory as "honorary research fellow" for the academic year 1977-1978, I was unemployed with a family of four to support and my wife at the graduate school. In particular, I was receiving unemployment benefits from the town of Newton, the office in Center Street. Despite my considerable experience and a nation wide search, I had simply been unable to find an academic job. Subsequently, I was authorized to file a research grant application with Shlomo, with Lyman being my affiliation. The application was accepted and the grant was sent to Harvard for signature on March 1978. I therefore applied at Lyman for: (A) the removal of the term "honorary" from my title for the remaining part of the appointment, so that I could draw a salary (according to Harvard's rules, a person with the term "honorary" in its title cannot receive salary); and (B) the consideration of the extension of my non-tenured, non-teachin g, terminal, research associate position for the duration of the grant (until June 1, 1979). I insisted repeatedly that I was primary interested in the removal of the term "honorary" for the remaining part of the appointment, so that I could draw a salary. The question for a possible extension was secondary for me. At that time, all my unemployement benefits had terminated (there is a maximum of 33 weeks in Massachusetts) and all my savings had terminated too. After a number of months of deliberation, the senior faculty at Lyman decided, on June 19, 1978, against the removal of the term "honorary" from my title, as well as against my appointment for the duration of the grant as research associate. In this way, I was forced into the rather paradoxical situation whereby I remained at Lyman until the end of the term, while I was prohibited to draw a salary from my grant, and while I was unemployed with two cildren to feed, without unemployment benefits, nor savings, nor the capability to look for a manual job to avoid conflicts with my federal grant. As you know, this situation was resolved thanks to the invaluable help by Dean Leahy.

You are also familiar how close we were to a duplication of the Lyman episode at your department. I am sure you realize that, after an experience of this type, I took all the initiatives to leave Harvard. Nevertheless, Shlomo reapplied for a one year extension of the grant, with my affiliation being this time the department of mathematics. The application was soon funded, and I therefore informed my outside contacts that I was not interested for 1979-1980. Nevertheless, in early April 1979 you informed me that the senior faculty had decided against the renewal of my terminal, one-year, research associate position covered by this already funded grant. As you recall, you further indicated to me that you saw "no possibility" of overcoming the situation. I am sure you realize what this decision meant to me: back to unemployment. Indeed, I saw no practical way of resolving the entanglements for a relocation of the grant under the circumstances. Also, in April all (the very few) openings in the academic world had been filled. The subsequent events are familiar to you. The impasse was resolved thanks, this time, to your invaluable help.

You should be informed that these occurrences have stimulated a predictable distorsion of my scientific programs. My article in the Hadronic Journal 1, 574 (1978) entitled "Need of subjecting to an experimental verification the valdity within a hadron of Einstein's special relativity and Pauli's exclusion principle" was released for publication on June 19, 1979, the day of the negative final decision on my case at Lyman. As you know, the article consists of a detailed critical analysis (for some 337 pages) of the validity of conventional laws for the strong interactions, that is, the basic laws used in the current quark conjectures. I had no plans at that time of publishing this article, nor I would have published it in case of my association with Lyman. Acually, after having spent a number of years of study of the topic, I was planning to spend a number of additional years to reach a maturity commensurate to the topic.

Similarly, my draft entitled "An intriguing legacy by Albert Einstein: the expected invalidation of quark conjectures" was written, as you know, after your communication in early April 1979 on the negative decision of my case at your department. As you also know, at that time I was deeply involved in completing my volume II with Springer-Verlag and I had no plans nor intention of writing this article. The article (distributed in some 15,000 copies) was this time a direct critical examination of the quark conjectures and presented only part of the reasons why, after studying these models for over a decade, I do not accept them. Nevertheless, the spirit of the article was not negative with respect to quark-oriented studies. All physical studies are valuable, even when they are proved as wrong. Physics is an approximation of reality. It is not an exact science such as mathematics. As such, quark models definitely provide a good approximation of the hadron ic world, whether quarks exist or are a mere imagination in the mind of physicists. The spirit of the article was different. Although not explicitly stated, the spirit of the article was to draw a moment of reflection on the politics surrounding quark conjectures. This spirit has been understood by a number of qualified readers, as attested by rather numerous letters and phone calls of support I received, some of them from quark-committed physicists. In short, it appears that the problems of our community, rather than being of scientific character, appears to be more of human character.

In conclusion, when faced with the rather unusual episodes I have recalled early, I selected the MINIMAL POSSIBLE REACTION: an aggressive scientific initiative, combined with the utmost confidentiality on my episodes here vis-a-vis with the outside world. You know that I have kept this confidentiality, and you can be confident of my mature attitude in the future. I am sure you realize that I could not have possibly remained totally passive. I am also confident you realize that no senior member here in true control of his mental capacity was expecting that I remained totally pass ve.

I hope you will now see that I am indeed actively looking for an outside department which, after authorizing me to file a research grant application, HONORS ITS COMMITTMENT AND DOES NOT SUBSEQUENTLY CHANGE MIND. I need this type of rather straighforward association, first of all, for my peace of mind, and, secondly, to prevent completely unnecessary, further scientific distorsions. You can therefore rest assured that I shall not apply to the department of mathematics here, nor at any other department, nor I shall seek, on my part, the association with the Center of Astrophysics. Nevertheless, you should not expect that my grant relocation will be easy.

I am also under the impression that my letter of September 24, 1979 has been misinterpreted (which does not surprises me owing to similar previous episodes). My sincere and only desire was with this letter to provide elements to Harvard administrators to keep themselves informed in a rapidly changing scientific scene, in a rather crucial sector of research, involving considerable amounts of taxpayers money. I am fully aware of the difficulties of their jobs, as well as their responsibilities. But this demands inside knowledge. I simply felt that, until I am a member of this community, I should provide this knowledge for whatever its value is. It appears that I was wrong in doing this, and you can rest assured that I see no reason for further disclosures in the future.

In closing, permit me to take this opportunity to confirm my sincere gratitude to you, as well as my sincere esteem in you as a person as well as a scientist. I appreciated your good wishes for a new job and, wherever I will be, I will remember you always with pleasure. Hoping that this letter will close the case, I remain

Sincerely Yours

Ruggero Maria Santilli

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Dr. GEORGE B. FIELD
Center for Astrophysics
Harvard University

Dear Dr. Field,

I appreciated your note of October 2. I enclose "Chart 4.9" of my volume II of "Foundations of Theoretical Mechanics" with Springer-Verlag, now in press, dealing with the doubts on conventional geometries and laws for the interior problem, as well as with the historical voices of doubts (see part 9 of this chart, pp. 343-349). Perhaps you will prefer the conventional scientific language of this presentation (as compared to the informal language of my note to ANGAS HURST). Also, please note the formal acknowledgment to Lyman in p. 19.

Please take into account that the chart is written for the audience intended for my volumes, that is, graduate students or researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical treatment of the subject is presented elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be distributed in early 1980.

The key technical point is the no-go theorem of quantization via conventional approaches of the (pre)symplectic geometry, outlined in pages 309-315 (Part 6), and technically treated in the workshop. I believe that this no-go theorem should be brought to the attention of astrophysicists who are not aware of it (it is little known in physical circles).

You might be interested to know that my paper "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" has been accepted for publication in FOUNDATIONS OF PHYSICS. I am currently working in reaching the best possible scientific maturity of presentation with the assistance of a number of colleagues.

Sincerely,

RMS/ml

Ruggero Maria Santilli

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

October 10, 1979

Professor FRED L. WHIPPLE,
Center for Astrophysics
Harvard University

Dear Professor Whipple,

I would like to thank you for your consideration in regards to the editorial finalization of my paper "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures", and for the courtesy of returning to me the material prior to your vacation.

You might be interested to know that the paper has been accepted for publication in FOUNDATIONS OF PHYSICS, although I am still working to achieve the best possible maturity of presentation with the assistance of a number of colleagues.

These studies are acquiring more and more an astrophysical implication and flavor. In case you are interested, I enclose copy of the "Chart 4.9" of my volume II of "Foundations of Theoretical Mechanics" with Springer-Verlag, now in press. This chart essentially presents an outline of the doubts on the validity of conventional laws and geometries for the interior problem (only), whether a hadron or a star. The chart also recalls the authoritative, historical,reasons of doubts ("legacies", in my language) by Einstein, Fermi, Jordan, and other founders of contemporary physics, that have been largely ignored by contemporary physicists.

Please keep into acc ount that this chart is intended for graduate students and researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization. The technical presentation is elsewhere and, in particular , in the proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, we held here at Harvard from August 1 to 7, 1979, with the particopation of mathematicians and physicists from the USA, France, Switzerland, France, Belgium, and Israel, and with corresponding participants from the USSR and from the People's Republic of China. The proceedings will be distributed in early 1980.

The astrophysical implication which transpared quite clearly at this workshop is a doubt on the conventional assumption that the geometries of the interior problem are Lorentz in local character. In essence, we considered a proton within the core of a star. We abandoned point-

like abstractions of this particle and consider it as it actually is in nature: an extended object with an extended wave packet. The high pressures and densities of the star force this wave packet to penetrate within those of the surronding particles. This results in forces more general than the simplistic $f = -\partial V/\partial r$, and in a dynamics which is fundamentally different than that conceived by Einstein for the special relativity (motion of the particle in vacuum under the electromagnetic interactions). The implications of these broader forces have been studied to a preliminary, but detailed extent. It essentially emerges that, under these broader forces resulting from the state of penetration of the proton within hadronic matter, not only the Poincaré symmetry is broken, but actually the breaking occurs at the level of its central part: the SU(2)-spin symmetry.

We are feverishly working at a generalization of conventional approaches via what we call the "symplectic-admissible" geometry (you might also call it a "Riemannian-admissible geometry"). This geometry was identified at the workshop as capable of representing the largest forces known at this time: the variationally nonselfadjoint, integrodifferential forces (superposition of local and nonlocal forces derivable and nonderivable from a potential).

By comparison, the Riemannian geometry, including its graded or gauge extensions, can accomodate a truly limited class of forces of nature. This point, of course, is not new. You will recall that Cartan made it quite clear that the Riemannian Geometry does not recover all of Newtonian Mechanics, but only that part with "geometrizable" forces (essentially that compatible with Galilei's relativity).

Irrespective of personal theoretical views, you will be amused that a visible example of the "collapse" of conventional geometries for the interior problem has been recently identified in Space Mechanics: SKYLAB had highly nongeometrizable forces. The Riemannian geometry essentially produced a point-like abstraction of this simplest possible interior system. This situation persists also for other systems, such as spinning tops, provided that one does not fall into the trap of representing them via Galilei's relativity (which would imply the perpetual motion), but treats them as they actually are in nature: extended rotational bodies in a resistive medium with drag torques for which the conventional SU(2) symmetry is meaningless (it would lead again to the perpetual motion). Apart technical profiles, this is conceptually our view of a proton within a star, and its possible departure from a local Lorentz character.

You might also be interested that we are feverishly promoting the experimental finalization of this situation, either in favor or against conventional views. In particular , we have experimental proposals at the nuclear level which, even though predictably delicate, are

feasible with current technology. At this nuclear level we expect the possibility of very small deviations (because the condition of overlapping is here very small), which might have escaped inspection simply because not looked for. It is understood that these nuclear experiments have a direct astrophysical inspiration, conception and technical formulation. Again, they are conceived to test whether the geometry is SU(2) as well as SL(2.c) of local character or not.

In case you are interested in being kept informed of these studies, please let me know. I shall instruct my secretary to mail you selected, relevant,material.

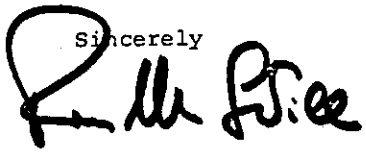
The spirit of these studies can be best expressed via Heisenberg's words ("Physics and Beyond", p. 70)

> "In science, it is impossible to open up new territory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hazardous leap forward."

To which he adds immediately after:

> "However, when it comes to entering new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do."

Sincerely



Ruggero Maria Santilli

RMS/ml
encls

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Professor RICCARDO GIACCONI
Center for Astrophysics
Harvard University

Dear Riccardo,

I am taking the liberty of keeping you informed of our progresses, because they appear to have a deeper and deeper astrophysical flavor and implication.

I enclose copy of the "Chart 4.9" of my volume II with Springer-Verlag entitled"Foundations of Theoretical Mechanics", now in print. This chart outlines the reasons of doubt for the conventional geometries and laws with regard to the interior problem, whether that of a hadron or a star. The chart also presents an outline of the hystorical voices of doubts on the subject by Einstein, Fermi, Jordan, and others (you may see part 9, pages 343-349).

Please take into account that the presentation of this chart is that for the intended level of the audience, graduate students and researchers without a technical knowledge of symplectic quantization and of the broader Lie-admissible quantization.

The technical presentation is elsewhere and, in particular, in the proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS held here at Harvard from August 1 to 7, 1979, with the participation of mathematicians and physicists from the USA, France, Switzerland, Belgium and Israel, as well as corresponding participants from the USSR and the People's Republic of China. The proceedings will be distributed in early 1980.

In non-technical terms, the implications which clearly transpared at this workshop for astrophysics are the following. What appears to be in doubt is the rather crucial assumption of current models for the interior problem (only) according to which all admissible geometries are Lorentz in local character. We have conducted a preliminary, but extensive and detailed analysis of the dynamical behaviour of a particle (say, a proton) within hadronic matter (say, the core of a neutron star). Once point-like astractions are abandoned, and the particle is faced as it actually is in nature, an extended object

possessing a wave packets of finite size, the physical picture appears to be different than conventional ones. The high pressures and densities of the core of a star force this wave packet to be in a state of penetration with those of surrounding particles. This results in generalized forces which break, to our understanding, not only the special relativity, but actually its central part: the SU(2)-spin symmetry, along much of the Newtonian breaking of the SU(2) symmetry of the spinning top in our environment which is necessary to avoid perpetual-type motions. A feverish activity is going on for the technical construction of covering notions via the Lie-admissible algebras, and the SU(2)-admissible covering of the SU(2)-spin algebra. The SU(2)-symmetry breaking forces are not geometrizable via conventional Riemannian approaches as well as conventional graded-gauge extensions of current consideration. This is not new. Cartan stated quite clearly that the Riemannian geometry does not recover the Newtonian Mechanics, but only that (minute) part with "geometrizable" (à la Riemann) forces (essentially that compatible with Galilei's relativity). A visible example of the "collapse" of contemporary views in the interior problem has been recently identified by NASA: SKYLAB had highly non-geometrizable forces (polynomial expansions in the velocities of nonself-adjoint character). The Riemannian geometry produces only a point-like abstraction for this simplest possible interior system.

You might be interested to know that I am feverishly working in astrophysics, but, by specific intent, not that conventionally done. In essence, I am working at a covering of Einstein's ideas for the dynamical conditions of an extended particle within the core of a star. The geometry I am using is, what we call, of symplectic-admissible type (you might call it also of "Riemannian-admissible" type). This geometry was proved at the recent workshop to be able to accomodate the largest forces known at this time: the variationally nonselfadjoint, integrodifferential forces (superposition of local and nonlocal forces derivable and not derrvable from a potential), as compared to the truly limited capability of the conventionally used geometries for the representation of the forces of nature. This work is, in essence, a "relativistic" extension of the Symplectic-admissible covering of Galilei's relativity I proposed in the Hadronic J. 1. 224 (1978), subsequently worked out and expanded by a number of independent authors.

A feverish promotional activity is also going on in the hope that experimentalists will finally put the machines in motion. We have the formulation of experiments at the nuclear level which are apparently feasable with current technonogy. These proposals predict very small deviations at the nuclear level (because of the very small condition of overlapping of the wave packets at this level). But, they would be sufficient to indicate larger deviations at the hadronic and the astrophysics levels. Again, the mechanics of these nuclear experiments is specifically intended to test the validity of invalidity of the conjecture that the geometries of the interior gravitational problem is Lorentz in local character.

page 3.

The technical ground of these expected deviations is essentially set by a no-go theorem of quantization via the conventional approaches of the (pre)symplectic geometry, which has been recently proved by mathematicians without any idea of the physical implications, particularly in gravitations. This theorem (outlined in Part 6 of the enclosed chart) appears to be little known in physics (and astrophysics) circles.

In case you are interested to be kept informed of these theoretical, and, now, experimental efforts, please let me know. I shall put you in our mailing list.

My paper (you eventually received in April 1979) entitled "An intriguing legacy by Albert Einstein: the expossible invalidation of quark conjectures" has been accepted for publication in FOUNDATIONS OF PHYSICS, although I am currently working on achieving the best possible maturity of presentation.

You might also be interested to know that in August I applied to Dr. FIELD to join your Center. But I received soon after an answer that the Center is interested only in research "directly related to astrophysics". In particular, my application was not for money. As a matter of fact, I bring money, in the sense that I am fully supported via my grant with the DEPARTMENT OF ENERGY. I simply asked for the possibility of reapplying for the renewal of this grant that expires on June 1, 1980, either alone, or in collaboration with senior, interested,colleagues.

I answered to Dr. FIELD that I respect his decision. In essence, I am under the impression that at the time of his answer he was not aware at all that our studies do have a direct astrophysical implication, and that I am indeed directly working in astrophysics.

In any case, as a result of this answer I have applied to other centers of astrophysics interested in the interplay between hadron physics and astrophysics, as well as in the experimental resolution of theoretical views. At this moment I have a couple of verbal offers, and I contemplate to reach a final decision by November-December.

Please do not feel obliged to intervene. I simply wanted to inform you. Nevertheless, in case you are interested in what we are doing, I would be happy to see you.

Sincerely

Ruggero Maria Santilli

RMS/nl
encls.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

December 18, 1979

Professor HEISUKE HIRONAKA
Chairman
Department of Mathematics
Harvard University

Dear Heisuke,

We are in the process of printing the PROCEEDINGS OF THE SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, and a draft of the table of contents is enclosed for your convenience.

As you can see, I would like to release my contributions to these Proceedings with my affiliation with the Department of Mathematics. The reason is that it is known in the scientific community that the studies of the Lie-admissible generalization of Lie's theory was launches while I was visiting your Department. I therefore believe that it would be scientifically correct, as well as in the interest of your department, to leave a formal record of the event.

In case you desire information on the quality of my contributions, please feel free to contact the following senior mathematicians who attended our workshop

Professor ROBERH H. OEMKE
Chairman
Department of Mathematics
The University of Iowa
Tel. 319-353 3669

Professor MARVIN L. TOMBER
Department of Mathematics
Michigan State University
tel 517 355 1000

The Proceedings are scheduled for print at the end of the year (around December 27-30). In case you have any objection, please let me know and you can count on my best understanding.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ml
c.c.: Associate Dean
R.G.LEAHY

January 2, 1980

Dear Heisuke,

I enclose copy of the table of contents and of the abstracts of the individual papers of the Proceedings of our Workshop on Lie-admissibility. Printing is expected to start sometime tomorrow. Again, in case you consider advisable that I change "Dept. of Math" into "Science Center" for my papers, please let me know, and I shall try to do my best.

There is a great exitement about these proceedings in mathematical and physical circles (outside quark financial interests) in the various Countries of the participants (USA, USSR, Switzerland, France, Greece, Israel, Australia, and others).

Perhaps, you might be interested to have an idea as a mathematician.

The starting point is provided by two theorems of invalidation of conventional quantization. One,proved by Abraham and Marsden in their recent edition of "Foundations of Mechanics", essentially states that there is NO MAP from functions A,B,... in the cotangent bundle T*M equipped with the Poisson brackets, to Hermitian operators $\tilde{A}$, $\tilde{B}$, ...in a Hilbert space equipped with Heisenberg's product

$$[A,B]_{ce} = \frac{\partial A}{\partial x^k}\frac{\partial B}{\partial p_k} - \frac{\partial B}{\partial x^k}\frac{\partial A}{\partial p_k} \longrightarrow \frac{1}{i}[\tilde{A},\tilde{B}] = \frac{1}{i}(\tilde{A}\tilde{B} - \tilde{B}\tilde{A}) \qquad (1)$$

The other, proved by a number of physicists, essentially states that Heisenberg's equations

$$\dot{\tilde{A}}(\tilde{x},\tilde{p}) = \frac{1}{i}[\tilde{A},\tilde{H}] \qquad (2)$$

are inconsistent on numerous counts (e.g., intrinsically inconsistent, violate the correspondence principle to Hamilton's equations, etc.) for all Hamiltonians that are of polynomial order in the canonical operators $\tilde{r}$ and $\tilde{p}$ higher than the second (that is, for all Hamiltonian vector fields that are nonlinear in the local variables).

The two theorems have resulted to be two sides of the same coin,
- one of geometrical character (general inconsistency of the symplectic quantization for canonical, that is, fundamental, symplectic two-forms); and
- the other of dynamical character (the quantization of Hamilton's equations into Heisenberg's equations is generally inconsistent).

The reasons for these rather serious inconsistencies have been unknown for some time. By spring 1979, the primary reasons had been sufficiently well identified. They have been then reverified at the workshop by the mathematicians and physicists who participated (either physically, or as corresponding participants because of lack of $ to travel). The most important one is that the Lie algebra of the Poisson brackets is the attached algebra to the

NONASSOCIATIVE LIE-ADMISSIBLE ENVELOPE WITH PRODUCT $A\cdot H = \frac{\partial A}{\partial x^k}\frac{\partial H}{\partial p_k}$ (3)

while the Lie algebra of the Heisenberg brackets is the attached algebra to the

ASSOCIATIVE LIE-ADMISSIBLE ENVELOPE WITH PRODUCT $\tilde{A}\tilde{H}$. (4)

As a result, mapping (1) is fundamentally inconsistent because it violates the algebraic structure of the underlying envelope. The validity of the Abraham-Marsden theorem is then selfevident. The valdiity of the other theorem is also selfevident.

This situation suggests, rather forcefully, the need of Lie-admissible algebras in quantum mechanics. In fact, as a necessary condition to attempt quantization, the time evolution law in quantum mechanics should be constructed via a nonassociative Lie-admissible algebra in exactly the same way as it occurs in Hamiltonian mechanics

HAMILTON'S EQUATIONS	→	QUANTUM MECHANICAL EQUATIONS
$\dot{A} = [A,H]_{Class.} = A\cdot H - H\cdot A$		$\dot{\tilde{A}} = \frac{1}{i}[\tilde{A},\tilde{H}]^* = \tilde{A}\cdot\tilde{H} - \tilde{H}\cdot\tilde{A}$
$A\cdot H$ = nonassociative Lie-admissible		$\tilde{A}\cdot\tilde{H}$ = nonassociative Lie-admissible, e.g. = $\tilde{A}\tilde{R}\tilde{H} - \tilde{H}\tilde{S}\tilde{R}$; $\tilde{R} \neq \tilde{S}$.

These are precisely the equations proposed in the Lie-admissible literature (in the HADRONIC JOURNAL) since 1978. They have remained largely ignored until now. They emerge now rather forcefully, because no alternative is known at this time to attempt consistent quantization.

The physical implications of this situation are substantial. The workshop succeeded in identifying the most relevant ones because we succeeded in putting toghether mathematicians and physicists. In essence, the replacement of the conventional associative envelope of quantum mechanics with nonassociative one implies the generalization of several crucial laws and principles, beginning with the generalization of the notion of spin.

The mathematical implications are equally intriguing. In my view, the following branches of mathematics

A- the theory of Lie algebras as conventionally studied by mathematicians until now, that is, in complyance with the Poincaré-Birkhoff-Witt theorem (associative envelope);

B- the symplectic geometry in canonical realization; and

C- the conventional theory of Hilbert spaces (that as one-sided modules)

are inapplicable for a consistent quantization, and should be replaced by

A' the theory of Lie algebra as the attached algebras of nonassociative envelopes (this is the key idea of Lie-admissible formulations);

B' the theory of "symplectic-admissible manifolds", to achieve a geometrization of the nonassociative Lie-admissible envelopes; and

C' the two-sided modular extension of the Hilbert space theory (nonassociative envelopes do not admit linear, one sided, representations on modules).

The energy-related implications are equally intriguing. Nonlinear effects in the controlled fusion are simply a reality. It was general consensus of the experts that the current techniques (based on an associative envelope) must be replaced, in due time, with Lie-admissible (nonassociative) techniques.

Part A of the Proceedings identifies the state of the art in these topics at the beginning of the workshop. My contribution is a memoir of some 550 pages with the review of some 300 theorems, lemmas, etc. (without even touching the proof).

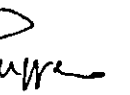
Part B of the Proceedings consists of the research papers along these lines. Most of them are of mathematical character (see the construction of the Lie-admissible algebras via a deformation of the Lie algebras by the Russians). By specific, planned, intent, the energy related aspect has been only touched in my paper with Fronteau and Tellez-Arenas of France. We are contemplating to have additional contributions by experts in the subsequent workshops with a progressive entrance into the topic.

Incidentally, generalizations A', B' and C' are some of the technical reasons why I propose to you the organization of a Center for Applied Mathematics (or other possible titles). In fact, in my view, they are so complex, intriguing, and valuable, to warrant the consideration of a center for coordinated study. These ideas were fully available at the time of my proposal, although I could not express them technically. The proceedings express these ideas in all needed details. Most intriguingly, it appears that the studies considered are unavoidable.

Please keep in mind this possibility. Whenever you consider appropriate or advisable to try again the organization of a Center for Applied Mathematics at Harvard, let me know. If, on my part, I shall organize this center at my new Institution next year, I shall let you know.

With my most sincere HAPPY NEW YEAR to you and your family, I remain

yours



P.S. The proceedings will be available within three weeks. The demand is quite great. Please let me know as soon as possible in case you desire a complimentary copy.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 4-35
One Oxford Street
Cambridge, Massachusetts 02138

January 8, 1980

Dr. G.B.Field, Director
Center for Astrophysics
Harvard University

University Mail

Dear Dr. Field,

To confirm our phone conversation, the DOE is interested in renewing my grant under the administration of the Hadronic Press, a Massachusetts Corporation producing the Hadronic Journal. Please feel free to contact Dr. D.C.Peaslee at the DOE, tel. 301 353 3624. Of course, this renewal is intended as an intermediate step of the current process of grant relocation.

In essence, I am currently considered by several institutions. One of particular interest is the Lawrence Berkeley Laboratory, where I have applied to Dr. R.W.Birge, Director of the nuclear division. Nevertheless, my current grant expires on June 1, 1980, while it appears advisable to avoid pressures for an early decision on rather intriguing but delicate topics. The intermediary solution of letting a Massachusetts non-academic corporation to administer my grant has therefore emerged quite natural.

To facilitate a smooth transition in this predictably delicate grant relocation, I have asked you the courtesy of considering me for a possible guest status. In essence, I would need a letter, to be enclosed in the grant application, stating that

(1) I can be your guest for the academic year 1980/1981; and
(2) I can use the library and parking facilities.

In case you consider it appropriate, please feel free to consider the addition that my guest status can be terminated prior to the end of the academic year for administrative and other possible reasons.

A desk or office either at your Center or here at the Science Center would be welcome, but it is not essential, and I see no reason to mention it in the letter, although I would like to rely in your judgment.

Also, in case you consider it advisable, I can continue to use this stationary indicating my affiliation with the "Science Center" rather than with your Center. As indicated to you by phone, at the time of my appointment at the Department of Mathematics I decided to use this stationary in all my correspondence, including that of editorial character, as a form of my appreciation for the hospitality received.

On scientific grounds it is a truism to say that this is an intriguing and promising moment. I have just released for publication the Proceedings of the Second Workshop on Lie-admissible treatments of the strong interactions. It consists of two volumes (one of review, and one of research) for over 1500 pages by mathematicians and physicists from the USA, USSR, China, France, Switzerland, Australia, Israel, and Greece. It will be a pleasure to send you a complimentary copy as soon as available (within a few weeks). There are a number of astrophysical implications which will likely intrigue you (essentially related to the geometry for the interior problem which allows the representation of at least "simple" systems such as a satellite in Earth's atmosphere).

Whatever your final decision will be, permit me to express my appreciation and gratitude for your time and consideration.

Sincerely yours Ruggero Santilli

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

January 14, 1980

Dear Dr. Field,

I enclose some informative material on the series "Developments of the Quark Theory of Hadrons" organized by the Hadronic Journal, under the editorship of Don Lichtenberg and Peter Rosen.

It is the result of my considerable effort in funding the project, which lasted for several months. Happily, the project is now well under way.

This series of strict quark orientation is intended to complement the other series "Applications of Lie-admissible algebras in physics" (edited by Hyo Myung, Susumo Okubo and myself), which is of strict non-quark orientation.

The idea is to achieve a well balanced presentation of research on hadrons in which all valuable lines, whether of quark or non-quark orientation, are pursued, in the traditional, as well as most effective way of confronting open physical problems.

It is regrettable that our colleagues at Lyman do not apparently accept this scientific view.

Best and sincere regards

Center for Astrophysics
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

January 14, 1980

Dr. Ruggero M. Santilli
Science Center, Room 435
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli,

I am sorry that it will not be possible to fulfill the request made in your letter of January 8, 1980. There is a large unmet demand for our regular visitors programs, so it would not be fair to make exceptions for scientists who actually have no direct relation to our work in astrophysics.

Sincerely,

George Field

George B. Field
Director

GBF/ljc

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

January 15, 1980

Dr. H. HIRONAKA

Chairman
Department of Mathematics
Harvard University

UNIVERSITY MAIL

Dear Dr. Hironaka,

As you know, my appointment expires on June 1, 1980. Please let me know whether I should free my office at that date, or I can remain through the summer, up to but not later than August 15, 1980.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
c.c.: Dr. R. Leahy,
Associate Dean

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 25, 1980

Dr. H. HIRONAKA
Chairman
Department of Mathematics
Harvard University

Dear Dr. Hironaka,

Your secretary has indicated that I should outline my activities in regard to my note of January 15, 1980 (copy enclosed).

You are familiar with my editorial/research activities. During this summer I shall continue my function as editor of the Hadronic Journal for papers in pure mathematics and theoretical physics, and I shall work at my Volume II with Springer-Verlag, which, as you know, is a monograph in applied mathematics. A copy of an independent review of my Volume I is enclosed. I do not expect to have the time to write papers. On the last two-three weeks of August I shall take my vacations, and on September 1 I shall assume my new job outside Boston (for several personal reasons I cannot leave the Boston area this summer).

The primary reason for my note of January 15, 1980, is whether or not I shall continue to have my address "Science Center, Harvard University" in the cover of the Hadronic Journal during this summer (copy enclosed). There will be only two issues this summer, those of June and of August 1980.

Since the cover of the Journal is printed well in advance, I would appreciate an early decision on this matter. Also, I would gratefully appreciate a written communication of your decision.

Very Truly Yours

Ruggero Maria Santilli
Research associate

RMS/ml
encls.
cc.: AssociaTe DeanR.Leahy

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 12, 1980

Professor Ruggero Santilli
Department of Mathematics
Harvard University

Dear Ruggero:

In the Departmental Meeting of February 1, it was discussed and voted once again to confirm that your appointment with the Mathematics Department is to be terminated as was previously planned and conveyed to you last year. It is our general policy that visiting positions should not be be continued too long so as to have constant flow of new research projects and stimulations within the Department.

By that departmental decision, your formal association with Harvard, Math Department or Science Center, will end at the end of May, 1980, and you have no right to use a Harvard office address in any official document. As to your moving out of you current office in the Math Department, we understand that you may find it difficult to move out immediately in June and wish to do it rather gradually (with reasonable effort to an early evacuation, of course).

Sincerely yours,

Heisuke Hironaka

Heisuke Hironaka
Chairman

HH/mjm

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

April 25, 1980

Professor H. HIRONAKA
Chairman
Department of Mathematics UNIVERSITY MAIL

Dear Professor Hironaka,

I acknowledge receipt of your recent note confirming the termination of my appointment on June 1, 1980, and indicating the possibility of my continuing to use the current office for a limited additional period of time (and definitely not beyond August 15, 1980).

For your information, and as a rather important part of my current research under DOE support, the THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS was tentatively scheduled in Cambridge (from August 4 to 9, 1980) several months ago.

The organization of this workshop is now close to completion. A list of participants is enclosed. In addition, we contemplate to have a number of distinguished guests (such as editors of physics Journals).

I assume you have no objection for having this scientific event at Harvard, and I am continuing the organization under this assumption.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
ecls.

c.c. Ass. Dean Leahy.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

Area Code 617
495-2170

Science Center
One Oxford Street
Cambridge, Massachusetts 02138

March 19, 1980

TO: Mathematicians interested in quantum mechanics
FROM: R.M.Santilli, Editor of the Hadronic Journal
SUBJECT: call for help for an intriguing editorial impasse

You might be interested to have some information about an editorial impasse which occurred recently at the Hadronic Journal. It concerns all physics articles in nonrelativistic quantum mechanics based on Heisenberg's equations (and related physical laws) with generalized Hamiltonians of the type

$$H_{gen}(q,p)=T_{gen}(q,p)+V(q,p)\,;\ \text{Polyn.Order}\ T_{gen}(q,p) \gtrsim 3;\ V(q,p) = \text{linear in } p, \qquad (1)$$

e.g., $H_{gen} = \frac{1}{2}pF(q)p + V(q)$ (Nota Bene: the impasse excludes conventional Hamiltonians $H = T(p) + V(q,p)$ with Polyn. Order T = 2, as occurring for electromagnetic interactions).

A significant number of papers in different fields are involved in this intriguing case, with particular reference to: nonrelativistic quark dynamics; nuclear physics; quantum statistical mechanics; plasma physics; controlled fusion; and quantum gravity.

The impasse originated with the submission to the Hadronic Journal of a comprehensive paper in nonrelativistic quark dynamics (for which the use of generalized Hamiltonians is necessary to achieve meaningful mass spectra). The paper was recommended for publication by qualified referees. But other, equally qualified, referees recommended the rejection quite firmly. The inability to resolve the technical differences between these equally qualified, opposing views, resulted in the impasse. The fact that the problems originate in the generalized structure of the Hamiltonian, and the joint use of conventional laws, suggested the extension of the impasse to other fields.

To the best of my understanding, the problematic aspects underlying the impasse are the following.

Problematic aspects in the quantization. As known in mathematical circles, a theorem by Abraham and Marsden (following notes by Chernoff, as well as preceding contributions) (ref.1) establishes the lack of existence of the full quantization for the models considered. A first group sees no problem in this, on the basis that two different disciplines should not necessarily admit a map. A second group disagrees on the basis that, to prevent possible intrinsic inconsistencies of quantum mechanical models, the problematic aspects of quantization should equivalently occur for all quantum representations (e.g., those via Heisenberg's equations, via Schrödinger's equation, via Lagrange's equations, etc.). The issue is therefore whether or not the various representations of quantum mechanics are consistent (that is, mutually compatible) from the viewpoint of quantization, e.g., whether or not the Abraham-Chernoff-Marsden theorem admits a form of image for the quantization of the Hamilton-Jacobi into Schrödinger's equation. To my knowledge, no contribution by mathematicians exists on this topic at this time.

Intrinsic problematic aspects. Generalized Hamiltonians (1) activate a lemma by Hellman and Hood (ref.2) according to which, for the Hamiltonians considered, Heisenberg's equations are not necessarily equivalent to the (operator) Lagrange's equations (for conventional Hamiltonians this problem does not exist). A first group dismisses this occurrence, e.g., on grounds that there exist transformations $(q,p) \to (q',p')$ mapping $H_{gen}(q,p)$ into $H'_{conv.}(q',p')$. The equivalence between Heisenberg's and Lagrange's equations is then

regained (under boundedness and other conditions inessential here) for the transformed Hamiltonian, as often used, e.g., in path integral approaches. A second group disagrees quite vigorously on a number of counts, e.g.,
(a) Generalized Hamiltonians violate the imprimitivity theorem (ref.3, p.204) for a genuine validity of Galilei's relativity. Thus, the transition from conventional to generalized Hamiltonians may imply the loss of Galilei's relativity, and, thus, of the notion of Galilean quantum particle.
(b) When the equations of motion are computed explicitly, generalized Hamiltonians imply nonconservative, nonlinear, velocity-dependent forces. In this case, the systems are open, that is, they violate the conservation of total physical (rather than canonical) quantities, such as, total angular momentum, energy, etc. (hint: for Hamiltonians (1) the symbol "p" does not represent the physical linear momentum $m\dot{q}$). This appears to confirm problematic aspects (a).
(c) The time evolution of open systems in the vector field form with local variables q and p = physical linear momentum is noncanonical at the classical level, and nonunitary at the quantum level for coherence of the theory under the classical limit. Under a nonunitary time evolution, most of the conventional laws and principles of quantum mechanics (e.g., Pauli's exclusion principle; Heisenberg's indeterminacy principle; etc.) are not preserved, as shown in ref. 4, pp. 1865-1888. Similarly, the transformations mapping $H_{gen}(q,p)$ into $H'_{conv}(q',p')$ are generally noncanonical at the classical level, and nonunitary at the quantum level. The equivalence of Heisenberg's and Lagrange's eqs. would be then regained at the loss of the basic physical laws. This confirms the problematic aspects for the conventional notion of Galilean quantum particle.

The implications of these occurrences are nontrivial. For example, for models of plasma physics with Hamiltonians (1) the validity of Pauli's exclusion principle is open (theoretically and experimentally, to my best knowledge); for models of dissipative nuclear processes with Hamiltonians (1) the validity of Heisenberg's indeterminacy principle is unresolved at this moment (also theoretically and experimentally, to my knowledge); for nonrelativistic quark models, the problematic aspects prevent at this time a consistent, quantitative, formulation of the hypothesis that quarks are physical Galilean particles, without affecting the physical content of these models as far as the Mendeleev-type classification of hadrons is concerned (the classification can be conducted via spectrum generating, Schrödinger-type equations for which no problematic aspect is know at this time).

Problematic aspects in the classical limit. Even though not universally accepted, classical mechanics is expected to be admitted by quantum mechanics under "a" suitable limit, for the logical coherence of the theory. The open problems are here numerous. For instance, we do not apparently know at this time whether the Abraham-Chernoff-Marsden theorem admits a form of "inverse". Also, we do not know whether problematic aspects in the limit of Heisenberg's into Hamilton's equations equivalently exist for the limit of Schrödinger's into Hamilton-Jacobi equations. The background issue is whether the various representations of quantum mechanics are mutually compatible under the classical limit (ref.5).

Any critical comment, remark, or advice would be gratefully appreciated. To assume full responsibility, I enclose copy of my ref.5 providing an outline of the problematic aspects, while I remain at the disposal of interested colleagues for more specific information.

REFERENCES

(1) R.Abraham and J. E. Marsden, Foundations of Mechanics, Benjamin/Cummings (1979 edition)
(2) W.S.Hellman and C.G.Hood, Phys. Rev. D5, 1552 (1972)
(3) G.W.Mackey, Unitary Group Representations, Benjamin/Cummings (1978 edition)
(4) R.M.Santilli, Hadronic J. 2, 1460 (1979)
(5) R.M.Santilli, Hadronic J. 3, 854 (1980)

P.S. Some of these open problems are contemplated to be studied at the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS scheduled in Cambridge, Ma, from August 4 to 9, 1980.

January 8, 1980 PLEASE POST

TO: The Editorial and Advisory Boards of Journals in Theoretical Physics

FROM: R. M. Santilli, Editor of the Hadronic Journal. Current address: Harvard University, Department of Mathematics, Cambridge, Ma. 02138 USA

SUBJECT: Theoretical and experimental papers on quarks and other topics which activate the theorems of inconsistency of Heisenberg's equations.

Dear Colleagues,

Recent studies by mathematicians and physicists have proved two theorems establishing that Heisenberg's equations are generally inconsistent, both intrinsically and with respect to the correspondence principle. A number of current applications of quantum mechanics activate the inconsistency theorems in a rather direct way. This poses the problem of the editorial processing of papers of this type. With this letter I would like to: fulfill my duty as regards rapid dissemination of the information; recommend a study of the problem; and solicite the possible achievement of a joint resolution. The technical aspects underlying this letter have been discussed in the informal sessions following the Second Workshop on Lie-admissible Formulations (held here at Harvard from August 1 to 8, 1979), and are reported in the Proceedings[1, 2]. To assume complete responsibility, I shall often quote my review[3] in the first volume of the Proceedings[1] and refer to it with the notation (3, pp. . .). The understanding is that the study of the original contributions, as well as of the complete Proceedings[1, 2] is essential for technical knowledge of the problem.

THE THEOREMS OF INCONSISTENCY OF HEISENBERG'S EQUATIONS.

In their recent treatise[4], the known mathematicians Abraham and Marsden have proved a theorem within the context of the symplectic quantization essentially stating that **THERE EXISTS NO MAP** from the Poisson brackets of functions A, B, . . . in phase space (the cotangent bundle), to Heisenberg's product of Hermitian operators $\tilde{A}, \tilde{B}, \ldots$ on a Hilbert space ($\hbar = 1$).

$$[A,B]_{cl} = \frac{\partial A}{\partial r^k}\frac{\partial B}{\partial P_k} - \frac{\partial B}{\partial r^k}\frac{\partial A}{\partial P_k} \longrightarrow \frac{1}{i}[\tilde{A},\tilde{B}] = \frac{1}{i}(\tilde{A}\tilde{B} - \tilde{B}\tilde{A}) \qquad (1)$$

Independently of these results, a number of physicists have proved a theorem via the theory of nonassociative algebras essentially stating that **HEISENBERG'S EQUATIONS ARE INCONSISTENT** for all Hamiltonians of at least polynomial order three in the canonical operators $\tilde{r}$ and $\tilde{p}$. Some of the inconsistencies are: (a) the Lie algebra product of Heisenberg's time evolution $(\tilde{A}\tilde{H} - \tilde{H}\tilde{A})|>$ is intrinsically inconsistent; (b) there is a violation of the equivalence with the (operator form of) Lagrange's equations; and (c) Heisenberg's equations in the Hamiltonian $\tilde{H}$ violate the correspondence principle to Hamilton's equations in the classical limit H of $\tilde{H}$. For a review of the inconsistency theorems, as well as of a number of additional lemmas and propositions I have omitted here for brevity, one may consult (3, pp. 1977-1782).

The implications of the theorems are rather delicate. First of all, the physical calculations are inconsistent (because the value of the product $(\tilde{A}\tilde{H} - \tilde{H}\tilde{A})|>$ is not unique, but depends on the selected application of the differential rule). Secondly, the theorems prevent a rigorous proof of the validity of conventional quantum mechanical laws, such as Heisenberg's indeterminacy principle, Pauli's exclusion principle, Einstein's frequency principle, etc. Finally, the lack of a consistent time evolution law prevents the achievement of a consistent formulation of Galilei's or Einstein's special relativity. For a review, the reader may consult (3, Section 2.3, pp. 1777-1905).

The technical identification of systems escaping the theorems is under study. It appears that all linear Newtonian systems (e.g., the familiar harmonic oscillator $m\ddot{r} + kr = 0$) can be consistently quantized via Heisenberg's equations because they admit quadratic Hamiltonians (e.g., $H = \frac{1}{2}mp^2 + \frac{1}{2}kr^2$). Also (under certain technical conditions still under study), the structure of atoms and the (nonrelativistic) electromagnetic interactions can be consistently quantized via Heisenberg's equations. As a result, the validity of conventional quantum mechanical laws for the atomic structure and the (long range) electromagnetic interactions in general, is unaffected.

The identification of systems activating the theorems is, in general, rather simple. For instance, nonlinear Newtonian systems (e.g., the anharmonic oscillator) cannot be in general consistently quantized because the Hamiltonian (when it exists) is of polynomial order higher than two (e.g., $H = \frac{1}{2}mp^2 + \frac{1}{2}kr^2 + \lambda r^4$). These nonlinear effects occur in a number of trends of contemporary research, although they appear to be more frequent for the strong interactions (see below).

The reasons for the inconsistency of Heisenberg's equations and related physical laws are intriguing. A rather important reason has been identified via the theory of nonassociative algebras. The envelope of the Poisson brackets $[A,H]_{cl}$ is a **NONASSOCIATIVE LIE-ADMISSIBLE ALGEBRA** with product $A \cdot H = (\partial A/\partial r^k)(\partial H/\partial P_k)$, while the envelope of Heisenberg's product $[\tilde{A}, \tilde{H}]$ is an **ASSOCIATIVE LIE-ADMISSIBLE ALGEBRA** with product $\tilde{A}\tilde{H}$[5]. Thus, the quantization

$$\begin{bmatrix} \textbf{Hamilton's equations} \\ \dot{A} = [A, H]_{cl} = A \cdot H - H \cdot A \\ A \cdot H = \text{nonassociative} \end{bmatrix} \longrightarrow \begin{bmatrix} \textbf{Heisenberg's equations} \\ \dot{\tilde{A}} = \frac{1}{i}[\tilde{A}, \tilde{H}] = \frac{1}{i}(\tilde{A}\tilde{H} - \tilde{H}\tilde{A}) \\ \tilde{A}\tilde{H} = \text{associative} \end{bmatrix} \tag{2}$$

despite its use for over half a century, is **INCONSISTENT** because it violates the character of the enveloping algebra (3, pp. 1787-1792).

A quantization currently under study to bypass the inconsistency theorems under nonlinear effects is given by (3, pp. 1800-1819)

$$\begin{bmatrix} \textbf{Hamilton's equations} \\ \dot{A} = [A,H]_{cl} = A \cdot H - H \cdot A = \text{Lie product} \\ A \cdot H = \text{general (nonassociative)} \\ \quad \text{Lie-admissible algebra} \end{bmatrix} \longrightarrow \begin{bmatrix} \textbf{Quantum mechanical equations} \\ \dot{\tilde{A}} = \frac{1}{i}[\tilde{A},\tilde{H}] = \frac{1}{i}(\tilde{A} \cdot \tilde{H} - \tilde{H} \cdot \tilde{A}) = \text{Lie product} \\ \tilde{A} \cdot \tilde{H} = \text{general (nonassociative)} \\ \quad \text{Lie-admissible algebra} \end{bmatrix} \tag{3}$$

where, e.g., $\tilde{A} \cdot \tilde{H} = \tilde{A}\tilde{R}\tilde{H} - \tilde{H}\tilde{S}\tilde{A}$, $\tilde{R} \neq \pm\tilde{S}$ and fixed, according to Santilli's proposal[6] which calls for the intermediary use of the Birkhoffian generalization of the Hamiltonian mechanics[7]. Quantization (3) at the level of the equation of motion is complemented by a quantization at the level of the enveloping (nonassociative) algebra proposed by Ktorides[8] (and extended to quantum field theory by the same author). More recently, quantization (3) for the flexible particularization of the general Lie-admissible algebras (e.g., $\tilde{A} \cdot \tilde{H} = \lambda\tilde{A}\tilde{H} - \mu\tilde{H}\tilde{A}$) has been worked out by Okubo[9].

The main idea of quantization (3) is that of generalizing the conventional associative envelope of Heisenberg's equations into a nonassociative form, as a necessary condition for preserving the algebraic structure of Hamilton's equations[3]. The implications of this generalization are nontrivial, mathematically and physically. Mathematically, quantization (3) calls for (i) the reformulation of Lie's theory, from its currently available version (as the attached algebra of an associative envelope according to the Poincaré-Birkhoff-Witt theorem) to a more general form (as the attached algebras of nonassociative envelopes according to Ktorides' generalization[10] of the Poincaré-Birkhoff-Witt theorem); (ii) the generalization of the symplectic geometry into a covering form capable of geometrizing non-associative Lie-admissible algebras (tentatively called symplectic-admissible geometry[11]); and (iii) the generalization of the conventional, one-sided, Hilbert space theory to a genuine, two-sided (left and right), bimodular form[12]. These and other problems were studied at the Second Workshop on Lie-admissible Formulations[1,2]. Physically, quantization (3) implies the need for a suitable generalization of the relativities, principles and insights of quantum mechanics (sometimes called "atomic mechanics"). A rather feverish study is now under way by a number of researchers for a possible covering mechanics (sometimes called "hadronic mechanics"), and I refer the interested colleague to (3, Part 2, pages 1682-1987).

Oddly, this generalization of quantum mechanics does not appear to interest the contemporary physics community at large. Yet, the generalization was advocated or expected by the Founding Fathers of contemporary physics. For example, the nonassociative generalization of the envelope of quantum mechanics was proposed by Jordan, von Neumann, and Wigner; the inapplicability of conventional geometries within a strongly interacting particle was predicted by Fermi; the lack of final character of Heisenberg's uncertainty was vigorously advocated by Einstein; etc. For these and other historical, authoritative, open legacies, one may consult (3, pp. 1700-1718; and 1865-1889).

PAPERS IN NUCLEAR PHYSICS, CONTROLLED FUSION, QUANTUM GRAVITATION, AND OTHER FIELDS.

Polynomial Hamiltonians of order higher than two (representing nonlinear systems) are not uncommon in the fields considered. All these Hamiltonians activate in a rather direct way the inconsistency theorems, by therefore posing the problem of the editorial processing of papers submitted to our Journals. Needless to say, there exist numerous approaches that are expected to avoid the inconsistency theorems. This is the case, for example, with the non-conservative formulation of statistical mechanics by Prigogine and his collaborators[13], or the approach to the exterior problem of gravitation by Yilmaz[14].

PAPERS ON QUARKS.

Nonrelativistic quark models activate directly all theorems, lemmas, and propositions invalidating Heisenberg's time evolution law. This is due to a number of reasons, such as the fine structure terms $H_{fs}(r,p,s,M,\ldots)$ which are added to conventional Hamiltonians $H = T(p) + V(r)$ to attempt mass spectra. Again, what is at stake is not only the consistency of the calculations, but more insidiously the validity of conventional laws, and, thus, the consistent, quantitative definition of quarks as physical particles (3, Section 2.4).

Irrespective of this, a number of additional inconsistencies of these models have recently come to light, such as the fact that the models violate the conservation laws of the total, physical, angular momentum, the correspondence principle; etc. (3, pp. 1928-1943).

Also, it appears appropriate to recall a rather increasing uneasiness in one segment of our community in regard to the problem of confinement[15]. A growing consensus is that, since the spontaneous decays

$$\text{mesons} \longrightarrow q + \bar{q} + \ldots; \qquad \text{barions} \longrightarrow q + q + q + \ldots$$

do not exist according to available knowledge, papers on quarks should present explicit, detailed, and consistent calculations on the fractions of the spontaneous decays of hadrons into free quarks. If these fractions are not identically null, or, at least sufficiently small, the models are inconsistent[16].

Needless to say, the inconsistency theorems in their currently available (discrete) form do not affect quantum chromodynamics. Yet, their impact should not be underestimated. The theorems leave QCD without a consistent nonrelativistic limit. Also, their expected field theoretical extensions might well affect QCD directly (because all gauge, unitary, and relativity algebras of current use in QCD are based on associative envelopes[17]).

I should add that the inconsistency theorems *do not* affect unitary models in their original conception, that of providing a *classification* of hadrons. In fact, the theorems are activated only when quarks are assumed as physical particles, thus demanding specific dynamical equations and specific physical laws. In different terms, the inconsistency theorems are activated when, as implicit in current trends, one attempts the representation of the totality of the hadronic phenomenology (classification, structure, and scattering) via one single model, the quark model.

As a result of this occurrence, the inconsistency theorems are stimulating, via mathematical arguments, the consideration of a proposal of the Lie-admissible literature[18] which is rather natural on physical grounds. As it occurred for the atoms, it may well be that the hadronic phenomenology demands different yet compatible models: the established unitary models for the **Mendeleev-type classification** of hadrons into multiplets, and a different, **Bohr-type model of structure** for each individual element of a unitary multiplet.

EXPERIMENTAL PAPERS IN STRONG INTERACTIONS.

As we know well, all measurements (and, thus, all experiments) are approximate. The approximate character of experiments in strong interactions is compounded by the fact that a number of theoretical assumptions are made in the data elaboration (3, pp. 1697-1699). For instance, the experimenter may use a cross section as derived from the "potential scattering theory". The underlying assumption is that the strong interactions are derivable from a potential. But this assumption is rather controversial. In fact, the idea that the strong interactions are nonderivable from a potential (as a local approximation of nonlocal settings) is rather old, and dates back to the very first studies on strong interactions. As a result, we simply do not know at this time whether cross sections of current use by experimenters in strong interactions are correct or erroneous. Similarly, the experimenter often uses in data elaboration (either in a direct or in an indirect way) Einstein's special relativity, the spin-statistics theorem, and other physical laws whose validity for the strong interactions is also controversial. In fact, a rather intensive effort is being made to study quantitatively the possible invalidation of conventional physical laws for the strong interactions, because they expectedly imply point-like approximations of particles interacting at distances smaller than their size.

Owing to this situation, it appears advisable that experimental papers in strong interactions present the identification in as detailed a way as possible (e.g., via a list) of *all* the theoretical tools, conjectures, and assumptions used in the data elaboration. In this way the individual reader can reach his personal assessment on the approximate character of the results; the interested researcher can attempt the elaboration of the same data via different theoretical tools; and the editor can ascertain whether or not some of the theoretical tools used in the data elaboration activate the inconsistency theorems.

Needless to say, what is much needed is to resolve experimentally whether or not the inconsistency theorems are activated by the strong interactions, that is, whether the conventional laws of the electromagnetic interactions are valid or invalid for the strong. A number of proposals of specific tests are available (3, Section 2.5), and several others are conceivable, all apparently feasible with available technology. For instance, I am told that the proposal

of ref.[6,18] to establish whether Pauli's exclusion principle is valid in nuclear physics or small deviations are detectable (that is, whether the SU(2)-spin symmetry is exact or broken and, thus, whether the Lorentz symmetry is exact or only approximate under strong interactions) can be tested via suitable implementations of the current experiments of neutron beams on crystals[19].

CONCLUDING REMARKS.

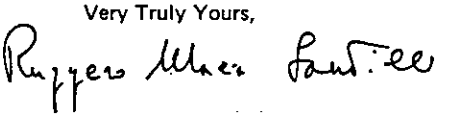
Basic research appears to be in an intriguing, but delicate phase, with a continuation of brilliant achievements, together with a proliferation of problematic aspects, particularly in the case of the strong interactions.

At present, we are apparently performing our editorial function independently, to the best of our capabilities. Nevertheless, this has created some disparities. For instance, some of us have implemented a moratorium on all papers on quark models and other fields which activate the inconsistency theorems. Others, perhaps unaware of these theorems, continue to accept papers which can be rigorously proved as being inconsistent.

Owing to this situation, I submit that we should conduct a coordinated study of the problem, and possibly reach a joint resolution. The most effective way to achieve the objective would be, in my view, the organization of a meeting which could be attended by all interested editors. The participation of researchers with a *technical* knowledge of the problem would, of course, be welcomed. Whether a formal meeting will be organized or not, you are welcome to attend our Third Workshop on Lie-admissible Formulations (tentatively scheduled here in Cambridge during the week of August 4 to 9, 1980), for which we are attempting to gather a number of mathematicians and physicists who are experts in the field.

If I can be of any assistance, please do not hesitate to contact me.

Very Truly Yours,



Ruggero Maria Santilli

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[redacted]

Department of Mathematics

Feb 6, 1980

Dear Prof. Santilli

Thanks for your invitation to attend the 3rd Workshop on Lie-admissible formulations. Unfortunately I am already committed for this August; I otherwise would have been delighted to attend.

Thanks also for the information on the use of our theorem in Foundations of Mechanics. I'd like to remain informed.

I'd be happy to be a corresponding participant in your conference in whatever capacity you wish.

Sincerely

[redacted]

P.S. Can I suggest inviting Paul →

[redacted] of our department to your meeting. The result in F of M you mentioned was obtained in collaboration with him (see bottom of p. 434).

GM

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 10, 1980

Professor J. MARSDEN
Department of Mathematics
University of California

Dear Professor Marsden,

I would like to express the sentiments of my appreciation for your kind letter of February 6, and for your interest. I am particularly grateful for your desire to be of assistance to us as a corresponding participant to our THIRD WORKSHOP ON LIE-ADMISSIBILITY. I believe that this would be equally invaluable.

I would like therefore take the liberty of suggesting that you prepare some informal notes (even hand written) on the problem and you mail them to me. I shall have copies of them distributed to the participants (only of our workshop). In case you have the time to subsequently finalize the notes and bring them up to the form of a paper, I would be simply pleased to publish them in the Proceedings of the Third Workshop.

For this purpose:
- the informal notes should reach me not later than August 1, 1980, and
- the formal paper should reach me not later than December 25, 1980.

What I am trying to express is that we would like to do our best so that your view is taken in due and full account at the workshop, particularly for the editorial, decision making process.

The editorial problem is essentially the following. Recall the following occurrences:

(1) Lack of uniqueness of the value of Heisenberg's time evolution. As you know well, $\overline{A} = (AH-HA)I$ does not have a unique value for all (hermitian) polynomial operators A and/or H in the canonical operators r and p of at least order three. Instead, it has a number of values depending on the selected use of the differential rule. This creates considerable physical problems (actually an array of problems). It appears that we have a form of indeterminacy o/o which, as such, cannot be removed via substraction-type techniques. Also, the selection of one use of the differential rule implies the lack of necessary equivalence of physical numbers computed via Heisenberg's and Schrödinger's equations. The distressing aspect is that we simply do not know a way out at this moment capable of preserving old doctrines (that is, the associative character of the envelope).

As a result of this situation, as an editor I simply do not know how to handle papers based on Heisenberg's equations, and activating this inconsistency. This lack of capability to reach a mature judgment is shared by other editors. At the third workshop we would like to relay primarily on the advice of mathematicians experts in the field, and hopefully reach a decision. The alternatives are:
- reject all papers of this type;
- accept them; or
- continue the moratorium until the air is cleared, e.g., by experimentalists.

(2) Inequivalence of Heisenberg's and Lagrange's equations.* This additional inconsistency is also due to a form of break-down of the differential rule, this time that of chain type. Thus, problems (1) and (2) are related. Yet, there are differences. For instance,

* Please see the simple calculations of pages 1779-1780 of the Proceedings of the Second Workshop, Volume A (I mailed you early).

the unharmonic oscillator activates (1) but not (2). On grounds of our truly limited knowledge,we do not understand this difference. Most importantly, we do not understand why quantum mechanics works for the Coulomb potential (atomic structure) which might activate (1), say, in polynomial expansions, while it does not activate (2).By recalling that the electromagnetic interactions are linear in the velocity, does this situation imply that the intrinsinc inconsistencies are activated only by systems that are nonlinear in the velocities (that is, activating jointly (1) and (2))?

In any case, we do not know how to handle editorially papers activating the inequivalence between Heisenberg's and Lagrange's equations. We cannot assume one equation more fundamental than the other. For instance, the assumption of Heisenberg's equations as the fundamental ones and the elimination of Lagrange's equations (by fiat!) implies fundamental problems for quantum chromodynamics (which, as you know, is based on Lagrange's equations). The inverse aprioristic assumption is equally problematic. Lacking a solution, we are forced into the current moratorium(that is,neither the acceptance nor the rejection because of lack of technically mature, final information).

(3) The invalidation of the correspondence limit of Heisenberg's equations into Hamilton's form.* This is a sort of inverse formulation of your crucial theorem 5.4.9. In principle, this occurrence could also be eliminated by fiat in the sense that one may assume the lack of existence of a meaningful map to "construct a true theory from a false one" according to another editor in pnysics. The problem is created because of the following occurrence.

Again, to our understanding, Heisenberg's representation (when consistent...) and Schrödinger's representation are equivalent. However, despite a considerable search, no Schrödinger's "image" of Heisenberg's invalidation of the correspondence limit has emerged. In different terms, under all needed condition (e.g.,boundedness from below) Schrödinger's equation is consistent not only intrinsically, but also with respect to the correspondence principle for all Hamiltonians activating (1) and (2). One reaches nicely and smoothly the Hamilton-Jacoby equation in the classical limit of the Hamiltonian, while we cannot reach Hamilton's equations in the same Hamiltonian when starting from Heisenberg's equations (dichotomy infinite/finite systems?).

In different terms, all the difficulties (1), (2) and (3) are restricted (to our limited understanding at this moment) to Heisenberg's equations, that is, to the quantum mechanical formulations in terms of the realization of Lie algebras via the trivial product ab-ba and the symplectic geometry in the canonical form.

The point remains that we do not know at this moment how to handle editorially papers activating (3) and, more specifically, papers for which the correspondence principle is fully valid for Schrödinger's equations and fundamentally invalid for Heisenberg's equations.

Notice that these problems are expected to multiply in time!**In fact, one researcher is already searching for corresponding problems for the Feynman path integral formulation.

With respect to your possible informal notes and participation as a corresponding participant, ANY comment,advice,or council on how to handle the editorial situations (1), (2) and (3) would be gratefully appreciated. Most welcome would be CRITICAL views of these statements, in case erroneous. In different terms, we are not looking for approval of our views, doubts and concerns. We are looking for advice in a truly intriguing, but delicate scientific moment.

* Please see the simple proof of pages 1779-1780 of Part A of the Proceedings.

** The difficulties for the map of the unity might intrigue you (see pages 1481-1483 of Part A of the Proceedings).

Page 3.

An additional area in which advice and critical inspections would be sincerely welcome is related to the possibilities currently under study to attempt the bypassing of troubles (1), (2) and (3). For instance, several of us (me included) are working at generalizing

- the Lie algebra product from the trivial AH-HA into (what we call the) isotopically mapped form ACH -HCA, C = fixed;
- the symplectic form from the fundamental to a local and exact, but otherwise unrestricted form.

Apparently, this removes all the computational troubles*. But the "price to pay" is the abandonment of quantum mechanical notion of algebraic origin, such as the notion of spin. In fact, the familiar value $J_iJ_i|>$ is now generalized into the form $J_iCJ_i|>$.

It would be invaluable for us to know whether we do have mathematical consistency in this generalized setting, that is , whether it is true or false that:

(1') the inconsistency (1) is eliminated, thus achieving unique value of the time-evolution law;

(2') the inconsistency (2) is also eliminated, thus avoiding conflict with Lagrangian formulations of high energy physics; and

(3') the inconsistency (3) is also eliminated, by therefore achieving consistency of the correspondence principle in its algebraic/geometric and wave/Schrödinger formulations.

Needless to say, we are truly exited at these possibilities, and a feverish activity is going on. I am leaving next week for a trip to Europe (to deliver invited presentations on the situation), and I contemplate to be back by mid March. It would be a pleasure for me to visit you and discuss in an informal manner the situation in more detail.

In closing, I would like to express my appreciation also for suggesting the invitation of Professor PAUL R. CHERNOFF. A formal invitation is enclosed and it would be a pleasure for us to have him at our workshop. To facilitate communication, I am taking the liberty of sending him copy of this letter. Needless to say, any advice, criticism, suggestion that Professor Chernoff might have would be sincerely welcome.

Incidentally, the second workshop was conducted in a truly relaxed, informal atmosphere without speeches. We shall do our best to preserve this atmosphere for the third workshop. We essentially hope to have a few selected mathematicians, a few theoretical physicists,and a few editors, for a total number of say 20-23 participants (to avoid dispersal of energies).

The participations by mathematicians will be coordinates among experts in nonassociative algebras, experts in Lie's theory and experts in the symplectic geometry. As you can see from the Proceedings of the Second Workshop, Professors Tomber, Oehmke and Myung are the experts of our group in the theory of nonassociative algebras. Besides you, Professor Abraham and Professor Chernoff, we have not issued other invitations at this moment for experts in the symplectic/Lie quantization (Shlomo Sternberg is in Israel and I do not know whether MIT-or other colleagues are interested). Any advice would be appreciated (I was personally considering Jedrzej Sniatycki, subject to the approval of the other members of our group). As far as the participations of theoretical physicists are concerned we give priority to those acknowledging the importance of mathematical rigour.

In closing, I would appreciate whether you could share with Professor Chernoff the two volumes of the Proceedings of the Second Workshop. I do not have another complimentary copy at this moment.

Sincerely

Ruggero Maria Santilli

* please see pages 1800-1814 of Part A of the Proceedings.

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332
(404) 894-5201

SCHOOL OF PHYSICS

February 7, 1980

Professor R. M. Santilli
Department of Mathematics
Harvard University
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Thank you for your letter of January 8, 1980, on the question of the consistency of quantum theories. I am sorry I cannot entirely agree with your conclusions.

It is my understanding that what you are challenging is actually not the consistency of, say, the quantum theory of the quartic anharmonic oscillator, but of the prescription called canonical quantization for obtaining this theory. Let me know, please, if I mistake your thought.

Since I have considered canonical quantization part of the historical development, not a formal part of the theory, I am not surprised by the result and do not consider it a serious criticism. I don't understand why there should be a prescription for deriving new true theories from old false ones.

At the same time, I agree with much of your conclusion. I do not believe any of the field theories we publish, no matter what their origin, on the grounds of their unphysical assumptions about the small-scale structure of time space. In this region nothing like canonical quantization seems physically correct to me even in cases where it is, or can be made, mathematically consistent.

Forgive this note of discord, but I thought you would prefer even a slightly disputatious response to total silence.

Yours sincerely,

David Finkelstein
Director

DF:ah

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ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 11, 1980

Professor DAVID FINKELSTEIN
Editor
International Journal of Theoretical Physics
Georgia Institute of Technology
ATLANTA Georgia 30332

Dear David,

I would like to express my sincere appreciation for the courtesy of your letter of February 7, 1980. I am particularly grateful for the critical content of your letter. In fact, I am seeking advice, not approval.

Perhaps my letter to colleagues-editors of January 8, 1930 is not completely clear. In fact, I am not challenging quantum mechanics, first of all, because I am not the author of the theorems quoted in the letter and, secondly, because I believe the problem is bigger than the capability of one isolated scolar.

In essence, I have been intrigued by the theorems because they constitute, in my view, such a new problem, to call for the attention of editors. Permit me to indicate a few points in more detail. I am taking the liberty of enclosing a xerox copy of my review (regrettably, I do not have copies of the complete work), as well as of the pages from Abraham and Marsden you certainly have.

As you can see, Heisenberg's time evolution law $\dot{A}|\rangle = (AH-HA)|\rangle$ is intrinsically "inconsistent" in the sense that it can take different values, depending on the selected use of the differential rule.

Permit me the liberty of confessing that, as an editor, I do not know how to handle papers activating this occurrence (whenever A and/or H are of polynomial order in r and p of at least order three). In fact, it is not a case of infinities, as in quantum electrodynamics. Instead, it is a case much along the indeterminacy o/o. Its removal by subtration would be a sort of mumbo jumbo hand waving merely intended to preserve old doctrines beyond the limits of rationale. The selection of only one use of the differential rule is also mumbo-jambo in my view because, e.g., the numbers in Heisenberg's and Schrödinger's representation would not necessarily agree. And so on. Should I accept or reject a paper activating this non-sense?

In addition, I do not know how to handle the inequivalence between Lagrange's and Heisenberg's equations. I have heard here numerous mumbo jumbos beyond the imagination. One fellows said that we should assume Heisenberg's equations as the true one. But then QCD is dead. Another fellow said the opposite. But then QM is dead. Again, I do not honestly know how to handle this point as an editor. Should I accept or reject?

Another clearly proved inconsistency is the correspondence limit to Hamilton (see the enclosed simple proof). Additional mumbo jumbos reached my ears in this respect. One fellow said that QM should not admit a classical limit. But then the QM anharmonic oscillator should not be called anharmonic oscillator. Also, Schrödinger's equation verifies beautifully (the selfconsistency as well as) the classical limit (all the difficulties are in the use of the Lie algebra with the trivial product ab-ba and the symplectic geometry in canonical realization).

Dear David, I hope you can see that the problems are more serious of what a first look might indicate, and, more insidiously, I do not know of any way out which preserves old doctrines.

At the third workshop we shall gather a restricted number of mathematicians and physicists (max 20-25)to look at this situation without any preconceived idea. During the second workshop we truly enjoyed ourselves (it was a relaxed, completely informal, friendly meeting without speeches). We shall do our best to preserve this atmosphere for the third. In case you can attend, you would be sincerely welcomed.

Needless to say, the problem is created by the familiar (to both of us)desire by physicists to preserve old doctrines as much as possible, or to react with skepticisms prior to a technical study of the literature. If QM is abandoned for nonlinear systems (read, in my personal view, strong interactions), and a covering approach specifically conceived for the situation at hand is constructed, we believe that the Lie-admissible formulations permit to bypass all the inconsistencies. But this is one aspect in which I am not expecting we can reach a general agreement. Regrettably, I do not have a complimentary copy of the Proceedings of the Second Workshop devoted to this latter aspect (two volumes for over 1,500 pages). The request was quite substantial, and all complimentary copies were committed months before their appearance.

I can however, mail to you my personal copy, with the understanding that you will return it to me at your convenience after a few weeks (you would be free to make xerox copies). In case you are interested in looking at the efforts of generalizing old stuff, please let me know.

Again, I would like to encourage you to express critical views, rather than approval of my own view. It is only in this way that we can reach a matury, joint, judgment, which is the primary objective of my letter of January 8, 1980. On one thing I hope we do not disagree: the problems are real indeed.

Sincerely



Ruggero Maria Santilli
Editor
Hadronic Journal

RMS/ml encls.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 11, 1980

Professor D. FINKELSTEIN
Georgia Institute of Technology

Dear David,

I read again your letter of February 7, and my aswer to you of this morning. Perhaps my asnwer was only partial to your sound question. The canonical quantization is only part of our concern. As a mattero of fact, it is the minor reason of concern. On the contrary, the primary problematic aspects are due to the intrinsic, quantum mechanical, inconsistencies of Heisenberg's time evolution, as indicated in my letter of this morning and in its enclosures. We are also interested in the canonical quantization for a number of reasons, the most intriguing (for us) being the fact that it identifies a possible reason for the problematic aspects (the lack of preservation of the nonassociative character of the envelope of the Poisson brackets), by therefore setting the way for generalized formulations which are capable of bypassing all the inconsistencies, including quantization.

I am in full agreement with you that there should not be necessarily prescriptions for the construction of new, true theories from falso ones. As a result, we are in full agreement on the main point of your letter, that troubles in canonical quantizations should not be reasons for major concerns when considered per se.

The reasons for concern are due to the fact that the problems with canonical quantization are only part of a recently emerged array of problems of consistency in the intrinsic, quantum mechanical setting.

Also, problems of canonical quantization becomes problems of consistency because the same problems, to our (rather limited understanding at this moment) are absent in the allegedly equivalent Schrödinger's representation (that is, as indicated in my letter of this morning , no inconsistency has emerged in the correspondence limit of Schrödinger's equation).

Again, please accept the sentiments of my appreciation for your kind letter of February 7.

Sincerely

Ruggero

May 1, 1980

Dr. DEREK C. BOK, President, Harvard University

Dear Dr. Bok,

My research contract under DOE support expires on June 1, 1980. Before leaving your campus, I would like to express again my concern for numerous aspects of my case.

You will recall that the primary objective of my research, as well as of my editorship of the HADRONIC JOURNAL, is to study and promote the direct experimental verification of the validity or invalidity of conventional laws for the strong interactions. The need for such verification has been confirmed by numerous scientists of proved ethical standard, and it is due to the fact that conventional laws are expected to be assumed in the data elaboration of current experiments. These experiments, therefore, are not expected to test the assumptions, and new, direct experimental verifications are needed. At any rate, the mere existence of controversies in the issue is sufficient to warrant the direct experimental verifications, owing to the fundamental character of the problem.

As you will recall too, a number of senior Harvard physicists have expressed their personal view that these studies have "no physical value". These physicists are free to express their opinions. At the administrative level, however, the issue is different. In fact, Harvard is currently using several millions of dollars of taxpayer's money in research on strong interactions at the nuclear, hadronic, and astrophysical level. It is essential, in my view, that Harvard gives clear proof of a well balanced use of such large public funds. On the contrary, all available indications points toward the preference by Harvard administrators at this time of the personal opinions by senior physicist toward the termination of the study here of the problem of the basic laws.

In fact, you will recall the escalation of opposition here against my studies. This opposition initiated with initiatives by individual senior physicists, and it is now continuing with the apparent support of Harvard administrators. In particular, you should recall the proposal made by DOE to Dr. Hironaka of the Dept. of Math., to the effect that I should be considered for a guest status here during next year while we complete the transfer of my grant and research to another institution. After all, Harvard has created and is continuing to create considerable problems in the pursue of a knowledge which is fundamental for energy related issues, such as the controlled fusion. It is only fair to expect that Harvard will assit in their orderly solution.

But, quite frankly, the extreme of occurrences in my case appear to indicate an almost desire by Harvard administrator to prevent an orderly solution.

I feel obliged to bring to your attention a rather similar case. Recently, an internal controversy occurred at the CERN laboratories in Geneva, Switzerland. Individual permanent members expressed their personal opinions of rather doubtful inspiration. They are allowed to do so, and they are still there. Yet, Dr. Van Hove, director of CERN, had to resign. I doubt whether senior members here know the true reasons for this resignation.

As stressed to you a number of times, I have provided my sincere best efforts to be loyal to Harvard. This has meant for me to keep the utmost possible silence with outsiders, while informing Harvard administrators of the problems, and while resisting internal recommendation by apparently outraged senior members. Yet, the situation is now grossly out of my control due to the escalation of opposition here. I simply do not know what will happen when the study on the basic laws will be terminated here, and all the several millions of dollars of public funds will be restrictively administered along the academic views (and interests) of current senior members.

Owing to this situation I would like to ask for the possibility that I visit you at any time of your convenience. Besides the pleasure of meeting you, I would like the possibility of confirming verbally to you my sincere desire for an orderly solution.. I believe that our joint study of the situation and of possible solutions can only be beneficial to Harvard, as well as to both of us.

Very Truly Yours

Ruggero Maria Santilli

[redacted]

Tel 495 3352 (office)

[redacted]

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 2, 1980

Professor Ruggero Santilli
Department of Mathematics
Harvard University

Dear Dr. Santilli:

According to my letter of February 12, 1980 which you clearly received and acknowledged in your letter of April 25, 1980, your status at Harvard is to be totally ceased on May 31, 1980.

Therefore you have no right whatsoever to call for a meeting or conference, academic or otherwise, to be held on the premises of Harvard University after the date of the termination of your appointment, unless you were to obtain special permission from the appropriate administrative board of Harvard University. In any event, you have no authorization and no recommendation from our Mathematics Department for the Hadron Workshop to be held at the Science Center during the summer after May 31.

Sincerely yours,

Heisuke Hironaka
Chairman

HH/mjm

cc: Dean Richard G. Leahy

Enclosures

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 8, 1980

Dr. DEREK C. BOK, President
Harvard University

Dear Dr. Bok,

I am here respectfully applying to be your personal guest from June 1, 1980 until June 1, 1981. A guest status with any other member of the Harvard community designated by you would be equally appreciated.

Some of the understandings for this possible guest status are the following.

1. The guest status should be a gesture of generosity on the part of Harvard University to assist me in the transfer of my research to another university, but under no circumstance it is expected to be renewed or extended beyond June 1, 1981.

2. The guest status should be completely without compensation. In addition, I should pay out of my research funds all logistic expenses (such as xeroxcopies), in such a way that no direct or indirect expense should be encourred by Harvard University on my behalf.

3. The guest status should essentially allow me to use Harvard libraries and parking facilities, as well as serve as my scientific address in the form of this stationary or any other form recommended.

4. The use of an office anywhere on campus would be appreciated but it is not essential.

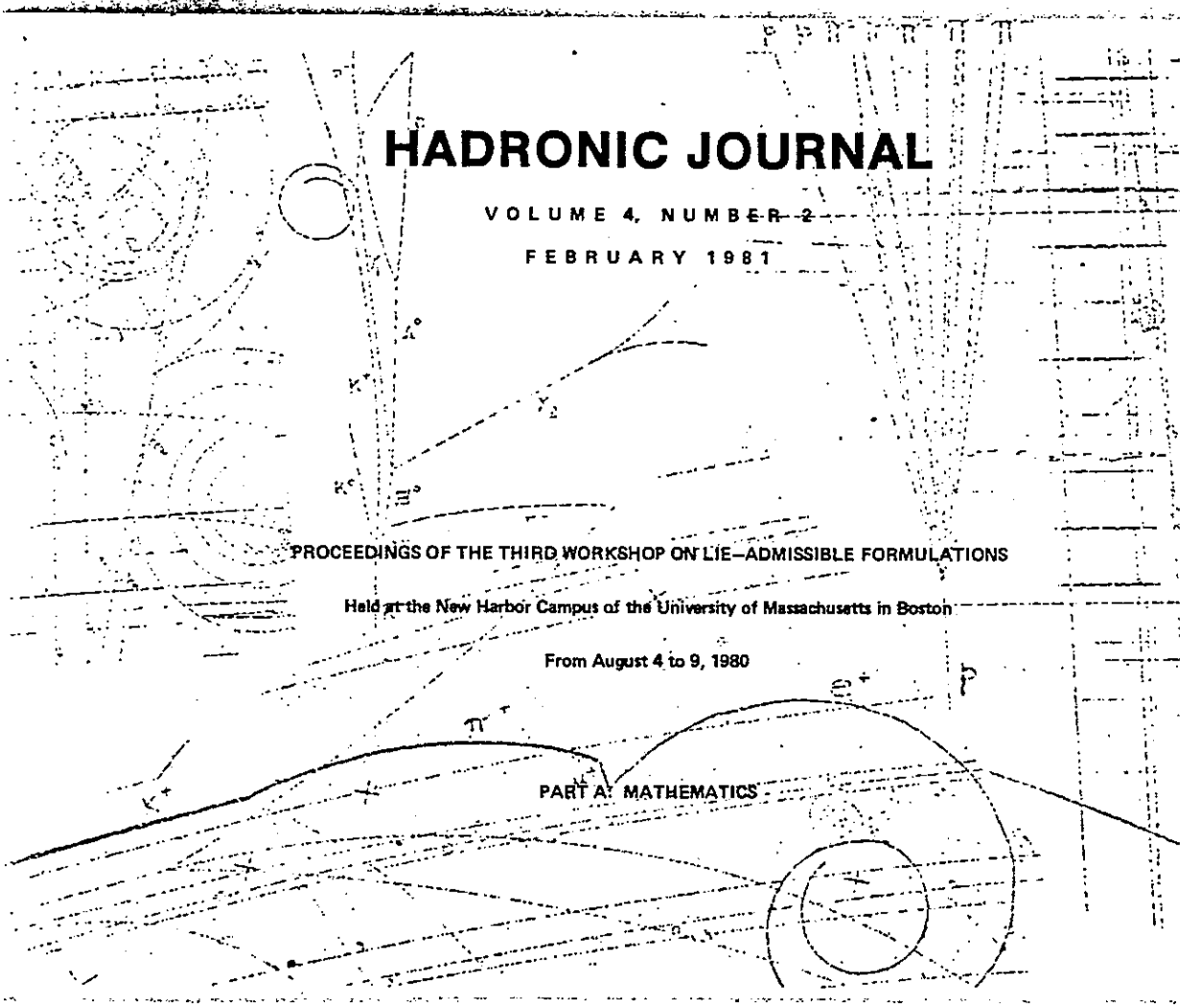
5. The guest status should allow the conduction at Harvard of the Third Workshop on Lie-admissible formulations tentatively scheduled from August 4 to 9, 1980 (but the Forth workshop scheduled for August 1981 should be conducted elsewhere). A list of selected, distinguished scientists interested to attend the Third workshop of this coming August is enclosed for your information.

I would consider it a personal courtesy whether a formal action on this application is reached before the expiration of my current research position here on May 31, 1980.

Very Truly Yours

Ruggero Maria Santilli

C.C. Dr. RICHARD G. LEAHY
Associate Dean



HADRONIC JOURNAL

VOLUME 4, NUMBER 2

FEBRUARY 1981

PROCEEDINGS OF THE THIRD WORKSHOP ON LIE–ADMISSIBLE FORMULATIONS

Held at the New Harbor Campus of the University of Massachusetts in Boston

From August 4 to 9, 1980

PART A: MATHEMATICS

HADRONIC JOURNAL

Volume 4, Number 2, 1981

PROCEEDINGS OF THE THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS
Held at New Harbor Campus of the University of Massachusetts in Boston
from August 4 - 9, 1980

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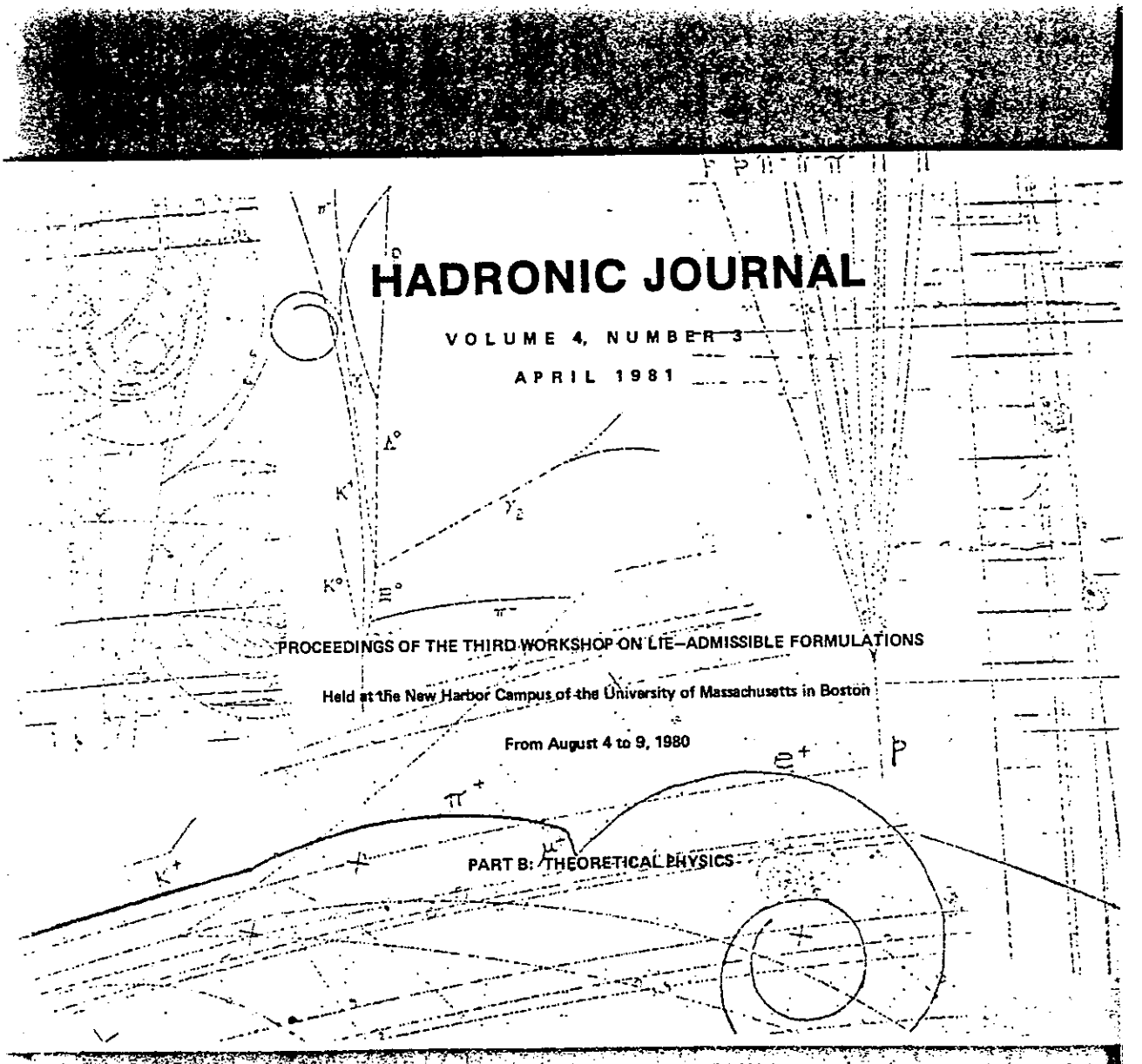
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* Corresponding participants

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VOLUME 4, NUMBER 3

APRIL 1981

PROCEEDINGS OF THE THIRD WORKSHOP ON LIE–ADMISSIBLE FORMULATIONS

Held at the New Harbor Campus of the University of Massachusetts in Boston

From August 4 to 9, 1980

PART B: THEORETICAL PHYSICS

HADRONIC JOURNAL

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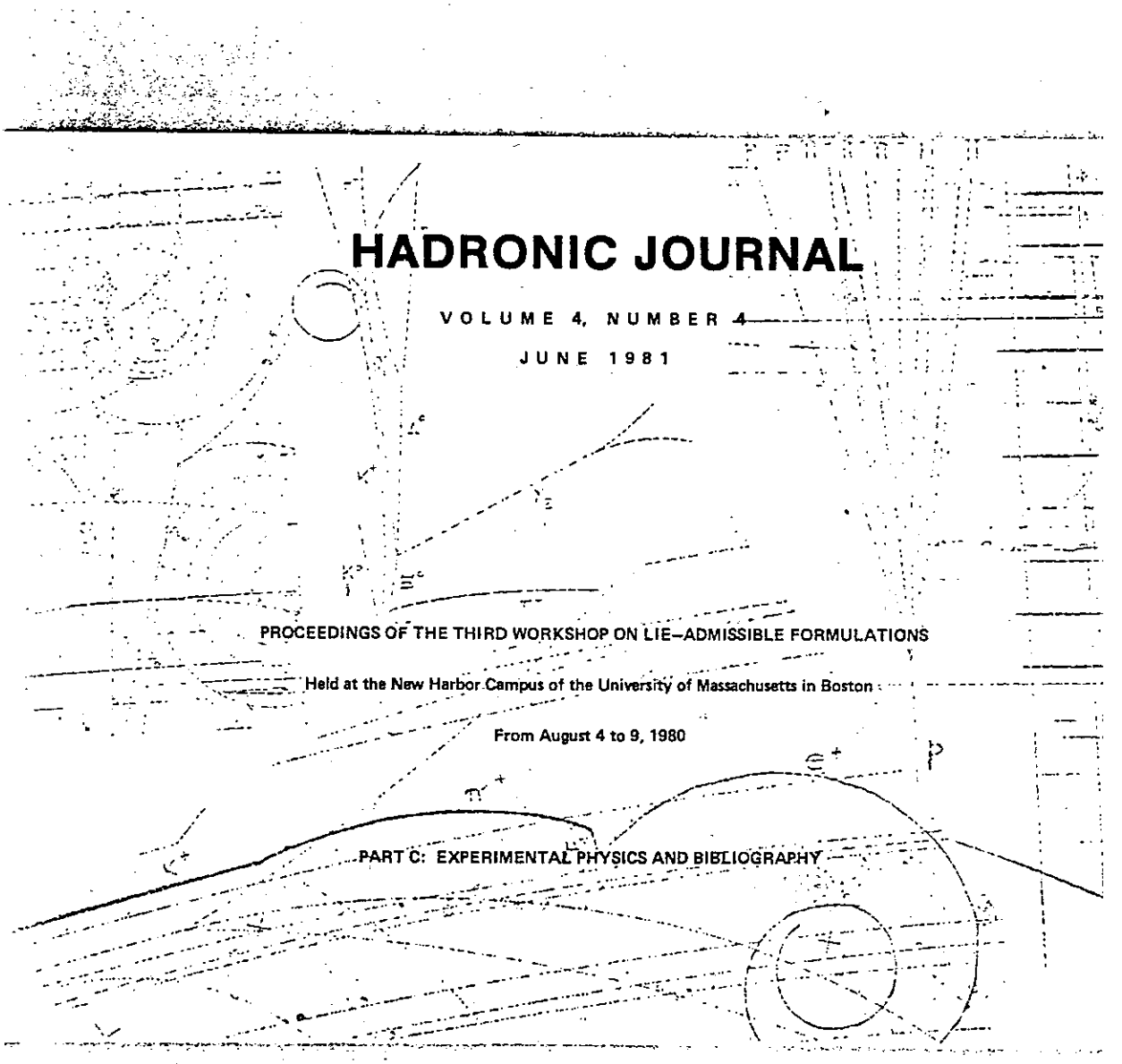
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* Corresponding participants

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VOLUME 4, NUMBER 4

JUNE 1981

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R. CALOI,* L. CASANA,* M.P. DE PASCALE,* M. MATTIOLI,* E. POLDI,* C. SCHAERF,* and M. VANNI* Universita degli Studi, Istituto di Fisica ed I.N.F.N., I-00185 Roma, Italy and
P. PELFER* and D. PROSPERI,* Universita degli Studi, Istituto di Fisica ed I.N.F.N., I-80138 Napoli, Italy and
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* Corresponding participants

The Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY under contract number DE-AC02-80ER10651

PART II:

TUFTS

UNIVERSITY

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 9, 1980

Professor A.E.Everett
Department of Physics
Tufts University
Medford, Ma 02155

Dear Professor Everett,

I am currently seeking a tenured position in physics. In case your department is interested in my candidacy, I enclose my curriculum, a list of references, and some illustrative material on my recent teaching-research-editorial activities.

My salary is currently supported in full by my research grant with the Department of Energy. The main topic of study is to appeal to advanced mathematical and physical knowledge for the formulation of experiments on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic interactions. This expected long range support is at its second year, and it is possible that the funds will increase in time, depending on our progress toward achivment of maturity of formulation of experiments. The expected indirect, energy-related relevance of our studies is for the controlled fusion and other aspects.

The studies are now conducted by a group of distinguished mathematicians and physicists, besides myself, and they are coordinated via (a) a yearly workshop (now at its third year); (b) a yearly series of reprint volumes edited by Professors H.C.Myung, S. Okubo, and myself (Volumes 3, 4, 5 are in press); and (c) the Hadronic Journal of which I am the editor in chief.

The new mathematical tools stimulated by these studies have also seen applications in areas other than that of the basic laws of the strong interactions. For instance, the integrability conditions for the existence of a Lagrangian or a Hamiltonian I have presented in my volume "Foundations of Theoretical Mechanics", with Springer-Verlag, are now applied in space mechanics, plasma physics and engineering. Similarly, the Lie-admissible generalization of Lie's theory currently under way for (local and nonlocal) forces nonderivable from a potential, even though still at the beginning, has already seen applications to statistical mechanics and other areas.

Quite intriguing appear to be two theorems of invalidation of Heisenberg's equations for all Hamiltonians of polynomial order in the canonical operators r and p higher than two. These theorems (primarily studied by mathematicians and still largely unknown in physics circles) have rather predictable implications in several current lines of studies. Perhaps, these theorems may interest some of your physicists.

In case you are interested in a review of these new trends, please let me know, and I shall do my best to visit your Department at some time of mutual convenience. In the meantime, I remain at your disposal for additional information you might desire.

Best Personal Regards

Ruggero Maria Santilli

RMS|ml; encls.

TUFTS UNIVERSITY

Department of Physics

January 23, 1980

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli:

Thank you for your inquiry about the possibility of a position at Tufts. I regret to say that I do not foresee our being able to make a tenured appointment in the area of theoretical particle physics any time in the foreseeable future.

Sincerely yours,

Allen E. Everett
Professor and Chairman
Department of Physics

AEE:rf

Medford, Massachusetts 02155
617 628-5000

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 5, 1980

Professor ALLEN E. EVERETT
Chairman
Department of Physics
TUFT UNIVERSITY
MEDFORD, Massachusetts 02155

Dear Professor Everett,

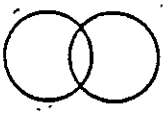
I would be interested in a position of unpaid visitor or guest at your Department for next academic year, possibly, beginning from June 1980. Besides expecting no salary, I can pay logistic expenses (xerox, etc.) from my own research funds, so that no financial expense is supported by your Department on my behalf. I do not expect participation to your faculty meetings, although I would be happy to provide services upon request, within the limitation of my time, including teaching.

As you eventually remember from our recent correspondence, I am fully supported by a grant from the Department of Energy. It appears that DOE has accepted my proposal to have my grant administered by a non-academic Institution (which, in my case would be much easier since I lack a permanent position). Therefore, I do not have salary problems. Yet, Il would be a pleasure for me to be associated with Tuft. Also, the production facilities of the Hadronic Journal (of which, as you will remember, I am the editor in chief) are quite close to your campus, with clear advantages for me as far as commuting is concerned. Jointly, it may be that the various scientific activities at our Journal could be of value for your Department.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 19, 1992

Ms. CELIA MEES
Editor
Boston Area Physics Calendar
Tufts University
Physics Department
MEDFORD, Massachusetts 02155

Dear Ms. Mees,

Please list in the Calendar the following seminar

FRIDAY, APRIL 30

The Institute for Basic Research

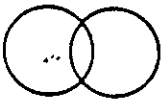
2.30 - Harvard Grounds, 96 Prescott Street
(next to Fogg and GSD, entrance at the left court)
Algebraic identities, vector fields, and coordinate changes

Prof. [redacted], Univ. of [redacted], Dept. of Mathematics, and IBR, Division of Mathematics.

Thank you.

Very Truly Yours

Ruggero Maria Santilli
President
RMS-mlw



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

FINAL NOTICE

PLEASE POST PLEASE POST PLEASE POST

SEMINAR

FRIDAY, APRIL 30

2.30 p.m.

Algebraic identities, vector fields
and coordinate changes

Professor [redacted]
University of [redacted], Department of Mathematics
and IBR, Division of Mathematics

Abstract

The extension of Lie groups to analytic H-spaces (manifolds with multiplication having identity element) requires the use of general nonassociative algebras in their analysis. In this lecture, anticommutative algebras are emphasized relative to tangent algebras, generalized Campbell-Hausdorff formulas, and connections. Also, various algebraic identities extending associativity on H-spaces are discussed in terms of invariant vector fields and coordinate changes.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 20, 1982

Dr. JACK SCHNEPS
Chairman
Department of Physics
Tufts University
Medford, Massachusetts 02155

CERTIFIED

Dear Dr. Schneps,

I phoned this morning to Ms. CELIA MEES, Editor of the "Boston Area Physics Calendar" to ask the listing of a forthcoming seminar by Professor [redacted] who, as you eventually know, is a distinguished mathematician of the University of [redacted] Copy of the intended announcement is enclosed.

To my considerable surprise, Ms. Mess informed me that she had received prohibition to list seminars from our Institute personally from you, and that I should contact you to know the reasons.

Following immediate consultations with our Board of Governors, I was instructed to write you this letter asking:

(1) The reasons for your prohibition that our Institute lists seminars in the Boston Area Physics Calendar;

(2) The clarification whether this was your personal action, or the action by Tufts University, or a joint action with other local colleges. In this latter case, please indicate the names of individual officers and colleges who participated in the decision.

(3) A detailed suggestion of procedures our Institute must comply with to have the listing of seminars, and the list of other Institutes currently excluded.

I do not know whether you realize the scientific gravity of the occurrence, or our determination to resolve it in the interest of the free and genuine pursue of knowledge, let alone to protect the dignity or the distinguished scientists who are damaged by the occurrence. I would like, therefore, to suggest that you take all the necessary appropriate actions. Lacking your answer in the near future, we will be forced to initiate our own actions without any further prior communication with you.

Very Truly Yours

Ruggero Maria Santilli
President
RMS-mlw
cc.: Professor [redacted]
WASSERMAN & SALTER, Law Firm of the IBR
Board of Governors, IBR
Ms. Celia Mess, TuftsUniv.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 22, 1982

TO SELECTED MEMBERS OF THE IBR CONFIDENTIAL

I feel obliged to report to you an expisode of rather vulgar academic greed typical of the Cambridge academic community, that has damaged the dignity of a member of our Institute, and will damage all members, unless prompt action is immediately undertaken.

On April 19, 1982, a distinguished mathematician, member of our Institute, was denied the listing of his seminar in the "Boston Area Physics Calendar". I subsequently contacted

Dr. JACK SCHNEPS, Chairman, Department of Physics, Tufts University, Medford, Ma 02155, tel (617) 628 5000, extension 3383

who is responsible for the editing of the Calendar. He indicated to me that he was acting under specific instruction by

Dr. KARL STRAUCH, Chairman, Lyman Laboratory of Physics, Harvard University Cambridge, Massachusetts, 02138, tel (617) 495 2844

as well as a number of faculty at the Lyman Laboratory. Schneps indicated that Strauch and his friends

opposed all listings of seminars from our Institute in the Boston Area Physics Calendar

This conclusion was reached on account of the facts that
(a) the rejection of the listings persists even when the seminars were for specifically indicated talks of physical orientation (the Calendar is for physical talks, whether theoretical or experimental);
(b) the rejection of the listings persists even under our best cooperation for the words of the listings. For instance, In the listings of the invited talk by Professors Myung and Schober we had in 1981 at our Institute, we indicated for logistic reasons that the talk would occur at "The Prescott House on Harvard Grounds". I therefore indicated to Dr. Schneps that if the indication of the location of the Prescott House would bother the academic greed at Harvard, it would be eliminated. The asnwer was that the prohibition to list the seminar persisted no matter what.
(c) The prohibition persists until Dr. Strauch and his [redacted] formally withdraw the opposition to the listings.

This situation, as you can see, implies great offense to the dignity of truly distinguished scholar who will visit out Institute (including experimentalists who are scheduled here within one or two months).

As a result of this situation, and after having exhausted all friendly attempts to solve it quietly, our Board of Governors has instructed me to initiate the study of a possible massive action against alleged [redacted] at Harvard University and at Tuft University which may involve:

- the filing of law suits for damages, currently under study by our Law Firm;
- an open written accusation of alleged [redacted] at Harvard University and at Tufts University by me as an individual to be mailed world wide;
- a formal request of initiation of investigations on the case (as well as numerous related aspects) to be filed by me as President of the IBR, under a formal vote by the Board of Governors, to the U.S.Senate, the American Institute of Physics, and other National Bodies.

To understand the episode in its true light, you should be aware of the extremes of academic greeds that eventually resulted in the creation of our new, independent Institute. We have absteined from disclosing them to you because, after all, they are of such gross humanity to be unbeliavable. This last episode is not, therefore, isolated. It is nothing else than a small episode in a much more serious chain.

For the very survival of the Institute, it is therefore essential that a halt to these occurrences be initiated once and for all. Now is the time.

This communication is intended to reassure you that, in case we do indeed decide for a massive, frontal attack, you will be informed individually with sufficient notice. Also, I would like to reassure you that your association with our Institute has not been disclosed by us, and that the confidentiality has been guarded to our best.

In case you are outraged of the enormity of the case, please feel free to write, or, better, call Drs.Schneps and Strauch and express your support for our Institute. However, I beg you not to feel obliged to do so. Also, it would be appropriate for you to contact me prior to any action, in order to be informed of latest developments.

The morale is the following. The studies of primary relevance for our Institute, that is:
- the Lie-admissible generalization of Lie's theory; and
- the consequential generalization of the atomic mechanics,

are against the "vested academic interests" of the departments of mathematics and physics at Harvard, MIT, Tufts and other universities. This has resulted in the truly unbelievable difficulties we have encountered in the past. Until now we have managed to go ahead with a tolerant, humanly decent response. Apparently, this has been counterproductive, in the sense that it has favored excesses, including alleged pressures in Washington to prevent the funding of our programs.

It is clear that, unless we succeed in containing this academic greed we will go nowhere.

Sincerely,

Ruggero Maria Santilli

The University of [redacted]

[redacted]



April 30, 1982

Professor Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Ruggero:

I intend, with your approval, to send the following letter to Tufts and Harvard.

> It has come to my attention that you may possibly have adopted a posture, relative to the Institute of Basic Research in Cambridge and the research efforts it supports, that runs counter to the traditional academic attitude towards open inquiry of all new ideas. I would appreciate very much in knowing if this is correct, and if so, on what grounds this position is predicated.
>
> If any thing new has developed please let me know.

Sincerely,

[redacted]
Department of Mathematics

bs

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

May 20, 1982

Office of the President

Dr. JACK SCHNEPS
Chairman
Department of Physics
Tufts University
MEDFORD, Massachusetts 02155

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

Dear Dr. Schneps,

I am hereby asking that you list the following seminar in the Boston Area Physics Calendar for the week of May 16—21, 1982

WEDNESDAY, MAY 19

The Institute for Basic Research

2:30 p.m. — Enter at the left court of the Prescott House on Harvard Grounds at 96 Prescott Street, Cambridge (tel. 864 9859)
Experimental and theoretical reasons why I do not believe in quarks
Ruggero Maria Santilli, IBR, Division of Physics

Please note the following:

(1) This letter will reach you with plenty of time prior to the deadline for listings in the Calendar (1:00 p.m. Monday, May 10, 1982).

(2) In case the indication of the logistics of the Prescott House in the grounds of Mr. Harvard, to facilitate colleagues, is unwelcome, simply remove the words "Harvard Grounds".

(3) Following my conversation with Ms. CELIA MEES of April 19, 1982, and subsequent phone conversation with you on the same day, it is our understanding that you have accepted a formal request by the Chairman of the Lyman Laboratory of Physics at Harvard, Dr. KARL STRAUCH, as well as additional faculty there (apparently Drs. S. GLASHOW and S. COLEMAN, as well as others) not to list seminars organized by our Institute, irrespective of (a) the scientific status of the speakers; (b) its specific physical nature and (c) our conciliatory attitude toward the wording of the listings. You are therefore sharing with the indicated persons and institutions the responsibility of the act.

I urge you to withdraw from this apparent, scientifically insane behaviour, and list our seminars in exactly the same way as seminars are listed at your Department, Harvard, MIT and other local institutions, in the genuine spirit of the free pursuit of knowledge, as well as of this Land. I hope you understand the gravity of the gesture, and the reactions that, regrettably our Institute, as well as its numerous members scattered throughout the world, may be forced to implement.

Very truly yours,

Ruggero Maria Santilli
President
RMS/mlw

cc: Law Firm of the IBR
Board of Governors, IBR
All members of the Divisions of Physics and Mathematics, IBR
Ms. Celia Mees, Tufts Univ.

NOTE OF 05/01/84:
THIS SEMINAR WAS NOT LISTED

THE BOSTON AREA PHYSICS CALENDAR

The Boston Area Physics Calendar is produced weekly during the Academic Year by the Physics Department, Tufts University. Announcements should be mailed to Tufts or called in to (617)-628-5000 x2295/3393 no later than 1:00pm on the Monday preceding the week of the talk.

* - *

March 21 - March 25

NOTE OF 5/1/84: SAMPLE LISTING OF SEMINAR IN APPLIED MATH.

MONDAY, MARCH 21

Boston University

12:00 - Bag lunch, Room 235, 111 Cummington Street
12:15 - Center for Polymer Studies Seminar, Room 235
Experimental Determination of Critical Exponents near the Gelation Threshold
Prof. Izumi Nishio, Boston University

Boston University

4:00 - Astrophysics Seminar, Room 506, CLA Building
OH Masers in Compact H II Regions
Mark Reid, Center for Astrophysics

Harvard University

4:00 - Tea, Jefferson 461
4:30 - Physics Colloquium, Jefferson 250
Sources of Gravitational Radiation
Prof. Douglas Eardley, Harvard Astrophysical Laboratory

TUESDAY, MARCH 22

Brandeis University

3:30 - Coffee and cookies, Physics Building, Bass 333
4:00 - ***Please note new time**** Physics Colloquium, Nathan Goldstein Lecture Hall, Abelson 131
Science as Intellectual Property
Prof. Dorothy Nelkin, Cornell

Massachusetts Institute of Technology

3:45 - Refreshments, Marlar Lounge, 37-252
4:15 - Astrophysics Colloquium, Marlar Lounge
The Orion-KL Infrared Cavity
Prof. Reinhard Genzel, University of California, Berkeley

WEDNESDAY, MARCH 23

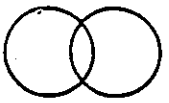
Harvard University

3:00 - Mathematical Physics Seminar, Jefferson 256
Supermanifolds Continued
J. Bernstein, Harvard

PART III:

BOSTON

COLLEGE



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

October 31, 1983

Ms. S. LYNCH
Editor
The Boston Area Physics Calendar
Department of Physics
Boston College
CHESTNUT HILL, MA 02167

Dear Ms. Lynch,

This past Tuesday, October 25, 1983, I contacted Barbara at your office to see whether we could list a forthcoming seminar of Professor [redacted] of [redacted] University, New York, under the title

"Doubly–linked ring networks and computer communications"

to be delivered at our Institute on November 4, 1983.

Barbara informed me that the deadline for the appearance of announcements for the first week of November had passed, and that the Calendar had already been printed and mailed out.

I therefore asked Barbara for the courtesy of a copy of the mailing list of the Calendar, to be used by our Institute for the mailing of the enclosed announcement of Professor Hsu's seminar. I stressed that the cost of copying the mailing list will be paid by our Institute.

Barbara indicated that she had no authority on the matter and that she had to contact you. She also promised to let me know soon, owing to the urgency of the matter. Since that time (Tuesday morning) I have contacted Barbara a number of times without any result. Each time she repeated that she had to contact you, or that you were out for lunch, or the like.

The possibility of using your list for the announcement of Professor [redacted] talk is now lost. Nevertheless, the situation may well repeat itself in the future.

Please let me have a xerox copy of the mailing list, jointly with your bill for the related expenditures, so that we can expediciously use it in the future for the announcement of advanced talks in physics and applied mathematics by distinguished scholars.

In case, for any reason, you do not wish to release a copy of this list, please do not be afraid to say so. The important background issue you should be aware of is that the policy, whatever it is, is applied in exactly the same manner to all institutions, in order to prevent discrimination in the propagation of scientific information, particularly when conducted under governmental support.

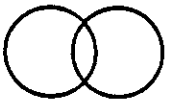
Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

cc: Dr. R. Uritam, Chairman, Department of Physics, Boston College

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859



PLEASE POST PLEASE POST PLEASE POST

SEMINAR IN APPLIED MATHEMATICS

FRIDAY, NOVEMBER 4, 1983

3:00 p.m.

Doubly—Linked Ring Netwoks and
Computer Communications

Professor [redacted]
[redacted] University
[redacted]

BOSTON COLLEGE
CHESTNUT HILL, MASSACHUSETTS 02167

DEPARTMENT OF PHYSICS
(617) 552-3575

November 9, 1983

Dr. Ruggero M. Santilli
President
The Institute for Basic Research
Harvard Grounds, 96 Prescott St.
Cambridge, Mass. 02138

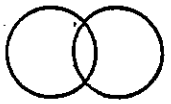
Dear Dr. Santilli:

In reply to your letter of October 31, 1983, the mailing list for the Boston Area Physics Calendar is not a matter of public record and is not available for distribution. The list represents the names of persons and institutions that have subsribed to the Calendar, and exists for the benefit of the Editor of the Calendar in his task of distributing weekly issues of the Calendar.

Sincerely,

Shirley Lynch
Administrative Assistant

SL/mr



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

November 22, 1983

Miss SHIRLEY LYNCH
Administrative Assistant
Boston College
Department of Physics
CHESTNUT HILL, Massachusetts 02167

Dear Miss Lynch,

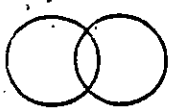
I appreciated the courtesy of your letter of November 9, 1983. Please be assured that I respect your decision concerning the lack of availability of the mailing list of the Physics Calendar.

Permit me, however, to suggest, if at all needed, that the same policy be strictly inforced for all members of the Boston physics community. In fact, I have been hearing repeated rumors that discrimination of research under governmental support has apparently occurred in the editing of the Calendar by other universities and that the matter is under consideration for possible submission to the appropriate legislative body in Washington.

Very truly yours,

Ruggero M. Santilli

RMS/mlw



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

March 7, 1984

Ms. S. LYNCH, Editor
BOSTON AREA PHYSICS CALENDAR
Department of Physics
Boston College
Chestnut Hill, Massachusetts 02167

Dear Ms. Lynch,

please include in the calendar the following talk.

MONDAY, March 26
The Institute for Basic Research
96 Prescott Street, Cambridge, tel. 864 9859
12:30 - Theoretical Physics Seminar, IBR Hall
Problematic aspects of general relativity for planetary orbits
Dr. H. Yilmaz, Hamamatsu Photonics, Japan

Thank you

Very Truly Yours

Ruggero M. Santilli
President
RMS-mlw

cc. Dr. Yilmaz
105 Church Street
Winchester, Ma 01890

THE BOSTON AREA PHYSICS CALENDAR

The Boston Area Physics Calendar is produced weekly during the Academic Year by the Physics Department, Boston College. Announcements should be mailed to Boston College or called in to (617) 552-3575 no later than 1:00 p.m. on the Monday preceding the week of the talk.

-

March 26 - 30, 1984

TUESDAY, MARCH 27

Brandeis University

2:00 - Theoretical Seminar, Yalem 229
Confinement in QCD
Professor R. Roskies, University of Pittsburgh

Massachusetts Institute of Technology

4:00 - Refreshments, Coffee, Kolker Room 26-414
4:15 - Nuclear Physics Seminar
Semi-Classical Studies of the Sub-Barrier Fusion Cross Section
Noboru Takigawa, Michigan State University

Boston University

8:00 p.m. - Philosophy of Science Colloquium, Room 314 of the George Sherman Union, 775 Commonwealth Avenue, near Boston University Bridge
From Quarks to the Big Bang: The Synthesis of the 1970's
Sheldon Glashow, Boston University and Harvard University

WEDNESDAY, MARCH 28

University of Lowell

3:30 - Refreshments, Coffee, Physics Faculty Lounge, OH 143
4:00 - Physics Colloquium, Olney OH 115
The Moon Paradox and Other Optical Illusions
Professor Roger McLeod, University of Lowell

Boston College

3:45 - Refreshments, Coffee, Higgins 354
4:15 - Physics Colloquium, Higgins 262
Micromechanical Devices
Dr. Paul Zavracky, Foxboro Company

Massachusetts Institute of Technology

4:00 - Refreshments, Coffee, CTP Seminar Room
4:30 - Joint Theoretical Seminar
Simulating Physics with Cellular Automata
Professor Gerard Vichniac, Massachusetts Institute of Technology

THURSDAY, MARCH 29

Harvard-Smithsonian Center for Astrophysics

3:30 - Refreshments, Tea, Phillips Auditorium, 60 Garden Street

4:00 - Scientific Colloquium

From Interstellar Grains to Comets

Professor J. Mayo Greenberg, Huygens Laboratory, University of Leiden, The Netherlands

FRIDAY, MARCH 30

Tufts University

3:30 - Refreshments, Tea, Math-Physics Library

4:00 - Physics Colloquium, Robinson 152

Heavy Particle Production in QCD

Professor Louis Clavelli, Indiana University

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

March 27, 1984

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

Ms. S. LYNCH,
Editor
BOSTON AREA PHYSICS CALENDAR
Department of Physics
Boston College
Chestnut Hill, Ma 02167

Dear Ms. Lynch,

Please include in the calendar the following announcement

MONDAY, April 16
The Institute for Basic Research
96 Prescott Street, Cambridge, Tel. 864 9859
12:30 - Theor. Phys. Seminar, bring your own lunch

... Problematic aspects of the general relativity for planetary orbits

Dr. H. Yilmaz, Hamamatsu Photonics, Japan

I understand that the preceding announcement of Dr. Yilmaz's talk, mailed to you on March 7, 1984 (via normal letter) did not reach you in time for the listing of the talk originally scheduled for March 26.

Owing to this apparent mixup, this communication is mailed to you via certified means. You should consider communicating to us reception and-or any decision as soon as possible.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

cc. Dr. Yilmaz
105 Church Street
Winchester, Ma 01890
Dr. R.A.Uritam, Chairman
Dept of Physics, Boston College

P 277 494 312

RECEIPT FOR CERTIFIED MAIL

NO INSURANCE COVERAGE PROVIDED—
NOT FOR INTERNATIONAL MAIL
(See Reverse)

SENT TO: S. LYNCH

STREET AND NO.: Dept of Physics

P.O. STATE AND ZIP CODE: Boston Geek, Mu

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THE BOSTON AREA PHYSICS CALENDAR

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-

April 16 - 20, 1984

MONDAY, APRIL 16

Harvard University

4:00 - Refreshments, Tea, Jefferson 461
4:30 - Physics Colloquium, Jefferson 250
Morris Loeb Lecture Series - Pulsars: Nature's Most Precise Clocks
Lecture #1 - Experimental Relativity: Timing the Binary Pulsar
Professor Joseph Taylor, Princeton University

Brown University

4:00 - Refreshments, Coffee, in Barus & Holley 168
4:30 - Physics Colloquium
Hunting for Quarks with 4 GeV Electrons
Prof. F. Gross, William & Mary College

TUESDAY, APRIL 17

Massachusetts Institute of Technology

4:00 - Refreshments, Coffee, Kolker Rm. 26-414
4:15 - Nuclear Physics Seminar
Photonuclear Reactions at the Bonn Electron Accelerator
Jurgen Arends, Bonn

WEDNESDAY, APRIL 18

Harvard University

1:00 - Gauge Seminar, Lyman Lab, Rm. 330
Physics of the Kaon System and Chiral Symmetry
Prof. John Donoghue, Univ. Mass, Amherst

Massachusetts Institute of Technology

3:45 - Refreshments, Tea, Coffee, & Cookies, NW16-213
4:00 - Plasma Fusion Seminar Series
ICRH Results on TMX-U
Dr. Guy Dimonte, TRW

Boston University

4:00 - Refreshments, Science Center, Rm. 121, 590 Comm. Ave.
4:15 - Physics Colloquium, Science Center, Rm. 115
Chertok Lecture
"'Maybe Diamonds Are Forever...': Results from the IMB Proton Decay Experiment"
Larry Sulak, University of Michigan

WEDNESDAY, APRIL 18 (continued)

University of Lowell
3:30 - Refreshments, Coffee, Olney Science Ctr., OH-143, Physics Faculty Lounge
4:00 - Physics Colloquium, Olney Science Ctr., OH-115
The Bates Linear Accelerator Facility and Some Experiments
Dr. Padmanabh Harihar, University of Lowell

Boston College
3:45 - Refreshments, Coffee, Higgins Hall 354
4:15 - Physics Colloquium, Higgins 262
Novel Phenomena in Superlattices
Dr. Lionel Friedman, GTE Laboratories/Boston College

Harvard University
4:00 - Refreshments, Coffee, Lyman 330
4:30 - Joint Theoretical Seminar, Jefferson 256
Discrete Gravity
Prof. T.D. Lee, Columbia University

Boston University
8:00 p.m. - Philosophy of Science Colloquium, Rm. 314 of the George Sherman Union, 775 Comm. Ave., near Boston University Bridge
God and Coherence: On the Epistemological Foundations of Religious Belief
Kai Nielson, Philosophy, University of Calgary

THURSDAY, APRIL 19

Harvard University
2:30 - Physics Colloquium, Jefferson 250
Morris Loeb Lecture Series - Pulsars: Nature's Most Precise Clocks
Lecture #2 - Clock Stability, The Early Universe, and Millisecond Pulsars
Professor Joseph Taylor, Princeton University

Massachusetts Institute of Technology
3:30 - Refreshments, Coffee, 26-110
4:00 - Physics Colloquium, 26-100
Neutrino Exploration of the Earth
Dr. Alvaro de Rujula, CERN

Harvard-Smithsonian Center for Astrophysics
3:30 - Refreshments, Tea, Phillips Auditorium, 60 Garden St., Cambridge
4:00 - Scientific Colloquium
Neutron Stars: New Results from the Japanese X-Ray Satellite TENMA
Dr. Takaya Ohashi, Leicester University - Recently of the Institute for Space & Astronomical Science, Tokyo

THURSDAY, APRIL 19 (continued)

Northeastern University
3:45 - Refreshments, Coffee, Tea, & Cookies, Dana 114
4:00 - Solid State Seminar
Sliding Charge Density Waves
Dr. Peter Littlewood, ATT Bell Laboratories

FRIDAY, APRIL 20

Massachusetts Institute of Technology
3:30 - Refreshments, Coffee, Building 9, Rm. 150
4:00 - Center for Materials Science and Engineering Colloquium
Optical Transitions and Elementary Excitations in GaAs-AlGaAs Multiple-Quantum-Well Structures
Dr. Daniel Chemla, AT&T Bell Laboratories

Massachusetts Institute of Technology
3:45 - Refreshments, Coffee, Tea, & Cookies, NW14-2209
4:00 - Plasma Fusion Seminar Series
High Power Electron Cyclotron Heating in the Tara Tandem Mirror Experiment
Michael Mauel, MIT

Harvard University - Division of Applied Sciences
4:00 - Atoms, Molecules, and Condensed Matter Seminar, Pierce Hall, Rm. 209
Thinking About Solid State Structures That Have Not Yet Been Made
Roald Hoffman, Visiting Prof., Chemistry Dept., Harvard University
Refreshments after the Seminar in the Brooks Room

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

April 11, 1984

Father DONALD J. MONAN, President
Boston College
CHESTNUT HILL, MA 02167

CERTIFIED LETTER
RETURN RECEIPT
REQUESTED

Dear Mr. President,

I feel an ethical duty to bring to your attention the fact that the Physics Department of your College has refused the listing, in the Boston Area Physics Calendar, of a seminar by Dr. H. Yilmaz entitled, "Problematic aspects of the general relativity for planetary orbits".

Copy of the pertinent material is enclosed including: copy of an initial request for the listing dated March 7, 1984, addressed to Ms. S. Lynch and mailed via ordinary first class mail; copy of a subsequent letter dated March 27, 1984, addressed also to Ms. Lynch, with copy to Dr. R. A. Uritam, Chairman of your Physics Department, this time mailed via certified letter, return receipt requested; copy of the certifications by the U. S. Post Office; and, finally, copies of the Calendar printed by your Physics Department without the listing of the seminar by Dr. Yilmaz.

Permit me the liberty of recommending most respectfully: (a) the immediate initiation of an in depth investigation of the case; (b) the termination of the employment of all persons found responsible for the event; and (c) the conduction of the investigation in a way as open to the general public as possible.

The latter suggestion is based on the fact that, on our part, we shall take all the necessary initiatives to propagate the information of this incredible occurrence, as much as conceivably possible, on a worldwide basis.

The need for this action is evident. In fact, the occurrence has all the ingredients for a possible suffocation of America in its most vital jugular vein: freedom of scientific inquiry.

My respectful suggestion for you to conduct an investigation as public and open as possible is to prevent possible shadows of complicity of your entire College.

For your information, Dr. Yilmaz is an internationally renowned, senior scientist. He has been publishing, during the past quarter of a century, in numerous technical journals certain problematic aspects of Einstein's gravitation, which now constitute an historical open problem in the field.

It is evident from the very title of the proposed talk by Dr. Yilmaz, that his studies are damaging to the vested, academic—financial—ethnic interests built throughout this century around Einstein's ideas.

Nevertheless, in my humble view, excessive leniency on these vested interests constitute a clear threat to our free society.

I promise you that the refusal by your Physics Department to list the seminar by Dr. Yilmaz on constructive scientific criticisms on Einstein's ideas will indeed be brought to worldwide attention.

It is a marvelous test case to see whether or not America is a truly free society. We shall resolve the episode in due time while the world is watching!

Most Respectfully Yours,

Ruggero M. Santilli
President
The Institute for Basic Research

encls.
cc: Dr. H. Yilmaz, 105 Church Street, Winchester, MA

encls.: 1) Correspondence with Boston College; 2) A recent paper by Dr. Yilmaz; and, 3) A description of the I. B. R.

PART IV:

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 7, 1979

Professor HERMAN FESHBACH, Chairman
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Herman,

You might be interested to know that the call for a moment of reflection on the basic physical laws currently used in hadron (and nuclear) physics, which I initiated while being a honorary guest at your Department in 1977-1978, has moved considerably ahead. I formally presented this call via a series of rather long papers in the Hadronic Journal, backed by two series of monographs, one with Springer-Verlag and one with Hadronic Press (the first two volumes were published in 1978, two additional volumes are in print and others are in preparation). This call was subsequently answered by independent researchers via papers already appeared in the literature or in print, all favorable.

Lately, I have released the enclosed review paper on the rather numerous and substantial criticisms on quark conjectures, which are moved by so many and outstanding physicists all over the world. You will be amused to know that I have released this paper for wide distribution (15,000 copies via the Hadronic Press) for the intent of indicating to quark-committed colleagues that a critical process of examination of quark conjectures is in motion on a world wide basis, jointly with the study of fundamentally different approaches. The idea is to indicate that their not so unusual corridor-type of opposition to quark-non-oriented studies on hadrons, nowaday, has no scientific value. If they have technical arguments to disprove these criticisms, they must present them in scientific papers.

In conclusion, it appears that the scientific scene in hadron physics is changing rapidly and drastically. Today, we have valuable physicists who do not believe in quarks and, actually, do not believe in the physical laws used in these models. In particular, the search for coverings of the basic physical laws of the electromagnetic interactions, specifically conceived for the strong, is in motion.

On administrative grounds, a new voice is being heard in our community, which I have attempted to reproduce in the enclosed paper. Studies along quark conjectures should indeed be continued (and funded). Nevertheless the restriction of all the studies on the fundamental problem of hadron structure to quark conjectures only may well result to be an "historical error". Thus, a more balanced conduction (and funding) of research in the sector is

advocated whereby all promising lines of study, whether of quark or non-quark inspiration, are conducted and confronted with physical veritas.

On technical grounds, the ultimate objective of this scientific effort is to promote the experimental verification of the validity or invalidity of the basic physical laws used in current trends on strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle. As you know, these basic laws are experimentally established until now only for the electromagnetic interactions.

The idea is that effective research in the sector cannot be any longer continued on the basis of the mere beliefs of the validity of the basic laws, however autoritative is their source. More particularly, we are currently spending truly large amounts of money in strong interactions, all based on the mere belief of the validity of the basic laws. In my humble view, if this situation is protracted any longer, without the joint experimental study of the basic laws, a process to our scientific accountability will be simply unavoidable.

You will be pleased to know that this call for a return to do physics via experiments, rather than beliefs, is being answered. Indeed, a number of experimenters have already expressed to me their desire to initiate a predictably laborious study.

The reason for writing to you is to candidly express my most sincere regret that the Massachusetts Institute of Technology is, at this moment, only a spectator of the study of these fundamental physical problems.

I would like therefore to recommend most warmly that the Massachusetts Institute of Technology initiates an active involvement on these issues. In particular, your Institute has all the human and technical resources to conduct an experiment for the verification of the expected small deviations or possible validity of Pauli's principle in nuclear physics, via low energy processes, according to my proposal in the HJ 1, 574 (1978) (see also the enclosed paper for an outline). It is for me regrettable that these invaluable resources should not be used for such a fundamental physical problem.

In closing, I would like to indicate that this letter is solely motivated by my gratitude for the kind hospitality I have received at your Institute, as well as my esteem and respect for all of your.

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.
c.c.: Professors F.E. LOW, M. DEUTSCH, P. MORRISON, F. VILLARS and V. WEISSKOPF.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

May 21, 1979

Professor HERMAN FESHBACH
Department of Physics
MIT
Cambridge, Massachusetts 02138

Dear Herman,

In case you might be interested to know more details about the possibility of testing Pauli's principle in nuclear physics, I enclose a copy of the joint papers with C.N.KTORIDES (Greece) and H.C.MYUNG (Iowa, USA) entitled "Lie-admissible approach etc.". Section 3, and Table 1 (p.37)in particular, of this paper present a tentative, conjectural study of the problem.

Apparently, a number of additional contributions on this problem are forthcoming by independent researchers. In case you desire to be kept informed of the progresses, please let me know. Similarly, it would be a pleasure to make available a complimentary copy of the reprint volumes I and II of papers of 1978 on related topics (H.C.MYUNG, S. OKUBO and myself, editors).

Almost needless to say, all these (and the forthcoming) studies have only an initial, preliminary character, and much remains to be done to reach the necessary maturity for actual experiments. Yet, the sentiments by a number of colleagues, which I share, are that the sooner we start, the better.

Again, I always remember all of you with sincere pleasure and gratitude.

Sincerely

Ruggero Maria Santilli

RMS/ml

c.c: Professors F.E.LOW, M. DEUTSCH, P. MORRISON, F. VILLARS and V. WEISSKOPF

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Professor Francis E. Low
Massachusetts Institute of Technology
Department of Physics
Cambridge, Massachusetts 02139

(Mailed at the home address).

Dear Francis,

On June 1, 1980 the first phase of my grant with the DEPARTMENT OF ENERGY terminates. This phase essentially contemplated my exposure to advanced mathematics, which I did, thanks to the invaluable teaching of masters such as Shlomo Sternberg and Raoul Bott.

I am now entering into a new phase of studies which is specifically intended to achieve maturity of formulation of experiments. I am therefore seeking the association with a physics Institution.

The DOE is apparently pleased with the output of my studies and is interested in continuing my grant, provided that I identify a suitable Institution for its administration. Please feel free to contact, if you so desire, the DOE officer in charge of my grant, Dr. DAVIS C. PEASLEE, tel. 301 353 3624.

I have therefore applied to a number of selected Institutions in physics. In essence, I am seeking a physics Institution that allows me to apply for the renewal of my grant, as principal investigator, with the understanding that I will be considered for tenure after the needed number of years of service. This last point is particularly important for me. I am now 45 years old, and I have to give preference to an Institution where I have the possibility of being seriously considered for tenure at some time in the future.

As you know, I did not apply to MIT, nor this letter is an application. Nevertheless, I would appreciate the courtesy of your consideration of my case, and your advice whether it is appropriate for me to apply or not. You can rest assured of my confidentiality and gratitude.

Permit me the liberty of bringing you updated on my research. As you know, I am involved in the problem of the validity (according to some) or the invalidity (according to others) of conventional laws for the strong interactions.

After a predictably nebulous orientational phase, this problem is now studied by a coordinated group of mathematicians and physicists. Some of the mathematicians of our group are:

- Professor H.C.MYUNG of the University of Northern Iowa; Professor M. L. TOMBER ofthe Michigan State University; Professor R. OEHMKE of the University of Iowa; Professor G.P. WENE of the University of Texas at San Antonio, and others;

Some of the physicists of our group are:

- Professor S. OKUBO of the University of Rochester; Professor C.N.KTORIDES of the University of Athens in Greece (currently spending his sabbatical as my guest here at Harvard); Professors F. CANTRIJN and W. SARLET of the Instituut voor Theoretische Mechanics of the Rijksuniversiteit in Gent, Belgium (the latter spent his sabbatical 1978-1979 as my guest here); Professor J. FRONTEAU and M. TELLEZ-ARENAS of the Universite d'Orléans; Professor J. KOBUSSEN of the Institut für Theoretische Physik der Universität Zürich; Professors J. LÕHMUS and L. SORGSEPP of the USSR Academy of Science in Tartu; Professor JIANG CHUN-XUAN of the People's Republic of Chine; Professor ELIEZER and his group at La Trobe University in Australia, and others.

It is for me rewarding to see that this group is now expanding in a promising way.

The efforts are coordinated via:

- a yearly workshop, called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS. The first was held here at Harvard in August 1978; the second was held also here in August 1979; and the third is scheduled for August 1980. In addition a CONFERENCE IN LIE-ADMISSIBLE ALGEBRAS is currently under independent organization by the mathematicians of our group for spring 1981.
- the assistance to independent researchers for their contributions in the HADRONIC JOURNAL as well as in other Journals;
- my two series of monographs, "Foundations of Theoretical Mechanics" with Springer-Verlag (Volume I was printed in 1978 and volume II is in press); and "Lie-admissible Approach to the Hadronic Structure" with the Hadronic Press (Volume I was published in 1978, Volume II is in press and Volume III is scheduled for 1980).
- the reprint series "Applications of Lie-admissible Algebras in Physics" edited by Professors H.C.MYUNG, S. OKUBO and myself, which reprints all contributions in the field (Volumes I and II were printed in 1979, while we are working at two additional volumes, one on contributions prior to 1978 and one on the contributions of 1979).
- consultation and collaboration with colleagues involved in other lines of studies. For instance, I have recently completed the funding and editorial organization of a new series of reprint volumes "Developments in the quark theory of hadrons", edited by Professors D.B.LICHTENBERG and S.P.ROSEN (the first two volumes are under preparation, one on contributions prior to 1979; one on contributions in 1979; and then the series will continue with a yearly volume identifying the yearly

advancements in the field). In this way, we hope to have in the editorial programs of the HADRONIC JOURNAL representations of different trends in high energy physics that are effective for comparative analyses.

As you can see, the studies in the problem of the validity or invalidity of conventional laws for the strong interactions have proliferated considerably. Today, the achievement of a mature judgment calls for considerable reading. Lacking this technical study, any judgment is purely superficial. For instance, our notion of hadronic constituents (we call eletons and antieletons)-representative of a particle under joint electromagnetic and strong interactions (the latter realized via the condition of overlapping of the wave packets)-calls for the totality of the classical, quantum mechanical, algebraic, geometrical, field theoretical, statistical and thermodynamical contributions on Lie-admissibility which exists by independent mathematicians and physicists.

I am fully aware that you do not have the time of reading such a voluminous literature. To assist you for the possible achievement of a first understanding of what we are doing, I have enclosed copy of the "Chart 4.9" of my Volume II with Springer-Verlag.

This chart essentially outlines the problem in a way understandable for the intended audience: graduate students and researchers without an in depth knowledge of the symplectic quantization and of the broader Lie-admissible quantization. In particular, I would like to bring to your attention the recollection, in Part 9 of this chart, pages 343-349, of the historical, authoritative, voices of doubt by Fermi, Einstein, Jordan, and others (that we call "legacies").

Permit me to stress that this is a non-technical presentation. The technical one is elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, attended by all members of our group with the exception of a few who lacked travel funds and sent in their contributions. In case you are interested to have a complimentary copy of these Proceedings, please let me know and I shall do my best. They are scheduled for distribution in early 1980.

I hope you will see that there are indeed serious doubts on the validity of conventional laws for the strong interactions, backed by historical voices of the caliber of Fermi, Einstein, and Jordan. What we are doing is in essence consider their legacies seriously (rather than ignoring them, as done by the majority of our community); work out their implications as much as possible; and attempt the achievement of maturity of formulation of their experimental resolution.

I am fully aware of the inertia in our community toward these studies, which sometimes becomes straight opposition against their conduction (I have experienced a real hardship here at Harvard myself, that I shall tell you some day). Quite candidly, this is the reason why I did not

apply to MIT. I simply do not know whether MIT is interested in being associated with efforts toward the experimental resolution of the problem of the basic laws for the strong interactions, and leave theoretical beliefs (whether of quark or non-quark orientation) to the theoreticians.

An effective way to represent our studies is via Heisenberg's words ("Physics and Beyond", p. 70):

> "In science it is impossible to open up new territory unless one is prepared to leave the safe achorage of established doctrine and run the risk of a dazardous leap forward."

to which he adds soon after:

> "However, when it comes to entering new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do."

Despite these predictable human aspects, I believe that a fundamental ethical rule of our profession is the resolution via experiments of theoretical divergences or controversies. When it comes to the problem of the basic laws for the strong interactions, I believe that the implementation of this rule is necessary, of course, upon achieving maturity of formulation and technical capability. In my view, there is too much at state to lightly overlook the issue. When, in addition,there are historical, unanswered legacies by the founding fathers of contemporary physics, we simply have a duty to perform.

Irrespective of whether I apply or not to MIT and whether I join or not the MIT, you should be perhaps informed that we are close to the needed theoretical and technological maturity.

I am referring here to the experimental test of Pauli's exclusion principle under strong interactions, beginning at the nuclear level, according to my original proposal in the Hadronic J. 1, 574 (1978 , subsequently elaborated in a number of articles, and treated again at the recent workshop on Lie-admissibility.

The idea is to ascertain whether the principle is valid in nuclear physics in the same quantitative amount as it is valid in atomic physics, or very small deviations exist , are experimentally detectable, and have escaped currently available studies simply because not looked for.

On more specific grounds, the proposal suggests the test via low energy scatterings of hadrons in nuclei selected in such a way that their charge volume is below the value predicted by the proportionality rule with the total number of nucleons. The objective is that of verifying the statistical character of the wavefunction of the identical nucleons of these nuclei, that is, whether it is totally antisymmetric, or small deviations

from this statistical character are detectable. This experiment is apparently feasable with current technology, with the understanding that it is predictably delicate, and that it will predictably call for further, coordinated studies by experimentalists and theoreticians.

For the nuclei selected, we have an experimentally established, statistically small, state of penetration of the wave packets of the nucleons, one within the others. Fermi's legacy is that, under these conditions, we have forces more general than $f = -\partial V/\partial r$. It is this legacy which has been studied to all possible extents. At a first look, under these broader forces (variationally nonselfadjoint forces nonderivable from a potential), there is the inability of treating the structure via the conventional Schrödinger's and Heisenberg's equations in $H = H_{free} + H_{int}$. At a second look, Heisenberg's equations have been proved to be inconsistent for the broader forces considered, via the no-go theorem of the (pre)symplectic quantization (See part 6 of the enclosed chart for a nontechnical outline). At a deeper look, the forces considered imply the breaking, in a very small amount, of the SU(2)-spin symmetry, in much of the conceptual way according to which we have to break the rotational symmetry of the spinning top in Newtonian mechanics to avoid perpetual-motion-type of academic abstractions. The studies predict in this way that, under very small conditions of overlapping of the wave packets, identical particles that are exact fermions under long range elm interactions (this is the only statistical character experimentally established now), are no longer exact fermions. An expected very small deviation from the applicability of Pauli's principle is then consequential.

I am sure you will see the impact of Fermi's legacy. But there are other legacies. Pauli made it quite clear in his historical lectures and papers that his principle was conceived for the case of lack of overlap of the wave packets. Indeed, under these latter conditions, he had "stronger" forces which prohibited him to separate the wave function, let alone to establish its totally antisymmetric character. This legacy by Pauli has not yet been resolved experimentally and, in my view, it calls for an experimental verification. If nothing else, it calls for due consideration.

But, perhaps, most important is Einstein's legacy. You are aware that he refused to believe up to his death on the terminal character of the conventional uncertainty of quantum mechanics. In Heisenberg's words, he could at most tolerate quantum mechanics as a "temporary expedient". It has been proved in the Lie-admissible literature that, under the conditions of overlapping of the wave packets and nonselfadjoint forces, the concentional uncertainty of quantum mechanics must leave the way to broader views. One way you can see it is via the fact that the time evolution law under these broader forces is strictly noncanonical, at the classical level, and strictly nonunitary at the quantum mechanical level. Assuming that the conventional undeterminacy holds at a given value of time, it is necessarily nonpreserved in time under nonunitary time evolutions. I am confident you will see the impact of Einstein's legacy on this issue, as well as its deep inter-relation with the seemingly uncorrelated legacies by Fermi and Pauli.

All these legacies are brilliantly expressed in a unified algebraic way by Jordan's legacy. As you know, he did not believe in the associative character of the central algebraic structure of quantum mechanics, the universal enveloping associative algebra. He therefore suggested a broadering of this structure into a nonassociative form, which he selected, for statistical considerations, of commutative character (the celebrated Jordan algebras). Jordan's legacy is at the foundation of the Lie-admissible formulations. We simply perform the transition to a yet broader non-associative algebra, as necessary to represent forces more general than $f = -\partial V/\partial r$. The algebra is Lie-admissible to achieve a covering of conventional Lie stuff. But the algebra we use is also Jordan-admissible. This means that Jordan's view in preserved in its entirety in our approach .

Once Jordan's legacy is taken seriously and worked out, you can see the direct inapplicability of conventional views in quantum mechanics under the condition of overlapping of the wave packets and forces nonderivable from a potential. For instance, the SU(2)-spin algebra becomes mathematically undefinable and physically meaningless, trivially, because of the lack of its primary structure, its enveloping associative algebra.

The reason why an increasing number of researchers is attracted into the problem is that the Lie-admissible algebras do not leave all these issues quantitatively open. No. They provide coverings of the Lie algebras. As such, they allow the quantitative formulation of coverings of the conventional notions definable via the Lie lalgebra, such as that of fermions. This is the reason why there is a feverish activity going on for instance, on the SU(2)-admissible covering of SU(2) via algebraic studies (Myung, and others), quantum mechanical studies (myself and others), statistical studies (Fronteau and others), functional studies (Ktorides and others), deformation-type studies (Lõhmus and others), etc.

The reason why I give utmost priority to the experimental verification of Pauli's principle in nuclear physics are numerous. First of all, this is the experiment most close to maturity. Secondly, it is expectedly less expensive than an equivalent experiment in high energy physics, and will requ re comparatively less time. Thirdly, the mechanics of possible deviations goes at the heart of conventional relativities. After all, the SU(2) spin is a vital part of Galilei's and Einstein's special relativities.

Most important, in my view, is the fact that the possible experimental detection of very small deviations from Pauli's principle in nuclear physics will have far reaching theoretical implications. For instance, at the hadronic level it will imply the expectation that the deviations from conventional laws are greater, if you abandon point-like abstractions of the hadronic constituents and represent them via wave packets of extended size which (from atomic and nuclear similarities) have the same dimension as that of the entire hadron and, thus, are in a much greater state of mutual penetration (on a comparative basis with the nuclear case). At the astrophysical level the implications are even greater. We would have the expected inapplicability of the Riemannian geometry for the interior

problem (only) and, actually, its collapse in the central part: the assumption that all geometries for the interior problem are Lorentz in local character. Irrespective of that, the problem essentially consists of the issue whether an extended particle such as a proton, while within the core of a star, has exactly the same statistical character as that when moving in vacuum under elem interactions or not. Permit me candidly to say that the belief that the proton, under these conditions of extremely high pressures and densities, is a conventional fermion, is nowday shared by a fastly decreasing number of physicists. For me, the idea that a proton is still a fermion under the conditions considered is mere academic hand waving essentially motivated by the customary inertia on established doctrine, and which see its historical origin on the notion of massive point by Galilei and Newton, preserved in full in the special relativity, and implemented in the most advanced contemporary theoretical views such as QCD or supergravity. These views are fully acceptable for systems of particles moving in vacuum and no overlap of their wave functions. When particles are in a necessary state of penetration of their wave packets, these views still produce excellent physics (as proved by QCD). Yet, they are a crude approximation of a much more complex physical reality.

What I am trying to convey to you is that the estreme conditions of penetration of the wave packets in the interior of astrophysical bodies have their intermediate realization in the hadronic structure, and a primitive realization in the nuclear structure, according to a decreasing order of complexity of the forces, dynamical effects, and impact in the basic laws and principles.

This is the reason why I consider as of fundamental character the resolution of the issue via experiments in nuclear physics.

Dear Francis, I believe that your scientific function in these studies can be invaluable, particularly after your acquisition of the post of director of the division of nuclear physics at MIT.

I hope that this letter will assist you in your independent assessement of the experimental test considered, including its scientific implications, and will stimulate an active involvement by your division.

Love

Ruggero Maria Santilli

RMS/ml
encls.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE. MASSACHUSETTS 02139

October 23, 1979

Dr. R. Santilli
367 Linwood Avenue
Newtonville, MA 02160

Dear Ruggero:

Thank you for your letter of October 10, and for sending me the notice of your publication.

I will inquire here as to the possibility of a position. If there should be any possibility, I will of course be in touch with you.

Please give my best regards to your wife.

Yours sincerely,

Francis E. Low

FEL:jad

P.S. We had a very nice visit with [redacted] last summer.

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Professor HERMAN FESHBACK
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Ma 02139

Dear Herman,

My Volume II with Springer-Verlag of FOUNDATIONS OF THEORETICAL PHYSICS is now in press, and I though I should pay you the courtesy of an inspection of the Acknowledgments prior to their appearance in print.

Please inspect them (p. 18). You will see that I felt obliged to thank you again for the hospitality during 1976-77, as I did it for Volume I.

You might be interested to know that this effort is receiving a rewarding response. For instance, the enclosed book review on my Volume I by Professor LEIPHOLZ just came to me as a surprise: it calls my monograph "truly epoche-making". There is a feverish activity in the applications of these methods, particularly in engineering circles (but not at MIT, to my knowledge). As you know, they consist of rigorous analytic methods for the treatment of systems with forces more general than $f = -\partial V/\partial r$.

I enclose also copy of "Chart 4.9" of this Volume II. It essentially presents an outline of the doubts on the validity of conventional quantum mechanical laws and principles for the conditions of overlapping of the wave packets. The chart also presents a review of the historical, authoritative voices of doubt by Fermi, Einstein, Jordan, and others. (see part 9, pages 343-349).

If you do not have the time of looking at this material, you may pass it to a graduate student. Please keep in mind that this is a presentation for graduate students and for researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical treatment of the problem is presented elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, we held here from August 1 to 7, 1979, with the participation of mathematicians and physicists from the USA, France, Belgium, Svitzerland, and Israel, and with corresponding participants from the USSR and the People's Republic of China. These proceedings will be distributed in early 1980.

page 2.

I would like to bring to your attention a no-go theorem on conventional quantization of the (pre)symplectic geometry, outlined in Part 6. This theorem, rigorously proved by mathematicians in the field, establishes that the conventional Heisenberg equations are inconsistent for dissipative forces, that is, generalized Hamiltonian structures capable of recovering true, genuine, Newtonian, dissipative forces non-derivable from a potential under the correspondence limit.

It appears that the study of dissipative nuclear processes is increasing, but it does not appear that the nuclear physicists are aware of the existence of this no-go theorem of quantization, at least speaking at large. I have seen papers around that are flatly wrong. Perhaps, you should keep this theorem in mind in case you stumble into polynomial Hamiltonians of order higher than the second. Your in house experts on this theorem are GUILLEMIN and KOSTANT.

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Professor VICTOR F. WEISSKOPF
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Weisskopf,

I am taking the liberty of sending to you enclosed copy of the "Chart 4.9" of my volume II of FOUNDATIONS OF THEORETICAL MECHANICS with Springer-Verlag, now in press.

This chart presents an outline of the studies on the "legacies" by Einstein, Fermi, Jordan, and others (see part 9 for the recollection of these legacies, pages 343-349). Perhaps, you may prefer the conventional scientific language of this presentation (as compared to the informal language of my note to HANGAS HURST I recently mailed to you).

Again, if you have any critical remark or historical recollection that may assist me in the final editorial control of this rather delicate chart, I would be sincerely grateful.

Permit me to stress that the presentation of this chart is that for the intended level of audience of my monographs: graduate students and researchers without an in depth knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical presentation of these studies is elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS we held here from August 1 to 7, 1979 with the participation of mathematicians and physics from the USA, France, Belgium, Switzerland, and Israel, and with corresponding participants from the USSR Academy of Science. In case you are interested in a complimentary copy of these proceedings (scheduled for distribution in early 1980), please let me know and I shall do my best (the request is quite large already).

I would like also to take the liberty of stressing that the notion of hadronic constituent used in the paper by Dr. JIANG CHUN-XUAN on my structure model of hadrons (I recently mailed to you) is based on the totality of the studies for the Lie-admissible treatment of forces more general than the simplistic one of current use, $f = -\partial V / \partial r$, at all levels. I am referring here to the contributions by mathematicians and physicists in the analytic, algebraic, geometrical, field theoretical, statistical, thermodynamical, and quantum mechanical aspects. Lacking a technical knowledge of all these contributions, any judgment is purely superficial.

The enclosed chart 4.9 can give you only a rudimentary, non-technical, characterization of this hadronic constituent (we call eleton).

Permit me also the liberty of clarifying my scientific position with you. I am fully aware that you are deeply involved and committed to quark lines of study on hadrons. I would like to say that I sincerely respect these studies. The fact that these studies produce excellent physics is proved by a large volume of evidence. The fact that I support the continuation of these studies is proved by a new yearly series of reprint volumes, specifically and entirely devoted to quark lines, I have lately organized as part of the HADRONIC JOURNAL initiatives, under the independent editorial control by Professors D. B. LICHTENBERG and S. P. ROSEN. You will see this series advertised soon in PHYSICS TODAY.

More specifically, I believe that unitary models and QCD have a FINAL physical character for the classification of hadrons, or, you can say, for their "chemistry" or, you can also say, for their "Mendeleev-type" treatment.

My doubts rely only on their joint interpretation as providing an actual structure model of hadrons. To understand my doubts you must keep into account that I have tried for years to reach a structure model via quarks that is mathematically and physically consistent according to my own standards, and beginning, most importantly for the lightest known hadrons (Bohr did not start with the uranium, but instead, with the hydrogen atom).

The presentation of the technical difficulties that forced me to abandon quarks would be impossible here. They were simply too many. As a conceptual indication, when I was working at the π^0 via quarks, my first requirement was to achieve a $q\bar{q}$ bound state with an identically null probability of tunnel effects (and NOT with an approximately null value). For me, this was a fundamental condition for physical consistency to prevent the decay

$$\pi^0 \rightarrow q\bar{q}$$

which simply does not exist in nature. When this strict form of confinement is truly implemented, then you can see real problems. Once pushed to its extreme consequences, this confinement was simply incompatible with the basic laws of quantum mechanics.

The transition to field theory essentially obfuscates these technical difficulties. But, in my humble view, they persist . Simply peoples do not look deep enough. But there are exceptions. Numbu clearly stated in the Einstein's celebrations that confinement under gauge invariance is still an open question.

A point of sort of "irreconciliable disagreement" between quark-supporters and quark-dissidents (for the structure profile only) is the following. When faced with these technical difficulties of confinement, quark-supporters often assume the attitude that it is a minor problem, that it can be resolved with, say, infinite potentials, or that it will be resolved soon, or that it can be resolved via phenomenological (rather than dynamical) models"à la bag".

Concerned scolars take a more serious attitude in these matters. The idea is that a quark model without a STRICT form of confinement via DYNAMICAL means (equations of motion obeying physical laws) is FLATLY INCONSISTENT WITH PHYSICAL VERITAS. The quarks are simply not produced in the spontaneous decays nor in all high and low energy scatterings.

As a distinguished scolar put it to me in a recent letter:

> "The lack of achievement of a strict form of confinement is such a major inconsistency that should be reason for rejection of all papers on quarks, when referred to hadron structure. It is the same as stating, on grounds of scientific accountability, that a symplectic structure is not closed."

The origin of this intriguing situation, in my view, lies on the assumption by quark supporters of desiring to resolve the entire hadronic phenomenology with one single model. I am referring here to the tacit implementation in virtually all papers on quarks that the models provide a joint representation of the classification of hadrons as well as the structure of each individual element of a unitary multiplet. Such an approach can be proved to be inconsistent for the atoms. When passing to the much more complex hadronic world , scientific accountability demands caution, much caution.

This is the reason why:.

(I) I firmly believe in the final physical char acter of unitary (and QCD) models for the classification of hadrons (only);

(II) I favor the continuation of studies on the hadronic structure based on quark conjectures; but

(III) I oppose as vigorously as I can the restriction of the studies on hadron structure along quark lines only, and I favor instead the joint conduction of studies of fundamentally different orientation, under the condition that they achieve compatibility with the established, Mendeleev-type, unitary classification of hadrons.

This is my position both as an individual researcher as well as editor in chief of the HADRONIC JOURNAL.

page 4.

You are familiar with the studies along lines I and II. The studies along line III have proliferated substantially and are expanding rapidly. We already have two volumes of reprints APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS, Edited by Professors H.C.MYUNG, S. OKUBO and Myself. We are working at two additional volumes of reprints. Plus my monographs with Springer-Verlag and with the Hadronic Press. The reading of all this would take you too much time. This is the reason why I have enclosed the summary chart 4.9 of my volume II with Springer-Verlag.

The differences of line III with line II are primarily of research attitude. An epistemological outline of the former is the following.

(A) The teaching by the Founding Fathers of contemporary physics. When entering into an unknown and unresolved field, such as the structure of hadrons, our first attitude is that of studying the teaching by the founding fathers of contemporary physics.

You may see in the enclosed chart that Einstein's legacy on the expected lack of terminal character of the conventional uncertainty of quantum mechanics is still fully open. Bohr and Heisenberg's refuted Einstein's criticism at a time when the words "strong interactions" had yet to be invented.

Also, you may see that Fermi's legacy on the expected inapplicability of conventional geometries in the region of space occupied by a strongly interacting particle is more open than ever.

Similarly, you may see that Jordan's legacy on the expected need to enlarge the enveloping algebra of quantum mechanics is fundamentally open at this time.

The contemporary quark community has literally ignored these legacies. On the contrary, we have studied them seriously. We believe that until these legacies are resolved via experiments we can only conduct conjectural studies. To state it explicitly, we believe that, until the validity or invalidity of conventional quantum mechanical laws for the strong interactions has not been resolved via experiments, the studies on hadron structure will remain controversial, conjectural and of tentative character, whether of quark-orientation or not. Still in different terms, we believe that the problem of the structure of hadrons is purely secondary. The basic laws come first.

This is reason why I have proposed, via separate letters, to Philip MORRISON and other friends at MIT the initiation of experimental studies to test the validity or invalidity of Pauli's principle in nuclear physics (where at most, very small deviations are conceivable). This experiment is conceptually and technical constructed to test the historical, unaswered legacies I am recalling here.

You should not see here a potential invalidation of quark lines and of QCD. Not at all. Permit me to elaborate on this point.

The origin of our contemporary theoretical physics must again be seen in the originators of the basic ideas. Galilei's relativity is fundamentally dependent on the notion of massive point conceived by Galilei and Newton. This notion of point-like objects is preserved in its entirety in the special relativity, as clearly indicated by Einstein. In turn, this notion emerges in an often ignored, but fundamental physical role in the most advanced research, such as QCD. I am sure you will agree that, after all, QCD is based on local differential equations (point-like abstraction), and the conventional Lagrangian structure $L = L_{free} + L_{int}$. Apart extreme technical complications (e.g, for renormalization) the physical foundations conceived by Galilei and Newton are intact: QCD treats only point-like objects with only action at a distance forces. This is Newton's idea that the sun can be approximated to a point, only applied to hadrons.

We all know that such an approximation produces excellent physics. QCD has indeed produced, and will continue to produce excellent physics. The point is that, by no means, QCD should be considered as the final, terminal, physical description of hadrons. Indeed, the weakness of QCD rest in its physical foundations: the point-like approximation of hadrons, and of their constituents.

A possible departure from established physical laws, we are expecting when the particles are treated as they actually are (extended objects), would be no disaster at all. Suppose that the special relativity is experimentally proved as inapplicable for the dynamics of a hadronic constituent. This may stimulate a"scientific renaissance," but QCD will remain intact in its current physical value: a description of hadrons under their point-like approximation.

(B) The efforts in the construction of covering formulations. As editor of the HADRONIC JOURNAL I have sensed more particularly the following scientific situation. I have published a variety of papers in differentiated fields of science, such as functional analysis, geometry, nonassociative algebra, Newtonian mechanics, space mechanics, quantum mechanics, engineering,

The trends in all these studies is to stay away from the trivial $f = -\partial V/\partial r$. I am sure you realize that an engineer would be fired if he ignores internal losses when treating, say, an electric circuit. Similarly, I am sure you realize that a NASA officer would be fired on the spot if he intended to treat SKYLAB with $f = -\partial V/\partial r$. In biophysics the situation is even selfevident.

page 6.

In conclusion, the virtual entirety of contemporary science is moving toward the construction and application of methods for the effective treatment of forces generally nonderivable from a potential.

But there is one exception: quark lines and QCD. I am confident you will agree with me that the totality of papers along these lines is based entirely on $L = L_{free} + L_{int}$, that is, $f = -\partial V/\partial r$. What I wanted to bring to your attention here is that, in the view of an increasing number of observers, this situation will create a scientific gap between quark studies on hadronic structure and the rest of science, if excessively protracted. The rest of science is already working at methods substantially more advanced than those used in quark lines which, most importantly on physical grounds, are capable of accomodating genuinely more general forces. There is no doubt that $f = -\partial V/\partial r$ is effective for the atomic structure and for most of the nuclear structure. But there are doubts, serious doubts, that $f = -\partial V/\partial r$ will result to be truly effective for the hadronic structure.

The enclosed chart 4.9 may give you an idea of these broader methods.

(C) The efforts in the experimental resolution of the basic laws for the strong interactions.

The invalidity of conventional relativities in classical mechanics for forces not derivable from a potential (representatives of the motion of extended objects in a resistive medium), is an established physical reality. Example: SKYLAB. The forces of this system were polynomial expansions in the velicities for which conventional space-time symmetries are simply inapplicable, and must leave the way to broader geometrical views.

Fermi's legacy, as you can see in the enclosed chart 4.9, is that particles under conditions of overlapping of the wave packets possess forces nonderivable from a potential. If these forces break conventional relativity at the classical level, then, as a necessary condition for consistency with the corresponding principle, the breaking of conventional space-time symmetries must persist at the quantum mechanical level.

This is only one (out of several) reasons of doubts on the final character of conventional laws for the strong interactions. Indeed, the overlapping of the wave packets for these interactions is expected to be the rule.

This is also the reason why we are involved in a scientific effort to reach maturity of formulation of experiments for the resolution of these fundamental physical problems, as well as in the promotion of the initiation of these experimental studies.

page 7.

But, we have encountered a considerable inertia in the scientific community at large toward these studies. Billions of dollars of taxpayers money are preferred to be spent, instead, in a pletora of experiments, all scientifically valuable indeed, but of ,minute incremental character as compared to the resolution of the historical legacies indicated.

This situation can be best expressed via the words by WERNER HEISENBERG (Physics and Byond, p. 70):

> "In science it is impossible to open up new territory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hazardous leap forward."

To which he adds soon after:

> "However, when it comes to enter new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do."

Dear Professor Weisskopf, I sincerely consider you among the Founding Fathers of contemporary physics. Thus, I believe that you have the vision of men such as Heisenberg, Pauli, Einstein, Jordan, etc.

I am appealing to you for support in my proposal to Philip Morrison and other friends at MIT to initiate studies at MIT in the experimental verification of Pauli's principle in nuclear physics.

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

P.S. You might be interested to know that my recent preprint "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" (which was distribited rather widely at MIT) has been accepted for publication in FOUNDATIONS OF PHYSICS.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

Area Code 617
495-2170

Science Center
One Oxford Street
Cambridge, Massachusetts 02138

October 10, 1979

Professor PHILIP MORRISON,
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Philip,

I miss your stimulating presence at the lunch meetings at MIT during my visit in 1976-1978. Here, we simply do not have meetings of this type.

Permit me the liberty of outlining what we are doing. It appears to have a rather intriguing astrophysical implication. Any critical comment or historical recollection you might have, would be sincerely welcome. If you are interested, we sould perhaps see each other some time and enter into more detail.

Statement of the problem. We are involved in achieving maturity of formulation of experiments for the resolution of the problem of validity (according to some) or invalidity (according to others) for the strong interactions of the familiar relativities and quantum mechanical laws of the electromagnetic interactions, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and Heisenberg's indeterminacy principle.

Conduction of research. After an introductory, orientational phase, the problem is now studied, either directly or indirectly, by a coordinated group of mathematicians and physicists. Some of the mathematicians of our group are

- Professor MYUNG (university of Northern Iowa); Professor TOMBER (Michigan State University);,Professor OEHMKE (University of Iowa); Professor WENE (University of Texas at San Antonio); and others.

Some of the physicists of our group are

- Professor OKUBO (University of Rochester); Professor KOBUSSEN (Institut für Theoretische PhysiK der Universität Zürich); Professor KTORIDES (University of Athens in Greece, currently spending his sabbatical here as my guest); Professor SARLET (Instituut voor Theoretische Mechanics of the Rijksunieersiteit Gent, Belgium, who spent his 1978-1979 sabbatical as my guest); Professor LÕHMUS and his associates (of the USSR Academy of Science in Tartu); Professor ELIEZER and his group (at La Trobe University in Australia); Professor FRONTEAU and his group (at the Universite d'Orléans in France) and others.

This group is expanding in a quite promising way.

Coordination of research. The studies are coordinated via:
- a yearly workshop, called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (the first was held here at Harvard in August 1978; the second was held also here in August 1979; and the third is scheduled for August 1980);
- a CONFERENCE IN LIE-ADMISSIBLE ALGEBRAS under independent organization by the mathematicians;
- the coordination of efforts by independent researchers via the HADRONIC JOURNAL, of which, as you eventually know, I am the founder and editor in chief (the second editor in Howard Georgi, and the editorial council comprises distinguished scolars, including two Nobel laureates).
- the reprinting of all papers in the problem in the series APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS, edited by Professors MYUNG, OKUBO and myself (the first two volumes were printed in 1978, and two additional volumes are under way; and
- two series of research monographs I am currently involved in, one with Springer-Verlag entitled FOUNDATIONS OF THEORETICAL MECHANICS (the first volume was printed in 1978, and the second is in print); and the second series with the Hadronic Press under the title LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE (Volume I was printed in 1978; volume II is in print and volume III is scheduled for 1980).

The historical, authoritative, voices of doubt. These studies have a precise historical origin, in the sense that, first of all, they are not new, and, second, they see their origin in a number of "legacies" by the founding fathers of contemporary physics which have been simply ignored by the contemporary scientific community at large.

Einstein made it quite clear that he did not believe in the terminal character of the conventional uncertainty of quantum mechanics. He kept this conviction, up to his death, as you know well. In Heisenberg's words (From"Physics and beyond"), Einstein could at most tolerate quantum mechanics as a "temporary expedient". He made numerous counterexamples, that were however knocked down by Bohr and Heisenberg. The point is that Bohr and Heisenberg's criticisms strictly apply only for the atomic case. The case of the structure of astrophysical bodies, that of the structure of hadrons, and to a certain extent (see below), that of the nuclear structure, were out. As a result, EINSTEIN'S LEGACY IS STILL OPEN.

Pauli made it also quite clear that his principle was conceived for the lack of overlapping of the wave packets (atomic structure). Indeed, when the wave packets overlap, he had "stronger" forces which would prohibit him from separating the wave function, let alone to establish its totally antisymmetric character. The point is that, by no means, the lack of overlapping of the wave packets is a universal property. On the contrary, the overlap is rule, and the lack of overlap is the exception. Thus, we believe that PAULI'S LEGACY IS ALSO STILL OPEN.

Fermi also made it quite clear that he did not believe in the applicability of conventional geometries in the area of space occupied by a strongly interacting particle. Notice that Fermi was fully aware that the lack of applicability of conventional geometries implies that of conventional relativities and quantum mechanical laws. To my reconstruction, Fermi

thought that, under the conditions of overlapping of the wave packets we have forces more general than the trivial forces $f = -\partial V/\partial r$ which is dominating contemporary physics (to my dismay). Under these forces, he then saw the lack of the existence of a Hamiltonian $H = H_{free} + H_{int}$ and, thus, the lack of applicability of conventional Schrödinger's and Heisenberg's equations, and consequently, the need of broader formulations. We firmly believe that FERMI'S LEGACY IS ALSO FUNDAMENTALLY OPEN.

Jordan also made it quite clear that he did not believe (for statistical reasons) in the central mathematical structure of quantum mechanics, the universal enveloping associative algebra. Indeed, he suggested a generalization to a nonassociative form. He then selected a nonassociative commutative form (the celebrated Jordan algebras) for statistical reasons. The point is that the conventional associative envelop can be proved to be inconsistent when forces more general than $f = -\partial V/\partial r$ are used. Thus, we believe that JORDAN'S LEGACY IS ALSO FUNDAMENTALLY OPEN.

Von Neumann and Wigner joined Jordan in their celebrated joint paper of 1934 to clearly express doubts on conventional quantum mechanical views for the nuclear structure.

My list of authoritative, historical, voices of doubts could continue.

The role of the Hadronic Journal. When I decided to organize the Hadronic Journal in early 1978, the situation was essentially the following. We had all these authoritative, historical, doubts by the founding fathers of contemporary physics, but their followers had completely ignored them.

Actually, the very reason why I decided to organize a new Journal (and enter into all the predictable problems, such as financing-all successfully solved), was precisely this complete ignorance of the teaching by these masters. The organization of a new Journal was clearly essential. Indeed, you will agree with me that the conduction of studies of this type, against a dormient orthodoxy, would have taken decades in conventional Journals.....

In particular, at the time of the organization of our Journal we had
- no organized conduction of an in deth study of these legacies;
- no organized efforts at the formulation of experiments for their resolution; and
- no initiation of the study for possible generalized formulations.

I am happy to report to you that, beginning from its first issue of April 1978, and thanks to the participation of numerous scientists with a genuine vision and interest in the pursuit of knowledge, the HADRONIC JOURNAL has made significant contributions along all these three aspects.

State of the theoretical studies. Dear Philip, the amount of the litarature accumulated by now in this problem is so large, to discourage an outline, and to prohibit it in a letter.

What I can say is that all the seemingly unrelated, independent, historical, voices of doubt by Einstein, Fermi, Pauli, Jordan, Wigner, von Neumann, and others, turned out to be deeply interrelated, self-consistent, and mutually compatible.

Fermi's vision on the existence of forces non-derivable from a potential under the conditions of overlapping of the wave packets turned out to be the fundamental physical point. It sets in motion an array of methods for the treatment of these forces which are a covering of those of conventional use. The physical implications are selfevident. In particular, mathematicians in the (pre)symplectic quantization have lately proved a no-go theorem of conventional (Heisenberg's) quantization . Once this theorem is applied to actual physical situations without point like academic abstractions, the problem emerged as being precisely at the level of the geometry, the symplectic geometry in canonical realization. In substance, some thirty years later, FERMI'S VISION TURNED OUT TO BE CORRECT. Conventional ideas, such as Lie algebras, Heisenberg's equations, and the like, are simply out, because they can be proved to be inconsistent via the most effective and rigorous mathematical methods available at this time. This point was established at the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS.

Einstein's vision of a genuine, nonincremental, advancement turned out to be correct under Fermi's view. This has been indicated in the Literature of Lie-admissibility in a number of different ways leading to the same conclusion. One way you can see it is by first setting your mental attitude toward forces which are simply outside the arena of applicability of what you have read in the books. The time evolution law under forces nonderivable from a potential is noncanonical at the classical level, and nonunitary at the quantum mechanical level. Thus, assuming that, under the conditions of overlapping of the wave packets, Heisenberg's principle is valid at one given time, it is not valid at a later time. In conclusion, despite Bohr's and Heisenberg's counter-criticisms (all of atomic character), EINTEIN's VISION TURNED OUT TO BE CORRECT AND DEEPLY INTER-RELATED TO FERMI'S VISION.

Jordan's vision turned out to be crucial. A necessary condition to represent forces nonderivable from a potential is to alter the structure of the envelope. As a matter of fact, Jordan's vision is at the foundation of the notion of Lie-admissibility. This latter notion simply realizes nonassociative enveloping algebras. These algebras are selected to be Lie-admissible to provide a genuine algebraic covering of conventional Lie stuff, which is capable of representing (via the generalized time evolution law) forces more general than the simplistic $f = -\partial V/\partial r$. The point is that this algebra turns out to be also Jordan-admissible. Thus, Jordan's approach is fully contained in the Lie-admissible approach. In conclusion, JORDAN'S VISION TURNED OUT TO BE CORRECT AND DEEPLY INTERRELATED TO FERMI'S AND EINSTEIN'S VISION. I am confident you realize that, when the associative envelope of quantum mechanics is

generalized into a nonassociative form, the conventional uncertainty of quantum mechanics does not makes sense, mathematically and physically, and must leave the way to broader views, of course, all under the idea that we have generalized forces for the conditions of overlapping of the wave packets. When particles leave this condition, and move in vacuum under only action at a distance forces (derivable from a potential), conventional stuff is recovered identically, apart secondary effects, because the Lie-admissible algebras reduce themselves into the conventional algebras at the limit of null forces nonderivable from a potential, and therefore, the conventional laws are recovered identically.

Pauli's vision also turned out to be correct. In essence, under forces nonderivable from a potential we have a breaking of the SU(2)-spin symmetry, that is, we lack the technical ingredient to properly characterize the notion of fermion, which is obviously a prerequisite for Pauli's principle. This has been independently established in the literature of Lie-admissibility. To see it,again, you must set your mental attitude outside what you read in contemporary books of physics, because you are treating forces and physical conditions outside their arena of applicability. Actually, the occurrence can be seen at the Newtonian level. Consider the spinning top. Conventional books treat this system, from a group theoretical viewpoint, via the SO(3) symmetry. I leave this treatment to their authors. As a physicist I intend first to look at the physical reality, and only after identify the methods for the treatment. The exact SO(3) symmetry for the spinning top literally implies the perpetual motion. Physical reality is different than these academic abstractions. The angular momentum of the spinning top decays in time. This means that the SO(3) symmetry is meaningless for the treatment of the system. Indeed, it must be necessarily broken to comply with physical evidence. apart technical aspects (Lie-admissible quantization), the situation for Pauli's principle under overlapping of the wave packets is conceptually the same as that of the spinning top.In both cases, point-like abstractions are academic hand-waving. In actuality,we have extended bodies and our theoretical tools must represent this extended character. In both cases we have, furthermore, extended bodies moving in a resistive medium.The spinning top rotates in a viscous macroscopic medium. A wave packet, when in a state of penetration with other wave packets, is conceptually along the same lines. In both cases the rotational symmetry is broken. In both cases, the physical quantities characterized via the group of rotations are inapplicable.

Equivalently, you simply do the homework of constructing a wave equation capable of actually representing forces nonderivable from a potential (this can be done starting from generalized classical Hamiltonians, and then quantizing via Hamilton-Jacobi equations - nothing more). You then recover Pauli's troubles which forced him to exclude the condition of overlapping of the wave packets. Indeed, the generalized Schrödinger's equation cannot, in general,be separated. Its totally antisymmetric character is then only in the imagination of physicists desiring to preserve the status quo.

Please excuse the passionate language in this passage. I have experience. in my past academic life real hardship because of the refusal by the ortho-

xy to even consider this legacy by Pauli, let alone to treat it decently.

But things are changing fast. I am happy to inform you that, nowday, the number of physicists who believe that, say, a proton in the core of a star is a fermion, is decreasing quite rapidly. The proton is an extended object. When under the extremely high pressures and densities of the core of a star, its wave packet is forced to penetrate within those of the surrounding particles. Under these conditions, the idea that it "spins" in exactly the same amount as that when spinning in vacuum under long range elm interactions (the hydrogen atom) is clearly questionable. For me, quite candidly, is it is an extremely crude approximation of a complexity beyond our imagination.

In conclusion, the literature of Lie-admissibility has indicated that, when a system of identical fermions penetrates hadronic matter, the particles are no longer exact fermions under forces nonderivable from a potential. In this sense, Pauli's principle is inapplicable. Thus, PAULI'S LEGACY TURNED OUT TO BE DEEPLY RELATED AND COMPATIBLE WITH THE LEGACIES BY FERMI, EINSTEIN, JORDAN, AND OTHERS.

A rather feverish research activity is now going on for the construction of coverings of conventional insights. You see, the Lie-admissible algebras are a bona fide algebraic covering of the Lie algebras with a fundamental physical origin: the time evolution law under forces nonderivable from a potential. "Universality theorems" for their existence under these conditions have been proved both classically and quantum mechanically. They verify the correspondence principle, in the sense that classical and quantum mechanical equations can be uniquely related. As a result, this settings allows the quantitative formulation of the covering notion under forces nonderivable from a potential (e.g., spin). This is the reason why an increasing number of mathematicians and physicists is joining our group. They see a clear possibility not only of doing physics, but new physics.

I have personally proposed a classical and quantum mechanical covering of Galilei's relativity for the conditions considered, which is under developement now by a number of independent researchers. The studies for the covering notion of spin, which is the most important part of my Lie-admissible relativity, is now studied:classically by Eliezer and his group in Australia; quantum mechanically by myself and others; field theoretically by Kobussen in Switzerland; statistically by Fronteau in France; algebraically by Myung in the USA; etc.

At this moment I am involved in extending this relativity to "relativistic" conditions of particles which are strictly outside the physical arena of Einstein's conception: motion of extended particles within a hadronic medium.

The expected astrophysical implications. We now come to the aspect for which I decided to write you this letter.

Permit me to clarify my scientific position. I am a firm believer of Einstein's special relativity (general relativity) for the motion of charged particles in vacuum under external elm interactions (the exterior problem of gravitation). Actually I am fascinated by the volume of experimental evidence for these settings and their credibility.

Nevertheless, I believe that the special relativity (the general relativity) are only a first, crude, approximation when referred to extended particles moving within hadronic matter (to the interior problem of gravitation).

The reason is that I have difficulties in establishing the conceptual foundations of these relativities under the latter conditions. For instance, when referring to the interior of a hadron, I have difficulty in establishing the conventional propagation of light, let alone the fact that its value is c and costant. This, of course, if you abandon point-like abstractions of the constituents and represent them as ordinary quantum mechanics tells us: with extended wave packets. Similarly, I believe in the elevator experiment. But this is strictly an exterior case. How do you do the elevator experiment in the interior case? do you do a cicylindrical hole in a star? Assuming that you do so, then the problem becomes automatically an exterior one. How you treat moving observers when referring to the interior of an astrophysical object? Can we let them move within a star? and if we keep them outside a star, how do we "measure" something occurring in its interior?

Irrespective of personal theoretical views, a rather visible "collapse" of the current models of gravitation for the interior case has been recently identified by NASA: One of the simplest possible interior systems is the motion of a satellite in earth atmosphere. In particular, SKYLAB, while it was falling on earth, had forces highly nonderivable from a potential (polynomial expansions in the velocities). At the Newtonian level, this system is simply not derivable from a Lagrangian action principle in the coordinates of its experimental detection (see my monographs with Springer-Verlag for equivalent formulations in new coordinates). This feature persists in its entirety after gravitational extension. The conventional theoretical views, whether Riemannian, of supergravity or of gauge type, simply failed to properly characterize these elemental physical aspects of SKYLAB. They yielded only a point-like approximation.

The situation was put to me in rather vivid terms during the SKYLAB episode. I am sure you realize that NASA people were under pressure those days. They were unable to predict the location of impact up to the very last moment. I understand that the entirety of our theoretical (and computer) knowledge was feverishly inspected in the hope to gain some clue. I was contacted by NASA because they learned of my monographs with Springer-Verlag (entirely devoted to forces more general than $f = -(\partial V/\partial r)$ as well as the concentration of papers in the HADRONIC JOURNAL for the treatment of these forces. Predictably, I was unable to make any contri-

bution, after all, with only a few days notice. Nevertheless, one thing was identified. Galilei's relativity, Einstein's special relativity and Einstein's general relativity all see their origin and arena of applicability in the notion by Galilei and Newton of a massive point. SKYLAB was a fundamentally different physical system. As a NASA men vividly put it to me on the phone: "if a theoretician comes here suggesting the treatment of SKYLAB with conventional relativities, he would be likely chased out of NASA premises."

Dear Philip, I believe that a scientifically effective attitude is that of simply face the fact that the interior problem of a hadron or a star will likely call for forces outside the capability of current geometries; and their study, in any case, is recommendable. The second step is then that of looking at the authoritative voices of doubt by the founders of contemporary geometry.

As an example, Cartan made it quite clear that the Riemannian geometry is unable to recover Newtonian mechanics at large, but only that part with geometrizable forces.

Lagrange and Hamilton conceived their equations with external terms. Their analytic vision was therefore substantially broader than that of contemporary astrophysicists working on the interior problem. Indeed, irrespective of realizations in curved manifolds, these physicists simply use Lagrange's equations in their truncated form, that without the external terms Lagrange and Hamilton were so keen on (for good reason too - to avoid perpetual-type approximations). Under these conditions, the very terms "Lagrange's equations" are historically incorrect and misleading. Once, again, the teaching by the founders is taken seriously, the next step is the search of a geometry capable of accomodating broader forces, as well as the recognition that the capability by the Riemannian geometry of representing the forces of nature is truly limited.

You might be interested to know that a feverish activity is going on also along these lines by mathematicians and physicists. We are using, as a first step, a geometry under study called "symplectic-admissible" (you might call it also "Riemannian-admissible"). This geometry has been proved at the recent SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS as being able to represent directly, in local charts (that is, without changing the coordinates of the experimental setting), the most general forces we now know : the variationally nonselfadjoint, integrodifferential forces (superpositions of local and nonlocal forces derivable and nonderivable from a potential). By comparison, the representational capability of the forces that are expected in the interior problem, for the case of the Riemannian geometry, are truly small.

The experimental profile. The studies indicated in this letter can be best expressed via the words by WERNER HEISENBERG (Physics and Beyond, p. 70).

> "In science, it is impossible to open up new territory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hazardous leap forward."

To which, he adds immediately after:

> "However, when it comes to entering new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do."

In other words, there is an inertia in the orthodoxy of physics against the experimental resolution of theoretical divergences, when they imply the possibility of departures from established doctrines. More specifically, we spend these days truly large amounts of taxpayers money in experiments which are certainly valuable, yet, in my view, of minute incremental character when compared to the experimental resolution of the legacies indicates here.

But there are exceptions to these general rules. These are the men that truly advance physics, that is, the foundations of physics.

I believe that you are one of these (few) men. After all, this is the reason why I am making this report to you.

After considering a number of alternatives (the possibilities are, in principle, many), we decide to concentrate our efforts in the achievement of maturity of formulation for

THE EXPERIMENTAL TEST OF PAULI'S PRINCIPLE IN NUCLEAR PHYSICS

according to my original proposal in the Hadronic J. 1, 574 (1978), subsequently elaborated in a number of articles and treated again at our recent workshop.

The idea is to ascertain whether Pauli's principle is valid in nuclear physics in the same quantitative amount as that of atomic physics, or very small departures exist, are experimentally detectable, and have escaped available inspections simply because not looked for, along much of the quantitatively similar, historical discovery of parity violation in weak interactions.

On more specific grounds, the proposal suggests the test via the use of low energy scatterings of hadrons in nuclei selected in such a way that their charge volume is below that predicted by the proportionality rule with the total number of nucleons. In these nuclei, the nucleons are in an experimentally established, statistically small state of overlapping of the wave packets. This activates the expected presence in the nuclear force of a small term nonderivable from a potential. In turn, this implies the expected breaking of the SU(2)-spin symmetry in a small form. Still in turn, this view implies that nucleons, under these conditions, are not exact fermions. Still in turn, this imply the expected, conceivable,

statistically small departure from the applicability of Pauli's principle. This latter aspect can be established via the experimental verification whether the wavefunction of identical nucleons of the nuclei selected is totally antisymmetric under particle permutations, or small deviations from this statistical character exist.

In the view of independent physicists, the test is feasible with current technology, under the expectation that it is indeed delicate, and that it will call for an additional, coordinated, joint effort by theoreticians and experimentalists.

I am confident you will see that this proposed experiment is conceived to test, directly, FERMI'S LEGACY. The test, can be also used for an indirect test of EINSTEIN'S LEGACY (because the mechnism of possible departure implies a corresponding departure from conventional uncertainty via nonunitary time evolutions), as well as JORDAN'S LEGACY (because the possible departures from an exact fermionic character call for a departure at the level of the enveloping associative algebra). But, perhaps most intriguing, this test can be seen as a first experiment to resolve the current belief in the interior problem of gravitation, according to which all admissible geometries are Lorentz in local character. Indeed, the possible breaking of the SU(2)-spin symmetry under variational nonselfadjoint forces directly tests this point. In the transition from the nuclear to the astrophysical conditions we only have a quantitative difference. But the theoretical foundations are the same.

Many of us see the Massachusetts Institute of Technology as one of the leading Istitutions in this Country in nuclear physics (and for very good reasons).

I would like to close this letter by appealing to you for the initiation at MIT of the experimental resolution of these historical legacies, beginning in nuclear physics.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ml
encls.

Dear Philip,

I enclose copy of "Chart 4.9" of my volume II with Springer-Verlag. This chart presents an outline of the ideas discussed in the letter.

Please keep in mind that this outline is intended for graduate students and researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

Thus, this outline is non-technical . The technical treatment is presented elsewhere and, in particular, in the proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS that will be available in early 1980.

Sincerely

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

October 10, 1979

Dr. ROBERT BIRGENEAU
Department of Physics
M.I.T.
CAMBRIDGE, Massachusetts 02139

Dear Robert,

As you can see, I am still in the USA (rather than back to Europe). As a result, we purchased a new house. I am confident you are enjoing yours.

I do not know whether what we are doing here may have some relevance or connection with what you are involved with. In any case, I am taking the liberty of sending you some informative material.

You will find enclosed copy of "Chart 4.9" of my Volume II of FOUNDATIONS OF THEORETICAL MECHANICS with Springer-Verlag, now in press. This chart essentialy outlines the problem we are working on, that is, a technical study of the reasons of doubts on the validity of conventional quantum mechanical laws for particles under conditions of overlapping of the wave packets. The chart also recalls the historical, authoritative, voices of doubt by Fermi, Einstein, Jordan, and others (you may see part 9, pages 343-349).

Please take into account that the presentation of this chart is that for the intended audience of my monographs: graduate students and researchers without an in depth knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical content is elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS we held here at Harvard from August 1 to 7, 1979, with the participation of mathematicians and physicists from the USA, France, Belgium, Switzerland, and Israel, and with corresponding participants from the USSR and the People's Republic of China. These proceedings are scheduled for distribution in Early 1980. In case you are interested, please let me know, and I shall attempt to let you have a complimentary copy.

You might also be interested to know that our group is involved in a promotional effort to resolve these historical "legacies" via experiments. In particular, we are establishing a contact with the community of

experimenters in nuclear physics in Europe and in the USSR (here in the States nuclear physicists seems interested in doing only conventional stuff).

One experiment we have formulated is to test the validity of Pauli's eclusion principle in nuclear physics. The idea is that of ascertaining whether this principle is valid in nuclear physics in the same quantitative amount as it is valid in atomic physics, or very small deviations exist, can be detected, and have escaped inspection until now simply because not looked for.

On more specific grounds, the proposal is via low energy scatterings of hadrons in nuclei selected in such a way that their volume is below the value predicted by the proportionality rule with the total number of nucleons. For these nuclei, we have an experimentally established, statistically small, condition of penetration of the wave packets of nucleons one within the others. Under these conditions, we expect the presence in the nuclear force of an additional term, this time nonderivable from a potential and with a small coefficient (Fermi's legacy). We have studied these forces to considerable extent. They essentially prohibit the consistent quantization via Heisenberg's equations because of a no-go theorem of the (pre)symplectic quantization (Part 6 of the enclosed chart). At a deeper analysis, these broader forces imply the breaking of the SU(2) spin symmetry, that is, identical nucleons, under the condition of overlapping of the wave packets, are not exact Fermions. The inapplicability of Pauli's principle is then consequential. Under the classical limit we have a conceptually similar situation: spinning tops with drag torques in a resistive medium, for which the SU(2) rotational symmetry simply does not make sense (it would imply the perpetual motion).

This proposal is in my article Hadronic J. 1, 574 (1978), and has been subsequently elaborated in a number of articles, as well as at the recent workshop.

Well, I do not want to take too much of your time. In case you are interested in these things, perhaps we should see each other, or have lunch sometime together. It would be a pleasure for me to see you again.

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

November 14, 1979

Dr. R. M. Santilli
Science Center, Room 331
1 Oxford Street
Cambridge, MA 02138

Dear Ruggero,

Thank you for your note of Oct. 10 and the preprints. Your ideas and proposals all sound very interesting. Unfortunately my own interests in physics are probably at the opposite limits from yours - collective behavior and phase transitions in condensed matter with special attention paid to the role of symmetry and the dimensionality of space. Our recent work has been focussed on systems at their lower marginal dimensionality, that is, those for which algebraic decay of correlations is allowed but no true long-range order.

We are indeed enjoying 74 Baker's Hill Road. My wife is especially happy in the neighborhood. The people are both varied and surprisingly interesting. If you have ever driven by then you will undoubtedly have noticed that I have become a tree and shrub fanatic. So far I have planted over 100 (maples, rhododendrons etc.)! My only real complaint is with the heating bill.

I would, of course, enjoy seeing you again some time. As you suggest, perhaps some time when I am up at Harvard we can have lunch together. Please give our regards to your wife.

Yours sincerely,

Robert J. Birgeneau

RJB:mb

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

November 12, 1979

Professor F. E. LOW,
Director
Laboratory for Nuclear Physics
M.I.T.
Cambridge, Ma 02139

and

Professor A. KERMAN
Director
Center for Theoretical Physics
M.I.T.
Cambridge, Ma 02139

Dear Francis and Arthur,

You might be interested to know the expected energy-related implications of my recent proposal to test Pauli's principle in nuclear physics, and I enclose some informative material to this effect.

I remember you always with sincere pleasure.

Yours, Very Truly

Ruggero Maria Santilli

RMS/ml
encls.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

Area Code 617
495-2170

Science Center
One Oxford Street
Cambridge, Massachusetts 02138

January 9, 1980

Professor Gian-Carlo Rota
Department of Mathematics
M.I.T.
Cambridge, Ma 02139

Dear Gian Carlo,

Subject to administrative finalisation, my research grant from the Department of Energy has been renewed for 1980|1981 with full coverage of my salary and research expenses. I would be interested in visiting your Department at M.I.T. for part of the next academic year, if at all possible, as your personal guest or in some other form you consider appropriate.

The Proceedings of our second workshop on Lie-admissible algebras and their applications to strong interactions are now well under way, and I should have a complimentary copy for you within a few weeks. A preview of the contents is enclosed.

I believe that my visit at your Department might be mutually beneficial, in the sense that I might be exposed to your current research, while some of you might be intrigued by the mathematical and physical potential of the Lie-admissible generalization of Lie's theory.

Looking forward to hearing from you, I remain

Yours Sincerely

Ruggero Maria Santilli

RMS|ml
encls.

P.S. I am continuing to print the ad on your encyclopedia in each and every issue of the Hadronic Journal, as agreed. Also, I would like to encourage you again to let me publish one of your review or research papers in our Journal. You might also be interested to know that Alex Doohovskoy was one of my readers of the state of the art memoir on Lie-admissibility I have published in Part A of the Proceedings. (I understand he was one of your students).

In case I can visit you, I would appreciate the courtesy of a letter indicating this guest status, its duration, as well as the capability of using the MIT Library, without any financial support whatever from MIT. With your permission, I would like to include this letter in the application for renewal of my grant. The grant has been already approved. Please feel free to contact the DOE officer in charge of my grant, Dr. D.C.Peaslee , tel. 301 353 3624.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE, MASS. 02139

DEPARTMENT OF MATHEMATICS

January 18, 1980

Professor Ruggero Maria Santilli
Department of Mathematics
Harvard University
One Oxford St.
Cambridge, Mass. 02138

Dear Professor Santilli:

I was very pleased to receive your letter and curriculum vitae. I would be delighted to have you join us at M.I.T. in the capacity of a Visiting Scholar (unsalaried). I have sent a copy of your letter to Professor Louis Howard who will act upon it and correspond directly with you.

I look forward to seeing you at M.I.T. some time during the 1980/81 academic year. Please keep me advised of your plans.

Sincerely,

Gian-Carlo Rota

GCR:lb

Gian-Carlo Rota

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

26.

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 28, 1980

Professor GIAN CARLO ROTA
Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professor Rota,

Please accept the sentiments of my appreciation and gratitude for your kind letter of January 18, 1980 and for the possibility of being a guest (or visiting scolar) at your department during 1980/1981.

Again, I would appreciate the possibility of using conventional research facilities (e.g., library, parking, and address) so that I can actuate my research according to my grant with the Department of Energy. A desk would be welcome, but it is not essential. No expense of any nature by your Department is expected.

I contemplate to pay you and Professor LOUIS HOWARD a brief visit this spring (I am now leaving for Europe to deliver a few seminars on Lie-admissible algebras). In the meantime, I shall keep you informed of our progresses in this line of study.

Sincerely

Ruggero Maria Santilli

RMS/ml

c.c.: Prof. L. Howard

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

March 20, 1980

Dr. M. A. HORNE
Department of Physics
Division of Nuclear Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Mike,

We would like to express our appreciation for your kind reception during our visit yesterday at your laboratory.

We appreciated in particular your comments concerning our proposal to achieve a quantitative experimental knowledge on the validity of Pauli's exclusion principle under strong interactions. A list of references on the proposal is enclosed for your convenience.

As you know, we are theoretical physicists with limited capabilities (if any) to identify specific and concrete experimental settings. Therefore, we would like to rely on the judgment by you, Mr. Atwood and Mr. Arthur whether or not your equipment can do the proposed experiments.

We remain at your disposal for any further consultation and exchange of ideas.

Best Personal Regards

Ruggero M. Santilli

[redacted]

RMS-CNK/ml
encls-

c.c.: Prof. [redacted] University
Mr. D.K.ATWOOD and J. ARTHUR, M.I.T.

REFERENCES ON THE PROPOSAL TO TEST PAULI'S EXCLUSION PRINCIPLE UNDER STRONG INTERACTIONS AS PER MARCH 20, 1980.

The proposal was originally formulated in the paper

1. R.M.SANTILLI, Need of subjecting to an experimental verification the validity within a hadron of Einstein special relativity and Pauli's exclusion principle, Hadronic J. 1, 574-901 (1978)

with particular reference to pages 786-797, 807-818, and 880-882.

In this paper the following results were apparently achieved for the first time:
(a) the SU(2)-spin symmetry is broken under nonlocal interactions, including their local nonpotential approximation, as expected for strongly interacting particles under conditions of mutual penetration of their charge volumes and (to a smaller extent, their wave packets).
(b) the replacement of Lie's formulations with the covering Lie-admissible formulations, and, in particular, the replacement of the conventional associative envelope of quantum mechanics with a covering nonassociative envelope, implies a generalization of Heisenberg's equations of Lie-admissible algebraic character which is capable of representing forces structurally more general than $f = -\partial V/\partial r$.
(c) the generalized dynamics as per approach (b) implies a covering notion of particle under conditions of mutual penetration with other particles whose spin is given by the generalized form for suitable operators C

$$J_i J_i = \hbar^2 j(j+1) \longrightarrow J_i C J_i = s(\hbar, j, r, \ldots)$$

This conceivable deviation from the value of conventional spin then implies a conceivable inapplicability of Pauli's principle .

It was stressed in ref. 1 that the quantitative study of a conceivable deviation was conducted to the effect of stimulating the currently lacking experimental verification of Pauli's principle, and that possible deviations from conventional values of spin can at most be internal effects in nuclear, hadron,and astrophysical structures.

The subsequent paper

2. C.N.KTORIDES, On a possible incompatibility of Lie-admissible local powers of a quantum field with the Wightman axioms and the spin-statistics theorem Hadronic J. 1, 1012-1020 (1978)

the implications of the discrete analysis of ref. 1 were extended to field theory. Ref. 2 essentially established that, under interactions structurally more general than the electromagnetic ones, realized via the generalization of the envelope to a nonassociative form, the spin-statistics theorem is not expected to be necessarily valid. This result provided the quantum field theoretical counterpart of the conceivable deviations from Pauli's principle in discrete mechanics. Again, the analysis was motivated by the intent to stimulate the achievement of a quantitative experimental knowledge on the basic laws of the strong interactions.

The subsequent paper

3. C.N.KTORIDES, H.C.MYUNG, and R.M. SANTILLI, Elaboration of the recently proposed test of Pauli's principle under strong interactions, Physical Review D, in press,

initiated the rigorous study of the quantitative theoretical profile. Thanks to the collaboration by Professor Myung (a leading mathematician in Lie-admissible algebras), the authors entered into a technical analysis of the replacement of the associative with a nonassociative envelope for the computation of possible spin mutations. Ref. 3 also presented the specific proposal to test Pauli's exlusion principle for nuclei whose volume is below the value predicted by the proportionality rule with the total number of nucleons. In fact, the conditions of mutual penetration of the charge volumes is an experimentally established fact for these nuclei, although of predictable small character.

The subsequent paper

4. H.C.MYUNG and R.M.SANTILLI, Further studies on the recently proposed experimental test of Pauli's exclusion principle for the strong interactions, Hadronic J. 3, 196-255 (1979)

continued the quantitative theoretical study of the proposal of ref. 1. In particular, the paper identified a hierarchy of generalizations of the SU(2)-Lie algebras to a Lie-admissible form. This hierarchy was applied to the study of a possible hierarchy of mutations of conventional spin (or SU(2)-spin breakings), depending on the conditions of mutual penetration of particles , such as in the transition from the nuclear structure to the structure of hadrons, and to the structure of astrophysical bodies undergoing gravitational collapse.

The more recent memoir

5. R.M.SANTILLI, Status of the mathematical and physical studies on the Lie-admissible formulations on July 1979 with particular reference to the strong interactions, Hadronic J. 2, 1460-2018 (1979)

presented an analysis of the implications of the experiment proposed in ref. 1 with particular reference to the historical profile (a possible deviation from Pauli's principle under strong interactions would apparently be in agreement with a number of authoritative voices of doubt), and the open problem of the structure of hadrons (where a possible deviation from Pauli's principle would apparently allow the identification of the hadronic constituents with physical particles).

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 16, 1980

Professor LOUIS HOWARD
Department of Mathematics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Howard,

I would like to thank you for your recent phone confirmation of my guest status at your Department from June 1, 1980 to June 1, 1981, following the kind invitation by GIAN CARLO ROTA (a copy of his letter of January 18, 1980 is enclosed).

Following this confirmation, I have instructed the staff at the HADRONIC JOURNAL to change my address in the second page of the Journal (copy of the current version is enclosed) to the new form

Ruggero Maria Santilli
Massachusetts Institute of Technology
Room 2-236
Cambridge, Massachusetts 02139

which will be used beginning from the June issue, 1980, currently in print. I have also communicated the change of address to other places (societies memberships, etc.).

As verbally indicated to you, I would prefer to avoid the indication "Department of Mathematics" in my editorial activity, as I have done with Harvard. The indication of the secretarial room number 2-236 is fully sufficient for mail purposes. As also indicated to you, the availability of an office on campus would be appreciated, but it is not essential. In fact, I can use my home office. In essence, I am interested in the capability for my conducting at MIT the current studies on the direct experimental verification of conventional laws for the strong interactions. For this purpose, I only need the possibility of using the parking-library-address facilities. Your assistance in obtaining the parking and library permits would be appreciated.

You might be interested to know that I was a guest of FRANCIS E. LOW at MIT from January 1, 1976 until August 31, 1977. I have always remembered this hospitality with pleasure and gratitude. I will be delighted to be at MIT again.

My research contract under DOE support expires here on May 31, 1980. I am therefore in the process of dismantling my office. Until I see you at MIT in June, you can reach me at home (temporary address: 367 Linwood Avenue, Newtonville, Ma 02160).

Very Truly Yours

Ruggero Maria Santilli

RMS/ml; c.c. Professors G.C.ROTA and F.E.LOW, M.I.T.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

CAMBRIDGE, MASS. 02139

DEPARTMENT OF MATHEMATICS
2-377

May 30, 1980

Dr. Ruggero M. Santilli
Science Center
Harvard University
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli:

When you called me on the telephone, you stated that you hoped to be here as a guest at the Mathematics Department at M.I.T. "with Gian-Carlo". I interpreted this to mean that you would be working with Professor Rota, and as I knew that he has several collaborators whom he hopes to have as guests next year, I assumed that you were one of these. Perhaps I was, in consequence, rather over encouraging about the prospects, though I recalled mentioning specifically that we are currently very tightly stretched for space and that I could not say anything definite until we have had an opportunity to look over the whole picture of visitors. The end of the term has brought this opportunity, and indeed it appears now that the space situation is even worse than anticipated, so that it seems that only in very exceptional circumstances shall we be able to provide any office space for visitors.

But now also, on consulting Professor Rota, I find that he will not be collaborating with you at all (and indeed scarcely knows you) nor does there seem to be any connection or common interest with other people in the applied mathematics group at MIT. Under these circumstances, the proposed guest status, in the Mathematics Department, does not really seem to be appropriate and I think we should not pursue this further. It is certainly inappropriate in any case to list the Mathematics Department's main office as an address for editorial correspondence of the Hadronic Journal, a Journal with which this Department has no connection, and I trust you will rectify this immediately.

Sincerely yours,

Louis N. Howard, Chairman
Applied Mathematics Committee

/nt

cc: Ms. Gerane West
Prof. Gian-Carlo Rota

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

CAMBRIDGE. MASS 02139

DEPARTMENT OF MATHEMATICS

2-377

June 13, 1980

Dr. R. M. Santilli

[redacted]

Dear Dr. Santilli:

Professor Low has advised me that the listing of the Mathematics Department Headquarters as your address in the Hadronic Journal cannot be amended until the next issue. Please be assured that there will be no difficulty about this, and that any correspondence which might arrive for you during the summer will be promptly forwarded.

If you wish some address other than that above to be used for this, kindly advise Mr. James Dalton in the Mathematics Department Headquarters, Room 2-236.

Sincerely yours,

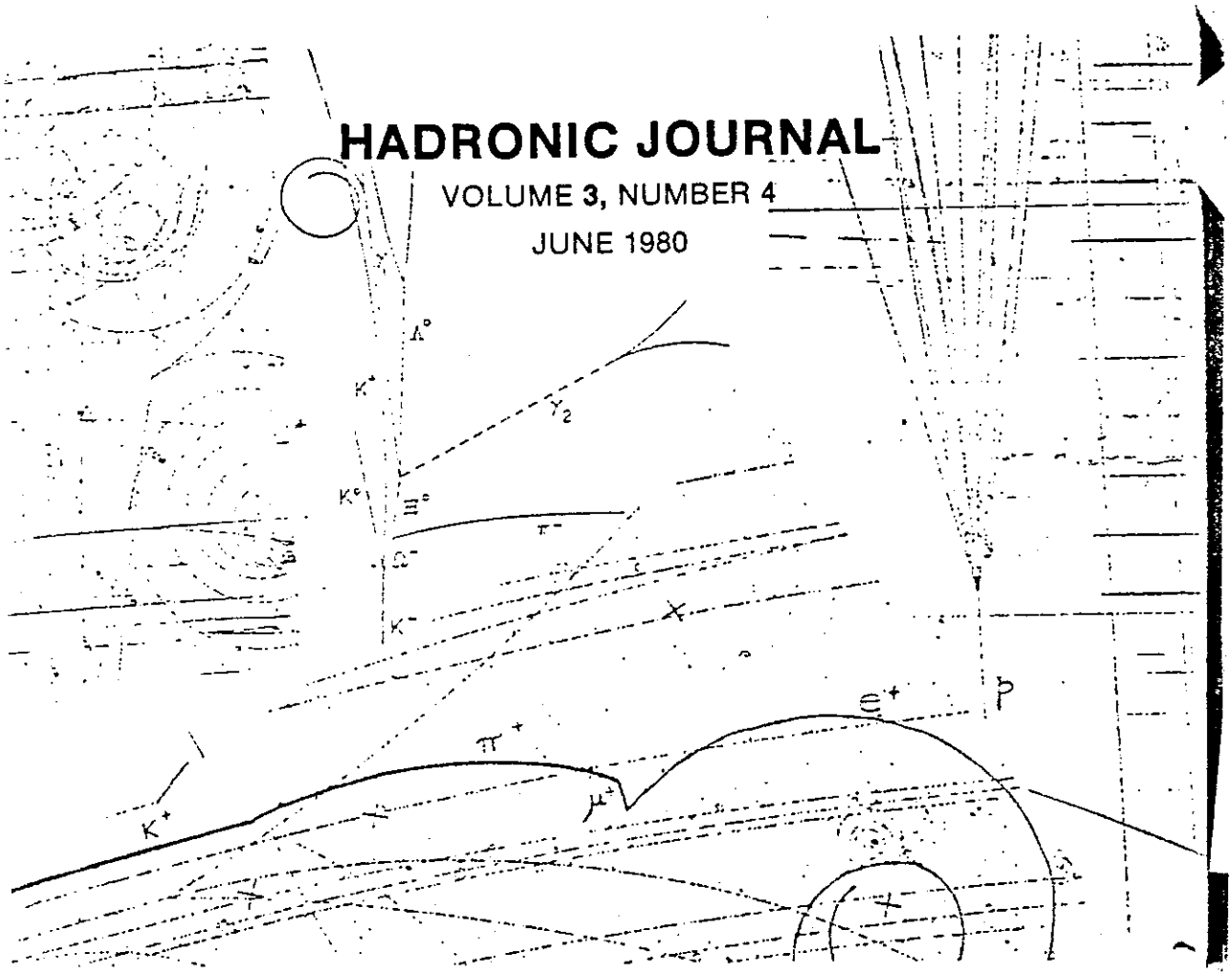
Louis N. Howard /gw

Louis N. Howard, Chairman
Applied Mathematics Committee

LNH/gw

Dictated by Professor Howard
and signed in his absence.

cc: James Dalton, Administrative Officer
Francis E. Low, Professor of Physics


HADRONIC JOURNAL
VOLUME 3, NUMBER 4
JUNE 1980
Λ°
K⁺
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HADRONIC JOURNAL

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The particle events printed in half tone in the front page of the Journal are derived from 80" bubble chamber photographs kindly provided by BROOKHAVEN NATIONAL LABORATORY

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

CAMBRIDGE, MASS. 02139

DEPARTMENT OF MATHEMATICS

August 27, 1980

Professor CLIFFORD G. SHULL
Department of Physics, Division of Nuclear Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professor Shull,

On March 19, 1980, during your leave, I visited your associates M.A.HORNE, D.K.ATWOOD, and J.ARTHUR for the purpose of indicating that your neutron interferometer equipment appears to be particularly suited for the experimental verification of the SU(2)-spin symmetry as well as of Pauli's exclusion principle under strong interactions. Copy of the correspondence with Mike Horne is enclosed.

I am referring, for instance, to suitable modifications and/or improvements of the initial tests on the 4π spinor symmetry already done by the European experimental group headed by Professor RAUCH (a copy of his last paper on the subject is enclosed).

On experimental grounds, the need for additional measurements are numerous. For instance, (1) the exact symmetry value of 720° barely makes it within experimental data (716.8 ± 3.8 deg); (2) the median angle in the latest as well as in the preceding experiments has a tendency to be below 720 deg; and (3) the best fit does not appear to be provided by a sinusoidal curve, as necessary for the exact symmetry (see the diagram of fig.3 of Rach's paper, p. 284).

On theoretical grounds, the need for additional measurements are equally numerous, and they have been discussed in detail in the specialized literature on the topic (see the enclosed list of references, copies of which were released to your associates). In its most rudimentary form a primary argument is as follows. For the case of the elm interactions the exact validity of the SU(2)-spin symmetry is incontrovertible, as established (for instance) by the property that the total angular momentum of a charged particle under an external elm field is conserved. For the case of the strong interactions the situation does not appear to be necessarily the same. As clearly indicated by available experimental data, strongly interacting particles are actually constituetd by wave packets in conditions of mutual penetration or overlapping (which is absent for the elm case, in general). This confirms the rather old expectation that one component of the strong interactions is constituted by a nonlocal, nonpotential (non-Hamiltonian) force. In turn, this is expected to imply the lack of applicability in an exact form of the entire Lie's theory, let alone that of the SU(2)-spin case. Irrespective from this aspect (or as a complement to it), the total angular momentum of a particle under strong interactions is not expected to be conserved (to avoid the perpetual-motion-type of approximation that, say, a proton orbits inside a star with a conserved angular momentum.....). In turn, this is expected to imply a form of breaking of the SU(2) symmetry. Needless to say, such a possible breaking can be only an internal effect of closed strong systems and, as such, not observable via external elm interactions. Also, for the case of the nuclear forces the effect can at most be quite small.

These ideas have been subjected to a quantitative study by a number of mathematicians and physicists via the so-called Lie-admissible generalization of Lie's theory. In essence, the approach studies the generalization of the Lie algebra/enveloping algebra/Lie group in such a way to permit the representation of nonpotential forces, whether local or not.

Also, the approach is applicable for the quantitative treatment of any broken Lie symmetry, and admits the conventional Lie theory as a particular case. The application of these new mathematical tools to the case of a strongly interacting particle under condition of penetration with other particles and expected nonlocal forces has provided: (A) the prediction of a conceivable deviation from the exact SU(2) symmetry of the order of at least 5×10^{-4} for the case of low energy nuclear processes; (B) the apparent interpretation of the "slow down effect" of the median angle; and (C) the apparent improvement of the fit of the experimental data by Rauch and his collaborators.

In conclusion, and to our best understanding at this time, the current experimental data appear to be compatible with both the exact and the broken SU(2) spin symmetry. The fundamental character of the symmetry for theoretical as well as applied physics (e.g. the problem of the controlled fusion) then warrants, in my view, additional experiments.

Since the time of my visit to your laboratory several developments have occurred, such as
- a number of experimentalists have answered my call for the initiation of a feasibility study for more refined experiments;
- I have delivered an invited talk at the recent Conference in Differential Geometry and Applied Mathematics held from July 23 to 25 at Clausthal-Zellerfeld, with encouraging results; and
- We recently had our Third Workshop in Lie-admissible Formulations here in the Boston area from August 4 to 9 with the participation of some 30 scientists, including mathematicians, theoretical and experimental physicists. The workshop was virtually devoted to the study of the problem.

In case you are interested in more detailed information, I would be happy to visit you either for an informal meeting or for delivering a seminar on the subject (I could essentially repeat my presentation at Clausthal-Zellerfeld). I can be reached more readily at my home address given below.

Best Personal REgards

Ruggero Maria Santilli
Visitor

RMS/ml

c.c. Professor FRANCIS E. LOW, M.I.T.
encls.

Z. Physik B 29, 281–284 (1978)

Zeitschrift
für Physik B
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Precise Determination of the 4π-Periodicity Factor of a Spinor Wave Function*

H. Rauch and A. Wilfing
Atominstitut der Österreichischen Universitäten, Wien

W. Bauspiess
Institut für Physik, Universität, Dortmund and
Institut Laue-Langevin, Grenoble

U. Bonse
Institut für Physik, Universität, Dortmund

Received December 12, 1977

The perfect crystal neutron interferometer is used to perform a precise determination of the 4π-symmetry of a spinor. The high precision is gained by the use of very well defined magnetic fields within two magnetized Mu-metal sheets, which are rotated in opposite directions within the coherent neutron beams. The periodicity of a spinor is determined to a value of $x = 716.8 \pm 3.8$ deg., which is in agreement with the theoretical prediction.

1. Introduction

The 4π-periodicity of a spinor wave function is a well known quantum mechanical property [1]. A 2π-rotation produces a phase factor of -1, which is usually not an observable quantity because quantum mechanical expectation values are quadratic in the wave function. Aharanov and Susskind [2] and, independently, Bernstein [3], predicted that the 4π-factor may be observed in certain experiments. Further suggestions for an experimental verification with neutron- and electron interferometers are given in literature [4–7]. In any case two coherent beams or states have to be created first and superposed afterwards.

Our group achieved the first verification of the 4π periodicity of the spinor wave function in 1975 [8] using the perfect crystal neutron interferometer developed previously [9]. Further measurements on this subject with another perfect crystal neutron interferometer [10], a Fresnel diffraction neutron interferometer [11], a molecular beam system [12] and for a NMR transition [13] have been reported. The basic theoretical treatment for a state where nuclear phase shifts and spinor rotations occur simultaneously within a neutron interferometer is given in [14, 15], and some of the related beat effects have also been verified experimentally [16].

In the first 4π-rotation experiment we obtained for the periodicity a value of $x = 704 \pm 38$ deg [8]. The main contribution to the error arises from the uncertainity of the magnetic field distribution along the paths of the coherent neutron beams due to the stray field of an air gap electromagnet (Fig. 1a). Therefore, we repeated this experiment with a much more well defined magnetic field distribution using magnetized Mu-metal sheets (Fig. 1b).

2. Theory

Within the neutron interferometer a coherent splitting of the incoming wave and a coherent superposition at the exit is achieved. The intensity of the beams behind the interferometer has contributions from the wave

* Work supported by Fonds zur Förderung der Wissenschaftlichen Forschung (Projekt 3185) and by Bundesminister für Forschung und Technologie 03-41A09

0340-224X/78/0029/0281/$01.00

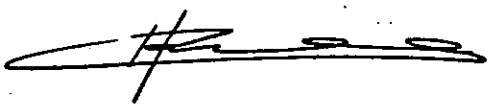
Atominstitut der Österreichischen Universitäten
Schüttelstraße 115
A-1020 Wien
Tel. (0222) 72 51 36
AUSTRIA
Prof.H.Rauch

Prof.R.M.Santilli
28 Cross Street
West Newton
MA 02165
U.S.A.

Wien, July 23, 1981

Dear Professor Santilli:

Before going to vacation I would like to answer your letters from January 19, March 23, and April 20, 1981 and to send you some supporting documents for an application of funds from DOE. Please change the numbers according to the usual application form and rewrite the text in good English. The proposed measuring procedure should give high accuracy indeed. Please sign also the research proposal for ILL-Grenoble and send it back to me or directly to ILL and a copy to me. I hope you have a delightful conference now at Boston and we will meet at the Orléans-conference. Please inform me as soon as you know about the decision of DOE concerning our common project. In principle we can start the experiment at the beginning of the next year.
Best wishes also to your family,
Yours sincerely,



Research Grant Application

Submitted to the

U.S. DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street

Cambridge, Massachusetts 02138

tel. (617) 864-9859

entitled

EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION

Proposed Starting Date:	Proposed Duration:	Amount Requested:
January 1, 1982	12 Months	$46,500

ENDORSEMENTS

H. Rauch
Principal Investigator
The Institute for Basic Research and Atominstitut
Cambridge, Massachusetts USA Wien, Austria
Tel. (617) 864-9859 Tel. (0222) 75 51 36

R.M. Santilli
Co-Investigator
The Institute for Basic Research
Cambridge, Massachusetts USA
Tel. (617) 864-9859

J. Summhammer
Co-Investigator
Atominstitut
Wien, Austria
Tel. (0222)75 51 36

A. Zeilinger
Co-Investigator
M.I.T. (and Atominstitut)
Cambridge, Massachusetts USA
Tel. (617) 253-4200

R.M. Santilli
President
The Institute for Basic Research
Soc. Sec. No. 032 46 3856
Tel. (617) 864-9859

Accounting Firm of the Institute
Vaccaro and Alkon CP, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
Att.: Mr. R. Alkon, President
Tel. (617) 969 6630

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
Att.: Mr. J. Grassia, Senior Partner
Tel. (617) 956-1700

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Appendices

A. Information on The Institute for Basic Research

B. Addresses of investigators

C. Papers:

H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse, Z. Physik B29, (1978), 281

H. Rauch and A. Zeilinger, Hadronic J. 4 (1981), 1280

R.M. Santilli, Hadronic J. 1 (1978), 574 (excerpts)

C.N. Ktoridss, H.C. Myung, and R.M. Santilli, Phys. Rev. D22 (1980), 892

G. Eder, "Physical implications of a Lie–admissible spin algebra", Hadronic J. 4 (1981), in press.

ABSTRACT

As it has been known for some time, the magnetic moment of neutrons can change within and perhaps even near the region of the strong interactions. The possibility of a corresponding change of the spin of neutrons under strong interactions was pointed out by R.M. Santilli (Hadronic J. 1 (1978), 574), and subsequently studied by several authors. More recently, G. Eder (Hadronic J. 4 (1981), in press) has pointed out possible fluctuations of the spin of the neutrons due to the magnetic field in the neighborhood of the nuclei, which are of the measurable order of one percent. All these effects can be tested most accurately via neutron interferometers, where widely separated coherent neutron beams are available. The most direct and precise test of the SU(2)–spin symmetry for neutrons has been done by H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse (Z. Physik B29 (1978), 281) via the test of the 4π periodicity of the spinorial wave function, yielding the value $\alpha_0 = 716.8 \pm 3.8$ deg. Recent corrections due to up–dated physical constants yield the value $\alpha_0 = 715.87 \pm 3.8$ deg which does not include the 720 deg expected for the exact SU(2)–spin symmetry. This proposal recommends a joint AUSTRIA–FRANCE–USA collaboration for the repetition of the experiment in such a way to render it most sensitive to the addition of the strong interactions, as well as to the electromagnetic fields in the vicinity of atomic nuclei. This can be achieved via an additional (Bi or Pb) phase shift placed alternatively into the coherent beams of the interferometer at a position with and without magnetic precession fields, as suggested by H. Rauch and A. Zeilinger (Hadronic J. 4 (1981), 1280) and R.M. Santilli (Hadronic J. 4 (1981), 1166). It can be estimated that a relative accuracy of $\Delta\alpha/\alpha_0$ in the range of 10^{-4} can be achieved by this advanced technique. It should be noted that the measure of any deviation from the SU(2)–spin symmetry due to strong interactions and/or other interactions at short range would require a suitable generalization of quantum mechanics, perhaps of the type studied at the yearly *Workshops on Lie–Admissible Formulations.*

PRIMARY BIBLIOGRAPHY ON THE PROBLEM OF THE EXACT OR APPROXIMATE VALIDITY OF THE SU(2)–SPIN SYMMETRY UNDER STRONG INTERACTIONS

PART A: THEORETICAL PAPERS

1 – R. M. Santilli,
'Need of subjecting to an experimental verification the validity within a hadron of Einstein's special relativity and Pauli's exclusion principle', Hadronic J. 1, 574 (1978)
NOTE: Only excerpts of this memoir are included.

2 – H. C. Myung and R. M. Santilli,
'Further studies on the recently proposed experimental test of Pauli's exclusion principle for the strong interactions'
Hadronic J. 3, 196 (1979)

3– C. N. Ktorides, H. C. Myung and R. M. Santilli,
'Elaboration of the recently proposed test of Pauli's principle under strong interactions', Phys. Rev. D22, 892 (1980)

4– R. M. Santilli,
'Experimental, theoretical, and mathematical elements for a possible Lie-admissible generalization of the notion of particle under strong interactions', Hadronic J. 4, 1166 (1981)

5— G. Eder,
'Physical implications of a Lie-admissible spin algebra'
Hadronic J. 4, 2018 (1981)

PART B: EXPERIMENTAL PAPERS

6 – H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse,
'Precise determination of the 4π periodicity factor of a spinor wave function', Z. Physik B29, 281 (1978)

7 – A. Rauch and A. Zeilinger,
'Demonstration of SU(2) - symmetry by Neutron Interferometers'
Hadronic J. 4, 1280 (1981)

Prepared by the staff of the Institute for Basic Research, Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138

EDITORIAL NOTE
prepared by the staff of
The Institute for Basic Research
on October 19, 1981

Professor RAUCH (President of the Atominstitut of Wien, Austria) and his associates have revised on July 1981 their best measure of the periodicity of the neutron wave function

716.8 ± 3.8 deg (1)

originally derived in Article 6 of this collection and discussed in Articles 4,5 and 7. The revision was suggested by up-dated physical constants and other reasons. The new value is given by

715.8 ± 3.8 deg (2)

Note that *the new measure does not include the value*

720 deg (3)

needed to establish via experiment the exact validity of the SU(2)-spin symmetry under strong interactions, which therefore constitutes an open physical problem at this moment, as pioneered in Article 1.

SOURCE:
Letter of Professor RAUCH to Professors SANTILLI (U.S.A.), FRONTEAU and TELLEZ-ARENAS (France) dated July 7, 1981
and
Invited talk of Professor EDER (Director for Theoretical Physics of the Atominstitute of Wien) at the
Fourth Workshop on Lie-Admissible Formulations
August 3-7, 1981, Cambridge, U.S.A.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

October 29, 1981

Dr. A. ZEILINGER
Department of Nuclear Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Dr. Zeilinger,

This letter is to review the recent events related to the research grant application entitled
"EXPERIMENTAL VERIFICATION OF THE SU(2) SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION"
which, as indicated by the enclosed copy of the front page, has been signed by all (scientific and administrative) participants except you.

As you know, the application arrived at our Institute from Wien on October 16, 1981 following the signature of the Principal Investigator Professor H. Rauch, and you were immediately informed. At our meeting the day following arrival of the application, you indicated the need of time to secure the M.I.T. authorization for you to sign. To assist you in this process, I indicated to you the following points.

(1) The staff of our Institute had investigated the administrative profile of your signature to ascertain whether there was any potential conflict with your current support at M.I.T. The results of the investigation were that no administrative conflict exists, provided that your current Governmental support at M.I.T. is fully disclosed in the application and, in addition, it is specifically indicated in the application that no proceed from the possible funding would be used for your salary.

(2) Your signature therefore appeared eminently an internal issue at M.I.T. With this in mind, I indicated that, on our part, we would welcome your signature and participation, with the understanding, of course, that your superiors at M.I.T. favor the experiment suggested in the proposal. Professor Rauch's approval of your participation is self-evident because he had suggested it in the first place.

(3) Since at our Institute we have no intention of interfering in the internal affairs of your college, I indicated also that, in case of unforeseeable difficulties, we would welcome the filing of the application in Washington without your signature. The separate decision whether or not you should participate to the experiment could then be taken at a later time.

Finally, as indicated and stressed to you, our Institute was unable to reach any decision (whether to file the application in Washington or return it to Wien) until you and your superiors at M.I.T. had reached a decision on the matter. As of today (October 29, 1981), no decision has been reached on your part, to our knowledge.

I am therefore formally asking you here that a decision be reached as soon

as possible, in order not to delay due scientific process on an experiment which is clearly of fundamental nature.

Your reply by letter would be appreciated.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-pm

cc. C.G.Shull, Head, M.I.T. Nuclear Division and
H. Feshbach, Head, M.I.T. Physics Department

WEDNESDAY, NOVEMBER 18

Massachusetts Institute of Technology

11:00 - Seminar on Optics and Quantum Electronics, RLE Conference Room, 36-428
Picosecond Electrical Pulses: Generation and Amplification
G. Mourou, University of Rochester

Harvard University

2:30 - Mathematical Physics Seminar, Jefferson 356
On Witten's Proof of the Morse Inequalities
Prof. Raoul Bott, Harvard

Boston College

3:45 - Refreshments, Higgins Hall Room 354
4:15 - Physics and Biology Colloquium, Higgins Hall, Room 307
Membrane Structural Transitions Modulate Growth Limits of E. coli
Dr. Andrew S. Janoff, Harvard Medical School/Massachusetts General Hospital and Department of Biology, Boston College

Boston University

3:45 - Refreshments, College of Liberal Arts Building, 725 Commonwealth Ave.
4:15 - Physics Seminar, CLA Building, Room 510
Inhibited Spontaneous Emission
Prof. D. Kleppner, M.I.T.

Massachusetts Institute of Technology

4:00 - Coffee and Tea, CTP Seminar Room, 6-322
4:30 - Joint Theoretical Seminar, CTP Seminar Room, 6-322
Random Lattice Field Theory
Prof. T.D. Lee, Columbia University

THURSDAY, NOVEMBER 19

SEE LAST PAGE OF CALENDAR FOR SPECIAL EVENT ANNOUNCEMENT AT M.I.T.

Massachusetts Institute of Technology

3:30 - Refreshments, Room 26-110
4:00 - Physics Colloquium, Room 26-100
Neutron Interferometry: Experiments with Matter Waves
Prof. Anton Zeilinger, M.I.T.

FRIDAY, NOVEMBER 20

SEE LAST PAGE OF CALENDAR FOR SPECIAL EVENT ANNOUNCEMENT AT M.I.T.

Massachusetts Institute of Technology

12:15 - Joint Experimental and Theoretical Particle Physics Seminar, CTP Seminar Room
Strings and Other Vacuum Structures
Prof. Allen Everett, Tufts University
Bring lunch; coffee provided

BOSTON AREA PHYSICS CALENDAR

The Boston Area Physics Calendar is published by the Physics Department, Tufts University. Items should be submitted to Celia Mees at 628-5000 x344. The deadline for submitting items is 1:00pm on the Monday preceding the week of the event.

- * -

NOVEMBER 16 - NOVEMBER 20

MONDAY, NOVEMBER 16

Harvard University

4:00 - Tea, Jefferson, Room 461
4:30 - Physics Colloquium, Jefferson, Room 250
10^{-35} Seconds After the Big Bang
Prof. Alan Guth, M.I.T.

TUESDAY, NOVEMBER 17

Massachusetts Institute of Technology

10:30 - Coffee, Marlar Lounge, Room 37-252
11:00 - Seminar on Modern Optics and Spectroscopy, Marlar Lounge
Macroscopic Coherence in Atomic Vapors
Thomas W. Mossberg, Harvard University

Brandeis University

2:00 - Theoretical Physics Seminar, Bass, Room 229
Confining Models of the Weak Interactions in Technicolor
Dr. E. Farhi, M.I.T.

Massachusetts Institute of Technology

3:45 - Coffee, Marlar Lounge, Room 37-252
4:15 - Astrophysics Colloquium, Marlar Lounge, Room 37-252
General Relativity and the Binary Pulsar
Dr. Saul Teukolsky, Center for Astrophysics (on leave from Cornell)

Massachusetts Institute of Technology

4:00 - Refreshments, Room 26-414
4:15 - Nuclear Physics Seminar, Room 26-414
The Measurement of Magnetic Moments of High-Spin Rotational States
Dr. L. Grodzins, M.I.T.

Brandeis University

4:00 - Coffee and cookies, Physics Building, Bass 333
4:30 - Martin Weiner Lecture Series Physics Colloquium, Nathan Goldstein Lecture Hall, Abelson 131
Molecular Orbitals in 1st Row Diatomics, Where is the $2p\sigma^*$ Orbital?
Prof. Russell A. Bonham, Indiana University

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

November 30, 1981

Professor L. GRODZINS,
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Grodzins,

Some of the members of our Institute did listen to your talk at MIT on November 17, 1981 entitled "The measurements of magnetic moments of high spin rotational states".

It appears that there is a considerable connection between your studies and those by Professor RAUCH (Director of the Atominstitut of Wien) on the test of the spinor symmetry of neutrons via neutron interferometers. In fact, in both cases we have an apparent breaking of the SU(2)-spin symmetry due to intense fields which alter the space geometry of charge distributions. In both cases the data can be apparently studied in a quantitative way via a Lie-admissible generalization of the SU(2) algebra (the simplest one is the replacement of the associative product AB with the isotopic product ATB, T = fixed). In both cases the SU(2)-spin symmetry is broken even when the value of the magnitute of the spin and of its third component are the conventional ones (recent results by G. EDER). In fact, the breaking-mutation of conventional charge distributions under intense fields can affect only the other components of the spin and the Casimirs of order higher than two.

In case you are interested, please come and see me for a friendly conversation on the topic (regrettably, I could not attend your seminar). Also, we are having an International Conference in France (January 1982) which will be much on the topic, with numerous mathematicians presenting the structure of the SU(2)-spin breaking and its Lie-admissible generalization for the quantitative treatment of the kroken context.

Sincerely

Ruggero Maria Santilli
RMS-vf

P.S. I feel obliged to enclose xerox-copy of a recent publication by our Institute, including the recent experimental data by Rauch which DO NOT reproduc the exact SU(2)-symmetry, even though, of course, are still inconclusive. This is dictated by the fact that, subsequent to your talk, Mr. ZEILINGER of your nuclear physics division delievered a seminar on Neutron interferometry, including the fundamental spin experiment by Rauch, but he FAILED to mention the recent numbers, as well as the crucial contribution by Eder. I can personally testify that Zeilinger was fully informed of these advances. His silence is therefore a rather preoccupying and self-qualifying occurrence.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

December 4, 1981

Professor R.M. Santilli
Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Mass. 02138

Dear Professor Santilli:

There seems to be considerable confusion as to the nature of the talk that I gave at M.I.T. on November 17, entitled, "The Measurements of Magnetic Moments of High Spin Rotational States." There is no connection between these studies and those by Professor Rauch on the test of the spinor symmetry of neutrons via neutron interferometers. I regret that members of your institute who heard my talk came away with the wrong impression. I obviously did not explain the material clearly enough for them.

Sincerely yours,

L. Grodzins

Professor Lee Grodzins
Physics Department

LG:bak

December 9, 1981

Dr. LEE GRODZINS
Physics Department
M.I.T.
Cambridge, Massachusetts 02139

Dear Dr. Grodzin,

I am in irreconciliable disagreement with your letter of December 4.

(1) Your experimental results are a clear indication of the breaking of the SU(2)-spin symmetry in nature. Period. Explicitly, the most direct possible interpretation of your results is that via the breaking of the SU(2)-spin symmetry (on which several thousands of pages of research have been written--all ignored at MIT). Of course, the interpretation is not final. The point that even a child can see (but not MIT people) is that the interpretation cannot be excluded. Only the well known mumbo-jambo dances of academic politics can exclude it.

(2) Your experiment is directly related to the fundamental experiment by Professor Rauch on the spinor symmetry of nucleon-nucli interactions, because you merely do nuclei-nuclei interactions. Period. Physics is one. The physical laws in your experiment and that by Professor Rauch are exactly the same. On financial-academic grounds the situation is different, inasmuch academicians may attempt dances to distinguish physical laws for certain academic objectives. But we then have no scientific content. Only a political one.

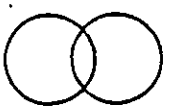
(3) Your firm rejection of clear point (2) in your letter of December 4, and your firm rejection of point (1) apparently made during your seminar may give the impression that you are acting under what is called "MIT politics". You should be fully aware of it. Of course, nobody can prove it. It is an allegation. Yet the doubt persists. Permit me the liberty to let you know, as an outsider, that the terms "MIT politics"are becoming more and more known as meaning "politics in favor of MIT interests, at times in disrespect of the true pursue of novel human knowledge, and, at the extreme, potentially against national-technological interests for advancements".

In the case at hand, there are known financial-academic interests at MIT, documented through the years, favoring the preservation of the SU(2)-spin symmetry, or that, at any rate, would be damaged by the possible theoretical-experimental establishing of the breaking of the SU(2)-spin symmetry. The problem is that this type of academic mumbo-jambo is done via a river of public funds.

I am concerned; I am very concerned indeed, because what I have been seen at MIT during the last years can only result into a crisis.

Very Truly Yours

Ruggero Maria Santilli



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

February 5, 1983

Professor W. F. WEISSKOPF
Center for Theoretical Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Weisskopf,

I would gratefully appreciate the courtesy of your review of the enclosed paper by Professors [redacted] and [redacted] from the [redacted] which has been submitted for publication to the HADRONIC JOURNAL.

As you can see, the paper deals with certain aspects of the bag model of hadrons which were developed by you and your collaborators at MIT.

The paper needs a generous refereeing, and I thought that you were the most qualified person for the review.

Thank you for your consideration and time.

Very Truly Yours

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS-mlw

encl.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

February 23, 1983

Professor Ruggero Maria Santilli
Editor in Chief
Hadronic Journal
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Professor Weisskopf asked me to look at the manuscript you recently asked him to referee. It appears to me, from the cover letter accompanying the manuscript, that the authors have not submitted the paper for publication but merely sent your institute a copy of one of their preprints.

Sincerely,

Robert L. Jaffe

RLJ:jm

enclosure

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

March 4, 1983

Professor ROBERT L. JAFFE
Massachusetts Institute of Technology
Department of Physics
CAMBRIDGE, Massachusetts 02139

Dear Professor Jaffe,

We were suprised in reading your letter of February 23, 1983. The paper by Professors [redacted] and [redacted] has indeed been formally submitted to the HADRONIC JOURNAL as you can see from copy of the formal letter of submission.

Very truly yours,

[redacted]

Editorial Office
HADRONIC JOURNAL

cc: Professor Weisskopf

mlw

PART V:

NORTHEASTERN

UNIVERSITY

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

Area Code 617
495-2170

Science Center
One Oxford Street
Cambridge, Massachusetts 02138

May 5, 1980

Professor ROY WEINSTEIN
Chairman
Department of Physics
Northeastern University
BOSTON, Massachusetts 02115

Dear Professor Weinstein,

To confirm our phone conversation of this afternoon, I am interested in a visiting (or guest) position at your department for next academic year, and preferably beginning from June 1980. Besides carrying no salary, the position would merely allow me to use the library-parking-address facilities of your Campus. An office would be appreciated, but it is not essential, as we are organizing the editorial office of the Hadronic Journal here in Cambridge on a permanent basis (on facilities close to Harvard University). No salary is, of course, expected for this visiting (guest) status, and all logistic expenses (xeros, etc.) can be paid from my own research funds.

During this year of visiting (guest) status, I would like to have the opportunity of applying for a research grant from the Department of Energy under the administration of your Department. This grant should cover all my salary and research expenses as principal investigator. It would be a pleasure for me to include interested colleagues from your Department as co-investigators.

As indicated to you since the time of my first visit, Harvard University has kindly provided hospitality for my DOE grant for the past two years. It is appropriate for me to seek hospitality from some other Institution for the continuation of this grant. I understand that the year 1980-1981 is considered for administration of my grant by a non-academic Institution. My visiting status during this period should provide ample time to prepare the research grant application to the satisfaction of your administration.

Thanking you for your courtesy and time, I remain

Yours Very Truly

Ruggero Maria Santilli

RMS/ml

NORTHEASTERN UNIVERSITY

BOSTON, MASSACHUSETTS 02115

ROY WEINSTEIN, CHAIRMAN
DEPARTMENT OF PHYSICS

June 26, 1980

Dr. Ruggero M. Santilli
Harvard University
Department of Mathematics
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli:

This letter is to state more formally what we have discussed by phone.

I invite you to visit our Department for the 1980-81 academic year. It will be a pleasure to have a researcher of your intellectual abilities to add stimulus to our particle theory groups.

The Department will arrange library access, parking, mail facilities, etc., and minor logistic support such as xeroxing. Major logistic support will have to be arranged as you suggest, via use of your research funds.

We will act as an umbrella for proposals you may wish to make which are within our Department constraints (which are minor).

I am fairly sure that we can arrange office space - virtually certain. However, I must review this with our Building Committee since we are a bit crowded, due to recent growth. The office space will probably be an office shared with another visitor.

When you want to start your visit, please tell our Administrative Assistant, Pat Kent, 437-2902, who will get things moving.

Sincerely,

Roy Weinstein
Chairman
Physics Department

RW:pk

cc: Prof. E. von Goeler, Executive Officer

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

July 9, 1980

Professor ROY WEINSTEIN,
Chairman
Department of Physics
Northeastern University
BOSTON, Massachusetts 02115

Dear Professor Weinstein,

It is a pleasure for me to accept your invitation to visit your Department for the 1980-1981 academic year, as per your letter of June 26, 1980.

I would appreciate whether your Department can arrange for library access, parking, and mail facilities and let me know at my home address [redacted]

Your kind offer of supporting xeroxing expenses has been particularly appreciated. All other logistic expenses, such as telephone, etc., will be paid out of my research funds.

In regard to a possible office, on my part I would be glad to bring to your Department the Editorial Office of the Hadronic Journal (of which I am, as you know, the editor in chief). Nevertheless, this would call for a small office anywhere in your building where I can fulfill my duty of confidentiality for consultation (direct or by phone) with referees and authors. In case this small office is not available, I would be glad to share an office with a colleague, but in this case I have to keep the editorial office of the Journal in another location. The communication of an early decision in this respect would be appreciated.

After the initiation of the academic year, I would be interested to deliver an informal seminar course for graduate students (whithout any charge to the Department), along the lines of the course I delivered at Lyman Laboratory in 1978-1979 (see the enclosure), as well as along my research monographs with Springer Verlag. The topic essentially deals with a variety of theoretical means to treat nonpotential forces as they occur in the real world. In principle, such an informal seminar course might be of interest to graduate students in mechanics, space mechanics, plasma physics, engineering, as well as high energy physics. Depending on the receptiveness of the students to this rapidly expanding new sector of research, I can deliver either a summary, one-semester presentation or a detailed two-semesters presentation. Due to the intended informality of the meetings, I am contemplating to contact you in this respect sometime in September for proper authorization and procedure. Nevertheless, in case you desire an outline of the course prior to September, please let me know and I would be glad to prepare it.

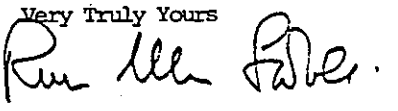
My whereabout for this summer is the following. On July 19 I shall leave for Chausthal, West Germany, to deliver an invited talk on the current experimental study for the test of Pauli's exclusion principle under strong interactions at the 1980 Conference on Differential Geometric Methods in Mathematical Physics. I should be back to the Boston area by July 27.

From August 4 to 9 we have the Third Workshop on Lie-admissible Formulations (also for nonpotential forces) which this year will be held at the Faculty Club of the New Harbor Campus of the University of Massachusetts (the meeting room has a beautiful view of the Boston Harbor). This yearly workshop is organized by the Hadronic Journal. Half of the participants are mathematicians and half are physicists. Needless to say, any colleague from your Department who is interested to attend the Workshop would be welcomed.

From August 15 to August 30 I will be back to Europe for working sessions with experimentalists interested in verifying the validity (or invalidity?) for the strong interactions of the conventional physical laws of the electromagnetic interactions.

Beginning from September 1 I should be able to initiate my regular attendance to your Department. I look forward to it with sincere pleasure.

Very Truly Yours



Ruggero Maria Santilli

RMS/ml
encls.

C.c. Professor E. VON GOELER, Executive Officer
Ms. PAT KENT, Administrative Assistant

NORTHEASTERN UNIVERSITY
360 HUNTINGTON AVENUE
BOSTON, MASSACHUSETTS 02115

COLLEGE OF ARTS AND SCIENCES
OFFICE OF THE DEAN

July 16, 1980

Dr. Ruggero M. Santilli
[redacted]

Dear Dr. Santilli:

Upon the recommendation of Professor Weinstein of the Department of Physics in the College of Arts and Sciences, I am pleased to extend to you our cordial invitation to spend the period of your leave from Harvard University at Northeastern University as Visiting Senior Research Associate of Physics.

Your appointment here will cover the period of August 1, 1980 through June 30, 1981, and carries with it a stipend of $1.00, which will enable you to use the University's library, laboratory, and parking facilities.

I believe that you can make a fine contribution to our program and gain valuable experience at the same time. I look forward to having you with us.

Sincerely,

Richard Astro
Dean

ng

cc: Department of Physics
Dean's Office File
Office of Personnel

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

July 18, 1980

Professor RICHARD ASTRO,
Dean
College of Arts and Sciences
Northeastern University
BOSTON, Massachusetts 02115

Dear Professor Astro,

I would like to express the sentiments of my sincere gratitude for your letter of July 16, 1980 and for your confirmation of my visit. It will be a pleasure for me to spend a year at Northeastern.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml

c.c.: Prof. R. Weinstein
Department of Physics

PART VI:

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

August 30, 1979

Head Search Committee
Physics Department
VPI & SU
BLACKSBURG, Virginia 24061

Dear Committee Chairperson,

I would like to express my interest for the position of Chairman of your Department, as advertised in the August issue of PHYSICS TODAY.

If my qualifications are not suitable for the chairmanship opening, I would appreciate the consideration for a regular faculty opening.

I am currently the recipient of a DOE grant for research in high energy physics that expires on June 1980. To keep continuity on this project, I should apply for an extension sometime during the end of 1979. I would appreciate whether a decision on my case can be reached during this calendar year.

I enclose for your consideration my curriculum, a list of referees, and other informative material on my teaching, research and editorial activities, while I remain at your disposal for any assistance you might need.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.

COLLEGE OF ARTS AND SCIENCES

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

Blacksburg, Virginia 24061

DEPARTMENT OF PHYSICS

October 1, 1979

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your recent application for our Department Head position. We certainly appreciate your interest and look forward to considering your application.

Please find enclosed a "faculty race/sex identification form"; we are required to send you this form and to ask your indulgence in filling it out and returning it to us at your earliest convenience.

With best regards,

C. D. Williams
Chairman, Head Search Committee

CDW:hh

Enclosure

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

October 15, 1979

Professor C. D. WILLIAMS
Chairman, Head Search Committee
Virginia Polytechnical Institute and State University
Department of Physics
BLACKSBURG, Virginia 24061

Dear Professor Williams,

I would like to express my appreciation for your recent letter acknowledging my application, and for your consideration.

As per your request, I enclose a duly completed "faculty race/ sex identification form", while I remain at your disposal for any assistance you might need. I understand that you have solicited letters of recommendation on my behalf, and I shall abstein from soliciting them on my own. Nevertheless, in case you need any assistance in this respect (e.g., names of additional distinguished scolars), please let me know.

This morning I have separately mailed to Professor MARSHAK copies of my monographs
- "Foundations of Theoretical Mechanics" with Springer-Verlag, and
- "Lie-admissible approach to the hadronic structure" with the Hadronic Press.

Also, I have mailed copies of the reprint volumes
- "Applications of Lie-admissible algebras in Physics" Vols I and II edited by Professors H.C. MYUNG, S. OKUBO and myself, and published by the Hadronic Press.

Copy of my letter to Professor Marshak is enclosed.

During the consideration of my candidacy, please take into consi-detation that I am interested and I would be honored to assume the chairmanship of your Department. As you can see from my curriculum, I have been chairman of the board of director of a Massachusetts corporation for four years. Also, I have organized the HADRONIC JOURNAL, that comprises in its editorial organization truly distinguished scientists, including two Nobel Laureates. Finally, I have numerous years of experience in academic committees. I therefore feel confident that, in case appointed, I can effectively serve your Department, with priority to a genuine loyalty to each individual member, as well as in the continuation of a relaxed and humanly rewarding atmosphere.

Nevertheless, my primary interest is in joining your department as professor of theoretical physics. Therefore, I would appreciate the possibility of being also considered for a normal faculty position. I would like to release to the Search Committe the selection between the consideration for chairman or for a faculty position without a chairmanship appointment.

Finally, during the consideration of my application, I would appreciate whether you can take into account the following aspect. I am currently the recipient of a research grant from the U.S. DEPARTMENT OF ENERGY. The DOE is apparently pleased with the research output of this grant and it is seriously interested in continuing it. Please feel free to contact the DOE officer in charge of my case, Dr. DAVID C. PEASLEE, Tel. 301 353 3624.

This grant expires on June 1, 1980. To keep continuity of grant, I should reapply sometime before the end of this calendar year. It is advisable that this application is filed with my new Institution, in order to avoid the usually long procedure of grant relocation.

In case my application is accepted by your Department, I would then appreciated an appointment without salary for the period December 1979-May 1980; a regularly salaried appointment beginning from June 1, 1980 until September 1, 1980 (the summer period), but with salary entirely covered by my grant; and finally, a regular appointment with teaching functions beginning from September 1, 1980 on.

In particular, the initial appointment from December-1979 to June 1 1980 should be such to allow me to apply for a DOE grant as principal investigator. In case senior members of your Department are interested in joining me in this application, I would be pleased and honored. As a matter of fact, I would like to take this opportunity to stress that I am sincerely interested in establishing the best possible collaboration with any interested member of your Department.

Yours, Very Truly

Ruggero Maria Santilli

RMS/ml

c.c.: Professor MARSHAK

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 15, 1979

Professor R. E. MARSHAK, Department of Physics
Virginia Polytechnical Institute and State University
BLACKSBURG, Virginia 24061

Dear Professor Marshak,

As you eventually know, I have recently applied for a position at your Department, following a suggestion by Professor OKUBO. Copy of my recent letter to Professor WILLIAMS is enclosed. Please note in this letter that I would be honored to assume the chairmanship. Nevertheless, I am also interested in joining your department in a regular faculty position. I would like to release to your Department the selection of the position for the consideration of my candidacy which is most appropriate.

In this letter I would like to take the liberty of outlining a rather intriguing research program, which is rather feverishly pursued by a number of mathematicians and physicists. In particular, the mathematicians of our group are

- Professor H.C.MYUNG of the University of Northern Iowa (currently on leave in Korea); Professor TOMBER of the Michigan State University; Professor OEHMKE of the University of Iowa; Professor WENE of the University of Texas at San Antonio; and others.

The physicists actively involved in our line of study are

- Professor OKUBO of Rochester, Professor KTORIDES of the University of Athens (currently spending his sabbatical as my guest here at Harvard), Professor CONSTANTOPOULOS also of the University of Athens in Greece; Professor KOBUSSEN of the Institut für Theoretische Physik der Universität Zürich; Professors CANTRIJN and SARLET* (the latter spent his sabbatical of 1978-1979 as my guest here at Harvard); Professors FRONTEAU and TELLEZ-ARENAS, of the Universite d'Orléans in France; Professor SALINGAROS of the University of Massachusetts in Boston; Professor ELIEZER and his group at the La Trove University in Australia; Professor LÕHMUS of the USSR Academy of Science; and Professor JIANG CHUN-XUAN of the People's Republic of China.

The number of mathematicians and physicists joining our research is expanding in a quite promising way. The efforts are coordinated via (A) the conduction of a yearly Workshop, Called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (the first was held on August 1978 at Harvard; the second was held in August 1979 also here at Harvard; and the third is scheduled for August 1980). (B) the organization of a mathematical conference on

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* of the Instituut voor Theoretische Mechanica of the Rijksuniversiteit Gent, Belgium.

Lie-admissibility (the first is scheduled for the spring 1981, upon return of Professor Myung from Korea); and (C) the coordination of the studies via the HADRONIC JOURNAL, of which, as you are eventually aware, I am the founder and editor in chief.

To keep a record of research, all contributions are reprinted in a yearly series entitled "APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS". edited by Professor MYUNG, OKUBO and myself. Volumes I and II were printed in 1978. We are currently working on two additional volumes, the first comprising all studies by mathematicians and physicists up to 1978 (including an historical paper by ALBERT in which he introduced the notion of Lie-admissibility), and the second comprising all contributions in 1979. The proceedings of the second workshop will be published in the December issue, 1979.

In addition to articles I have written and I am writing in collaboration with mathematicians and physicists, my personal contributions in this line of studies are complemented via the following research monographs:
- "Foundations of Theoretical Mechanics", Volume I (1978) and II (in press) published by Springer-Verlag, Heidelberg, under the series "Texts and monographs in physics" edited by Professor BEIGLBÖCK, and
- "Lie-admissible approach to the hadronic structure", Volume I (1978), II(in press) and III (scheduled for 1980), published by the Hadronic Press, Nonantum, Massachusetts in the series "Monographs in theoretical physics".

I have taken the liberty of mailing you, by separate parcel, copies of all printed volumes. Copies of the typescripts of the volumes now in print are at your disposal.

I have also mailed to you, separately, a "chart" of my Volume II with Springer-Verlag, which essentially outlines, in a way understandable by graduate students, the technical treatments of our Second Workshop. (the proceedings of this workshop will be available in 1980 and I shall mail you a copy soon thereafter). Also I have mailed to you copy of a paper entitled "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" which was intended to stimulate a moment of reflection on current trends in hadron physics (this paper was distributed world wide in some 15,000 copies). The paper has been accepted for publication by FOUNDATIONS OF PHYSICS, and I am working to finilize it, with the inclusion of numerous comments and generous help I received, including by quark committed physicists, as well as with the editorial assistance of Professor BIEDENHARN.

As you can see, the material which has already accumulated on these studies is considerable and expanding considerably. Hoping that I do not take too much of your time, in this letter I would like to provide an outline of the essential aspects. Please consider my presentation as informal as possible (I simply did not have the time to rewrite this letter up to all the necessary maturity- so please kindly excuse any

insufficiency and deficiency). I hope in this way you may have an idea of what we are doing, without spending too much of your time in filtering it through the existing literature. Also, please consider this presentation as my personal view. Our colleagues and friends working on this line may indeed have a different perspective.

THE HISTORICAL VOICES OF DOUBT ON THE TERMINAL CHARACTER OF THE CURRENTLY USED BASIC LAWS FOR THE STRONG INTERACTIONS. When I decided to initiate active studies on the strong interactions during my graduate studies at the University of Torino, Italy, I decided to give priority to the identification of the teaching by the Founding Fathers of contemporary physics. I identified in this way a number of authoritative, historical, voices of doubt, that are certainly known to you.

FERMI made it quite clear in his teaching and historical notes "Nuclear Physics" that he did not believe in the validity of conventional geometries (the symplectic and the Riemannian, in our current language) in the region of space "occupied by a strongly interacting particle". To my reconstruction via correspondence with his colleagues, he thought that the strong forces are local and non-derivable from a potential, as an approximation of expected non-local settings. In this way, a conventional Hamiltonian description did not exist, in his view, for an effective representation of the strong interactions. This, in turn, would imply the lack of existence of conventional Lie algebras at the level of the time evolution law, both classically and quantum mechanically. Still in turn, this would call for the inapplicability of conventional relativity and quantum mechanical laws, thus callig for the courageous construction of broader methods, relativities and laws, specifically conceived for the physical arena considered.

EINSTEIN is well famous for his lack of belief in the terminal character of the undeterminacy of quantum mechanics, which he kept up to his death. As you certainly know, this historical occurrence ("Einstein legacy" in my language) is touchingly reported in Heisenberg's memoires "Physics and Beyond", pp. 79-81). In Heisenberg's words, Einstein could at most tolerate conventional quantum mechanics as a "temporary expedient".

JORDAN is equally famous for his doubt on a central mathematical structure of quantum mechanics, in our current language, the universal enveloping associative algebra of a Lie algebra. He suggested a broadening of this structure which resulted in the now celebrated Jordan algebras.

PAULI made it quite clear in his historical papers and lectures that his principle had been conceived under the conditions of lack of overlap of the wave packets, that is, the atomic structure. When the wave packets did indeed overlap, he was simply unable to establish the totally antisymmetric character of the wavefunction because of the emergence of "stronger" forces which would prohibit even the separability of the wave function, in general, let alone the identification of its totally antisymmetric character(for fermions).

WIGNER also made it quite clear in his memoir "Symmetry and Reflections" that there are fundamental open problems on the notions of interations in general, let alone the strong.

VON NEUMANN joined Jordan and Wigner in these doubts, in their celebrated paper of 1934.

My list of historical, autoritative, voices of doubt could continue.

THE ROLE OF THE HADRONIC JOURNAL. When I decided to organize the HADRONIC JOURNAL in January 1978, the situation was essentially the following. We had all these numerous, authoritative, historical voices of doubt on the applicability to the strong interactions of tools, laws, and insights conceived specifically for the electromagnetic interactions. Nevertheless, we had:

- no coordinated effort aiming at an in depth study of these legacies;
- no initiation of the studies for the possible construction of more general mathematical tools and physical laws for the strong interactions; and, most importantly,
- no initiation of studies aiming at the formulation of experiments on the resolution of these legacies.

I am happy to report to you that, thanks to the participation by numerous mathematicians and physicists, the HADRONIC JOURNAL has made valuable contrinutions in each of these aspects, as I shall outline below.

Also, at the time of the founding of the HADRONIC JOURNAL we had the known of proliferation of papers along quark lines. Nevertheless, as admitted by a number of qualified quark supporters, these models are faced with rather fundamental, open problems, such as (1) the lack of identification of the quarks with physical particles (despite Fairbanks' indication of fractional charges); (2) the yearly multiplication of different, unidentified quarks, as forced by new experimental data; (3) the lack of achievement of a genuine form of confinement which prohibits, according to clearly proved and explicitly stated calculations, decays of the type $\pi^0 \longrightarrow q\,\bar{q}$ (NAMBU has clearly stated this deficiency at his speech in Israel in connection with Einstein's celebration); etc.

The priority which I have given to the HADRONIC JOURNAL vis-a-vis with this situation is that of considering specific models of structure of hadrons as of purely secondary physical value. Instead, utmost priority is given to the experimental and theoretical finalization of the basic physical laws. The problem, in my view, is that only after achieving a finalization of these laws, studies on hadron structure can acquire a solid ground of scientific credibility, whether these studies are of quark orientation or not.

In conclusion, the HADRONIC JOURNAL has initiated a process of critical examination of the basic laws currently used in hadron studies, which

takes in full account the teaching by the Founding Fathers of contemporary physics, and which is primarily devoted to the achievement, in due time, of their experimental resolution, either in favor or against.

THE CONCEPTUAL OUTLINE OF THE STUDIES CONDUCTED AT THE HADRONIC JOURNAL.

Our starting point is Fermi's teaching. All hadrons have approximately the same "size" which coincides with the "range" of the strong interactions. Thus, as a necessary condition to even activate the strong interactions, hadrons must enter into a state of penetration of their wave packets. By using similarities with the atomic context, the same situation is then expected for the hadronic constituents, that is, they are in a state of mutual penetration of the wave packets during the life of the system. This produces a view of the structure of hadrons which is fundamentally different than that of the atomic structure (in which no appreciable overlapping of the wave packets occurs). According to an established physical line, fundamentally different physical arenas generally imply differences in the dynamical behaviour and in the physical laws.

Most of the technical efforts for Lie-admissibility are centered in the identification of the implications of such a condition of overlapping of the wave packets. That is, we have taked Fermi teaching seriously, and worked out, as much as possible, its implications.

Predictably, Fermi teaching turned out to be fully correct. A recently proved no-go theorem on (pre)symplectic quantization essentially establishes that the symplectic (and Riemannian) geometry do not produce a consistent quantization under forces more general than $f = -\partial V/\partial r$, as expected by the overlapping of the wave packets. This theorem has been studied at the recent second workshop, and it will be considered in detail in the proceedings. It essentially turns out that Heisenberg's equations are inconsistent for the arena considered.

The inconsistencies are apparently removed via the replacement of the conventional Lie algebras with their algebraic coverings called Lie-admissible algebras. The enlargment of the product then allows the direct representation of broader forces (variationally non-self-adjoint), thus directly allowing the treatment of the conditions of overlapping of the wave packets.

Additional theorems have established the "universality" of the Lie-admissible character of the time evolution law, classically and quantum mechanically, for the representation of the broader systems considered. Apart technical aspects, what we have is a return to the analytic equations originally conceived by Hamilton and Lagrange, those with external terms, (representative precisely of these broader forces). Permit me to stress, as I did it in my monographs with Springer-Verlag, that the "Lagrange's equations" and "Hamilton's equations" currently referred to in the con-

temporary physical and mathematical literature are not those conceived by the Founding Fathers of Analytic Mechanics; instead, they are in the "truncated" version without the external terms. In different terms, Lagrange's and Hamilton had an analytic vision substantial broader then that of their followers in high energy physics, who restrict the basic equations in their "truncated" form, and attempt the representation of reality via the simplistic structure $L = L_{free} + L_{int}$. Notice that this situation occurs at all levels of contemporary condution of conventional studies, up to the most advanced technical developments, such as non-abelian gauge theories and QCD. We simply have $L = L_{free} + L_{int}$. Nothing more. I am confident you will see that Fermi's vision on the complexities of the strong interactions is fully in line with Lagrange's and Hamilton's vision of Analytic Mechanics. Within this context, current trends emerge as a a first approximation.

Einstein's legacy has also been taken seriously and worked out to the best possible details. It essentially turns out that, when the wave packets do not overlap, the forces are variationally self-adjoint (action at a distance forces only). Then, the conventional uncertainty of quantum mechanics applies in full. Thus, Bohr and Heisenberg were indeed right in refuting Einstein's counterexamples (all based on this type of force). Nevertheless, when Fermi's point is taken in full consideration, Einstein's vision does indeed emerge as being true, to our best understanding. In simple words, the time evolution under nonselfadjoint forces is necessarily nonunitary. This implies that, assuming Heisenberg's indeterminacy principle is valid at a given time, it is not valid at a later time. In actuality, the no-go theorem of symplectic quantization prohibits the consistent formulations of all the algebraic ingredients for the formulation of the conventional Heisenberg's principle, let alone to establish it on physical grounds. In conclusion, Fermi's and Einstein's visions turned out to be deeply interrelated.

Jordan's legacy has also been taken seriously. As a matter of fact, his teaching is at the foundation of the physical and mathematical use of the Lie-admissible algebras. Indeed, in order to treat forces more general than $f = -\partial V/\partial r$, we are forced to an enlargment of the basic envelope of quantum mechanics, from an associative Lie-admissible form (the conventional one) to a nonassociative, but still Lie-admissible form. In particular, this form turns out to be Jordan-admkssible, thus including Jordan's view in its entirety. But, once the basic envelope is enlarged, conventional relativities and principles of quantum mechanics are forced to leave the way to broader views of Lie-admissible (rather than Lie) character. Thus, Fermi's, Einstein's and Jordan's visions turned out to be deeply interrelated.

Pauli's teaching has also been taken seriously by us. It turns out that a system of identical fermions, when it penetrates hadronic matter (e.g., a star) is subjected to broader forces whose time evolution law is nonunitary. Thus, assuming that they are fermions at a given value of time, they do not preserve this statistical character in time. The lack

of preservation of this statistical character then implies the inapplicability of Pauli's principle under the conditions considered.
Again, the teaching of all Founding Fathers of contemporary physics, which were seemingly independent and unsubstantiated in early 1978, turned out to be deeply related and fully substantiated, provided that one abandons the point-like abstractions of hadrons, and confronts their experimentally established, extended character.

Upon identification of this situation, a number of sequential studies was then implemented. Here I am forces to be sketcy, to avoid a prohibitive length.

In essence, I believe that a prerequisite for the proper formulation of the problem of structure of hadrons (let alone its consistent treatment) is the achievement of a mathematically and physically consistent notion of particle under joint electromagnetic and strong interactions. The physical consistency is inferred from the expected necessary condition of overlapping of the wave packet with that of other particles.

Once the problem of particles (or constituents) is approached from this profile, a rather fundamental differentiation with the conventional notion of particle under elm interactions (the only experimentally destablished today) emerges. Dirac's equation for the peripheral electron of the hydrogen atom characterizes a non-conservative system (or an open system), trivially, because the elm field is external (that is, Dirac's equation does not discribe the closed, two-body, system composed by the electron and the proton, but only the electron in the external field of the proton). The force in this case is variationally selfadjoint. Most crucial is the property that, in this case, the SU(2)-spin part of the Poincaré symmetry is an exact symmetry for the electron. This allows the identification and technical treatment of the fact that the electron, under these circumstances, is a fermion.

In the transition to a particle under elm and strong, with the condition of overlapping of the wave packets, the physical profile is profoundly altered. We still have the characterization of an open system , in exactly the same measure as that of Dirac's equation . But the condition of overlapping of the wave packets implies the additional presence of the broader non-self-adjoint forces. Studies have indicated that these forces imply the breaking of the SU(2)-spin symmetry. As a result, the conventional notion of intrinsic quantities, as currently known for the elem interactions, become inapplicable under the conditions considered. Instead, we see the direct applicability of the covering Lie-admissible algebras, and, in particular, of the covering SU(2)-admissible algebra. In turn, this provides the technical characterization of a covering notion of the intrinsic quantities which are expected under the conditions considered (which is feverishly under study).

All our studies possess a direct Newtonian counterpart under the correspondence principle. Thus, you can visualize this occurrence in the

physical spinning top, that is, not the conventionally treated top with an exact SU(2) symmetry and the consequential realization of the perpetual motion. No. Instead, the terms" physical spinning top" are referred to the system actually occurring in our environment, with drag torques responsible of the decay in time of the angular momentum. The breaking of the SU(2) symmetry is then necessary to comply with this Newtonian evidence. Upon Lie-admissible quantization, the situation persist conceptually, apart a number of technical implementations. What is important is that a covering SU(2) admissible treatment of the broken SU(2) symmetry exists at the Newtonian level of the physical spinning top, and persists in its entirety for the description of an extended hadron (say, a proton) within dense hadronic matter (say, the core of a neutron star).

In conclusion, we are feverishly working at a covering notion of particle under conditions of overlapping of the wave packets, and broader forces. This activates the applicability of the Lie-admissible formulations, thus allowing a technical treatment of the problem. The emerging notion is a covering of the conventional one because the Lie-admissible algebras are capable of reducing themselves to the conventional Lie algebras. This algebraic property is physically interpreted as the condition of exiting a hadronic medium, with the consequential null value of the strong forces. In other words, consider a scattering process of a hadron within hadrons e.g., a hadron "passing through" a nucleus . Then, three stages are identifiable according to this view: (A) the approching motion to the nucleus- the motion is in vacuum; no overlapping of the wave packets occurs; and conventional relativities and quantum mechanical laws of Lie algebraic character hold; (B) motion through the hadronic matter- state of overlapping of the wave packets; emergence of the nonselfadjoint forces and consequential treatment via the covering, Lie-admissible, relativities, and quantum mechanical laws; (C) exiting of the hadronic matter- return to the motion in vacuum without overlapping of the wave packets- the Lie-admissible algebras collapse into their Lie particularization and all conventional relativities and quantum mechanical laws are recovered, apart secondary processes (emission of neutrinos, and others).

This broader setting was then applied to the construction of the rudiments of a new model of structure of hadrons. The key idea is to achieve a model in which the constituents are physical particles. The covering Lie-admissible formulations essentially allow the possibility that the hadronic constituents are physical particles simply produced free in the spontaneous decays. They simply perform the transition from generalized forces, laws and formulations, to the conventional ones (apart, again, secondary effects). Notice that this simple view is strictly prohibited by conventional, Lie, relativities, and laws.

I have no words to stress the fact that these studies are at the very beginning and a truly long way remains to be covered. What we have done is merely identify the technical problems and conduct a preliminary study for their solution. One thing has transpared clearly: if one takes seriously the legacies by Fermi, Einstein, Jordan, Wigner,

von Neumann, Pauli and others, the possibility of genuine, non-incremental advancements of the foundations of our current theoretical knowledge is possible. This is the vision of the "scientific renaissance stimulated by the strong interactions" I dared to indicate in my paper "An intriguing legacy by Albert Einstein...".

The following words by Heisenberg here come to my mind:

> "In science, it is impossible to open up new terrotory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hazardous leap forward."

To which he adds soon thereafter

> " However, when it comes to new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do."

THE EXPERIMENTAL PROFILE. Again speaking on personal grounds, all these efforts are devoted to the experimental resolution of the validity (according to some) or the invalidity (according to others) of the conventional relativities and laws for the strong interactions, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and Heisenberg's indeterminacy principle .

I believe that the fundamental ethical rule of our profession is the separation of facts from beliefs, and the resolution via experiments of diverging theoretical views. When referring to fundamental issues, such as the validity or invalidity of basic laws for the strong interactions, the implementation of this ethical rule becomes mandatory.

In particular, we are living in a period with a cloudy economic future of our society. Basic, open, issues of this type have a subtle, but potentially crucial role for energy-related profiles. Despite opposing views by a number of colleagues, I see the controlled fusion as nothing more than the laboratory construction of a bound state of hadrons. The problem of the basic laws has indeed a crucial character. When considering the high temperatures and pressures of the controlled fusion, the condition of overlapping of the wave packets appears, to me, unavoidable. In conclusion, apart scientific profiles, I believe that we simply cannot affort the luxury of overlooking issues of this type.

If nothing else, I see the need of these experimental resolutions as a tribute to the Founding Fathers of contemporary physics, that is, as a concrete expression of the fact that their teaching of broader scientific visions has not been replaced by the easy acceptance of familiar doctrines.

The studies I have outlined here are all based on the conception of the invalidity of conventional settings for the strong interactions. I would like to stress that this is solely motivated by the intent of stimulating their experimental resolution. The rationale is quite simple. I believe that, until somebody indicates the plausibility of the violation,

the scientific community has full reason to continue its dormient attitude and the easy preservation of the status quo.

In conclusion, all efforts (at least mine) are solely intended for these experimental resolutions. We are currently working at a number of experimental tests and it is understood that much work remains to be done. The experimental proposals currently available are essentially the following.

- I have proposed (Hadronic J. 1, 574 (1978)) the experimental verification of Pauli's principle in nuclear physics to the effect of ascertaining whether the principle is valid in nuclear physics in the same quantitative amount as that in atomic physics, or small deviations exists, can be experimentally established, and have escaped inspections until now. The nulei recommended for this test are those whose volume is below the proportionality rule with the total number of nucleons. For these nuclei, nucleons must be in a statistically small state of penetration of their wave packets, thus activating the breaking of the SU(2)-spin symmetry in a very small measure, the emergence of forces non-derivable from a potential added to conventional nuclear forces with very small coefficients, and the Lie-admissible covering formulations of spin and of the time evolution law of the individual nucleons. The experiment is suggested via low energy scatterings of hadrons in the nuclei selected, and it is essentially aimed at the establishing of the totally antisymmetric character of the wave function either in an exact measure, or only as an approximation , owing to these conceivable departures. They, incidentaly, are quantitatively similar to the historical case of parity violation in the weak interactions. We simply have the transition from the breaking of the discrete part, to that of the space-time part of the Poincaré symmetry. According to the view of experimental nuclear physicists, the experiment is feasible with current technology.

- Kim (Hadronic J. 1, 1343 (1978)) has independently suggested more accurate measurements of the mean life of unstable hadrons in flight. If the structure forces are non-local and approximated via nonselfadjoint forces, deviations from the values predicted by the special relativity are expected. These deviations are significant (of the order of 12 % at certain energies). Thus they are detectable with current technology, according to the view of a number of physicists.

These two proposals, even though seemingly non-related, have in actuality exactly the same dynamical basis. In both cases the mechanics of breaking of conventional settings is due to broader forces. Thus, these two proposals simply deal with complementary views of the same physical arena.

A GUIDE TO THE LITERATURE ON LIE-ADMISSIBILITY. As indicated earlier, the literature on these studies is considerable and expanding rapidly. We have until now material for some nine volumes of research contributions (four printed, two in press and three forthcoming). I am fully aware that you do not have the time to inspect this material. I am therefore taking the liberty of suggesting a reading program, as an indicative guide, with

a progressive increase of time committment.

Suggested first reading. For a general outline of the quantitative aspects treated in this letter, I suggest the reading of "Chart 4.9" of my volume II with Springer-Verlag, which has been enclosed in the material mailed separately to you.

Suggested second readings. After the introductory first reading, the second depends on the line you desire to inspect in more detail.
- For the notion of "nonselfadjoint interactions" the reading of my two volumes with Springer may be advisable;
- for the classical universality of Lie-admissible algebras, I suggest the reading of my articles Hadronic J. 1 223 (1978), and 1, 1279 (1978).
- for the application to a new model of hadron structure, I suggest the reading of my article Hadronic J. 1, 574 (1978) and the paper by Jiang Chun-Xuan (also mailed to you separately).

Suggested third readings. The notion of hadronic constituent we are working on demands the knowledge of the totality of the studies, most importantly, of the mathematical contributions by mathematicians. My suggestions for an in depth study of this notion calls for the reading of the entire literature, that is, steps one and two, plus
- the outstanding articles by Okubo, Myung and others on Lie-admissible algebras (reprinted in the volumes mailed to you);
- the study of the mathematical and physical literature prior to 1978 (currently under reprint); and, most importantly,
- the study of the proceedings of the Second Workshop on Lie-admissibility (which is now in press).

In case I can be of any assistance, please do not hesitate to call me (Office no. (617) 495 3352; home no. (617) 969 3465).

Hoping that I did not abuse of your time and courtesy with this long (passionate) letter, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml

c.c. Professor WILLIAMS, VPI &SU

THIS LETTER WAS NEVER ACKNOWLEDGED BY MARSHAK.

COLLEGE OF ARTS AND SCIENCES

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

Blacksburg, Virginia 24061

DEPARTMENT OF PHYSICS (703) 961-6544/6545

May 5, 1980

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

At long last our search for a department head has ended, and it gives me great pleasure to announce that Dr. Alexander Abashian will be our new Head. Dr. Abashian is currently Program Director for Elementary Particle Physics at the N.S.F., and he will join us in September 1980.

The pool of applicants for our head position was one of exceptional quality by any standard, and you have certainly honored us by your interest and willingness to apply. The selection process unfortunately consumed a long span of time, about twice as long as we initially expected, and we both apologize for it and extend to you our appreciation for your patience in awaiting its development.

With best regards,

C. D. Williams
Chairman, Head Search Committee

CDW:hh

COLLEGE OF ARTS AND SCIENCES

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

Blacksburg, Virginia 24061

DEPARTMENT OF PHYSICS (703) 961-6544/6545

May 26, 1980

Dr. R. M. Santilli
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santili:

I am writing in response to your recent telephone call concerning the possibility of some kind of faculty position for you in the Physics Department at Virginia Tech. I am sorry to report that, after a fairly extensive assessment of the situation, I find that there appears to be no possibility of such a position for you here, at least in the near future, despite the likelihood of your salary being provided by your current grant. The problem is two-fold: there seems to be only tangential interest at most among our faculty concerning your indicated research interests, and faculty openings for the next several years here in physics are likely to occur only by retirement or other modes of displacement, if then (i.e. we are given to understand that our number of physics faculty positions will not only probably not grow but may in fact decrease in the 1980's).

In any case though we appreciate your interest and wish you all good fortune in your future associations.

Yours sincerely,

C D Williams

C. D. Williams
Associate Head

CDW:let

PART VII :

INSTITUTE FOR THEORETICAL PHYSICS, UNIVERSITY OF CALIFORNIA AT SANTA BARBARA

Institute for Theoretical Physics
University of California
Santa Barbara, CA 93106

July 27, 1979

Professor Ruggero Maria Santilli
Department of Mathematics
Harvard University
Cambridge, MA 02138

Dear Professor Santilli,

Your letter applying for a position at Santa Barbara associated with the Institute for Theoretical Physics has been received and given to the search committee of our advisory board for their consideration.

We appreciate your interest in the Institute for Theoretical Physics and in Santa Barbara.

Sincerely yours,

Douglas J. Scalapino
Acting Director

pm

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

August 14, 1979

Professor DOUGLAS J. SCALAPINO,
Acting Director,
Institute for Theoretical Physics
University of California
SANTA BARBARA, California 93106

Dear Professor Scalapino,

I would like to express my appreciation for your letter of July 27, indicating reception of my application to join your Institute.

We have just complited the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, held here at Harvard from August 1 to 9.

I believe that the search committee might be interested to know the outcome of this meeting. I am therefore taking the liberty of sending you copy of my informal report to the DEPARTMENT OF ENERGY. As you can see, it is informative, although it is written in a friendly, non-formal language.

I would appreciate whether the search committee can consider this disclosure as confidential.

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml
encl.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

July 21, 1979

Professor Walter Kohn, Director,
Institute for Theoretical Physics
University of California
Santa Barbara, California 93106

Dear Professor Kohn,

following your announcement in the July issue of PHYSICS TODAY, I am here applying for a permanent position at the NSF Institute of Theoretical Physics. I enclose my resume, as well as informative material on my recent research, teaching and editorial functions. I am currently the recipient of a grant from the Department of Energy that covers my full salary until June 1, 1980. I am available for relocation beginning from September 1979. Subject to approval by the Department of Energy, my possible appointment would require no salary during the first year. The following elements may have some value for the consideration of my candidacy.

Editorial. I am the founder and editor in chief of the HADRONIC JOURNAL, and I would like to continue this editorial function at my new Institution. Under the guidance of an Editorial Council of distinguished scolars, we are attempting a dialysis between mathematics and physics, with priority on the presentation of advanced mathematical tools of direct or potential relevance for hadron physics. Jointly, we are also attempting a contact with the engineering community, because methods conceived for the treatment of strong interactions as nonderivable from a potential have already seen their application in engineering problems (trajectory problems with drag forces, electric circuits with internal losses, etc.).

The Journal has completed Volume 1, 1978 for a total of 1,603 pages, and Volume 2, 1979 is at an advanced stage. The Journal is published bimonthly. We have a record of publishing each and every issue on schedule. Our Journal has also a good record of rapidity of publication. The current average between reception of an article and its distribution (including refereeing, printing, and mailing) is of 48 days. Finally, our journal is conceived to promote scientific values with the minimal possible burden to the authors. Indeed, we have no publication charges, no restriction on length, and no editorial restrictions on footnotes, figures, etc.

My personal attitude in the conduction of this Journal is to attempt the presentation of a well balanced research on hadrons, in which all valuable lines of study are equally considered, whether of quark or non-quark inspiration. To be specific, I respect studies on quarks; I favor their continuation; and I often invite papers on quarks. Nevertheless, I oppose the restriction of studies on the fundamental problem of hadron structure

along quark lines only. This editorial attitude appears recommendable because of the known, rather numerous, problematic aspects of quark models.

In case of my appointment at your Institute, you can count on my best possible efforts to review my editorial conduction of the HADRONIC JOURNAL in such a way to achieve compatibility with your Institute, if at all needed. In any case, we are continuously expanding our editorial organization, and we would be happy to add interested members of your Institute.

Conferences. I am the organizer of a yearly workshop on the so-called Lie-admissible formulations. The first meeting was held here at Harvard on August 1978, and the second meeting will be held here from August 1 through 4, 1979. In case of my appointment at your Institute, I would like to have the opportunity of continuing this workshop.

The primary lines of studies essentially consist of the attempts by mathematicians and physicists of generalizing Lie's theory via the covering Lie-admissible algebras. At the mathematical level we have identified an intriguing array of open mathematical problems ranging from the generalization of Cartan's classification, Lie's theorems, representation theory, to the treatment of the geometry underlying the covering Lie-admissible algebras (which is not of symplectic nor of Riemannian type). These problems are attracting an increasing number of mathematicians in the field. At the physical level, the analytic equations originally conceived by Hamilton, those with external terms, have resulted to possess a Lie-admissible (rather than Lie) structure. As a result, Lie-admissible algebras have resulted to be "universal" in Newtonian Mechanics, that is, capable of characterizing the brackets of the time evolution law under arbitrary forces (of class C^1). These forces generally imply the breaking of conventional symmetries. Thus, Lie-admissible algebras have emerged as valuable for the treatment of broken Lie symmetries, that is, via the replacement (technically realized via embedding at the level of the enveloping algebras) of the Lie algebra with the covering Lie-admissible algebra. The departure from the Lie product is then directly representative of the symmetry breaking forces. These ideas have been applied to a number of cases, such as the breaking of SU(3) under strong interactions and the construction of the Gell-Mann-Okubo mass formula, or the breaking of the SO(3) symmetry of the spinning top (to avoid perpetual-motion-type of abstractions) due to drag torques.

At the quantum mechanical level, the Lie-admissible formulations have identified an additional array of intriguing, open problems of both mathematical and physical nature. In essence, we have identified the existence of a mapping from functions to operators which preserves the Lie-admissible (rather than Lie) algebras. As a result, the Lie-admissible algebras appear to be intriguing for the problem of quantization of variationally nonselfadjoint forces (nonderivable from a potential). At the mathematical level, this has stimulated a number of issues, ranging from geometric quantization to spectral decomposition, to the use of nonunitary transformation, etc. At the physical level, this situation is stimulating a number

of intriguing problems, ranging from dissipative nuclear processes, to quark confinement, to the problem of the structure of the strong hadronic forces.

As you can see from the wording of the enclosed announcement, this workshop is inspired by the desire to avoid monopolistic conductions of research, by soliciting the partecipation of researchers of different beliefs or orientations. The workshop is also intended to attempt the conduction of research without prejudices as much as humanly possible. Finally, the workshop is strictly devoted to the study of fundamental, nonincremental, open problems (research of minute incremental character is gently referred to other meetings).

Research. I am involved in a long term research project concerning the experimental verification of the validity (according to some) or invalidity (according to others) of conventional physical laws for the strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

The issue appears to be related to the problem whether the strongly interacting particles can be consistently subjected to the conventional point-like abstractions, or a genuine representation of their actual, extended, size is needed. In turn, this is related to the issue whether the strong forces are of the simplistic structure of current use, $f = -\partial V/\partial r$ (variationally selfadjoint), or structurally more general forces (variationally nonselfadjoint) are needed. In the former case conventional laws and principle are expected to be valid. In the latter case, suitable coverings of these laws are expected to apply.

The search for achieving maturity of formulation of these problems is conducted via: (1) a series of monographs I am publishing with Springer-Varlag and the Hadronic Press; (2) research papers in collaboration with mathematicians and physicists; (3) the coordination of efforts by independent researchers via the HADRONIC JOURNAL; (4) contacts with qualified colleagues (experimentalists, in particular); and (5) the yearly workshop on Lie-admissibility. All valuable contributions are now reprinted in a yearly series of volumes by the Hadronic Press (the first two volumes have been printed in 1978, and we are now working on two additional volumes). Copies of the first two volumes are at your disposal.

This project has now completed the initial (predictably nebulous) orientational phase, and it is now entered into the second phase of technical treatments of specific issues by mathematicians and physicists. It is understood that a long way remains to be yet covered. Irrespective of the future outcome, permit me to confess that I am personally fascinated by the some many and so intriguing open problems which are created by the mere attempt to formulate the experimental verification of basic laws for the strong interactions. For instance, it has been for me rewarding to see that methods conceived for strong, variationally nonselfadjoint

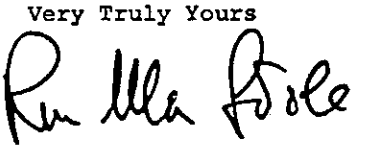
interactions have already seen their application to a number of problems in engineering (you may consult my monographs with Springer-Verlag in this respect, copies of which are at your disposal upon request).

I believe that the backing of your Institute to the pursue of this fundamental physical problem would be scientifically invaluable. It is in this spirit that I submit my candidacy.

A list of references is enclosed. Please feel free to contact directly the colleagues indicated, as well as, in addition, any member of the Editorial Organization of the HADRONIC JOURNAL.

In case you desire that I solicite letters of recommendation, please let me know. I remain at your disposal for any additional assistance you might desire.

Very Truly Yours



Ruggero Maria Santilli

RMS/ml
encls.

PART VIII:

UNIVERSITY

OF

CALIFORNIA

AT

BERKELEY

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

July 21, 1979

Professor GEOFFREY F. CHEW,
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Chew,

I am here applying for a faculty position at your Department commensurate to my qualifications. I enclose for your consideration my resume, as well as additional, informative, material on my recent teaching, research and editorial activities.

I am currently the recipient of a grant from the Department of Energy that covers my full salary until June 1, 1980. I would like to have the opportunity of applying for the renewal of this grant as principal investigator, either alone or jointly with interested colleagues. As you know, I am also the editor in chief of the HADRONIC JOURNAL, and I would like to continue this editorial function at my new Institution. Finally, I am the organizer of a yealy workshop on the studies by mathematicians and physicists of the Lie-admissible algebras, and I would like to have the opportunity of continuing this workshop.

As you eventually know, I am involved in a long range research project concerning the experimental verification of the validity (according to some) or the invalidity (according to others) of conventional physical laws for the strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

The search for achieving maturity of formulation of these problems is conducted via (1) a series of monographs I am publishing with Springer-Verlag and the Hadronic Press; (2) research papers in collaboration with mathematicians and physicists; (3) the coordination of the efforts by independent researchers via the Hadronic Journal; (4) contacts and correspondence with qualified colleagues interested, but not yet formally committed to the problems; and (5) the yearly workshop on Lie-admissibility. All valuable contributions on the problems considered are now reprinted in yearly volumes published by the Hadronic Press (the first two volumes appeared in 1978 and we are now preparing two additional volumes).

The ultimate issue appears to be related to the problem whether strongly interacting particles can be consistently subjected to point-like abtractions, or representations of their extended size are necessary. In turn, this appears to be related to the problem whether the strong interactions can be well approximated with the current simplistic forces $f = -\partial V/\partial r$,

or structurally more general forces are needed. A rather feverish research activity is now going on on these issues, and ranging from the methods for the treatment of local nonselfadjoint forces (i.e., nonderivable from a potential), to conceivable coverings of the relativity and quantum mechanical laws of the electromagnetic interactions and related point-like approximations. In particular, a new line of research has been lately initiated. It consists of the identification of the scattering theory and related tools of experimental relevance for forces more general than $f = - \partial V / \partial r$. It is understood that these studies are at the very beginning, and a long way remains to be yet covered.

I am interested in joining your Department because of its close connection with the Lawrence Berkeley Laboratory. In essence, the initial, predictably nebolous phase of identification of the existence of the problem appears now to be completed, and we have entered into the second phase of technical treatments of the individual issues by mathematicians and physicists. In order to effectively continue my studies, both as a researcher and as an editor, I need an exposure also to the experimental community. Such an exposure appears to be essential to focus the research into a form suitable for the future, actual conduction of experiments.

Permit me the liberty of touching the delicate issue of quarks vis-a-vis with these studies. You might be aware that I have launched a moment of reflection on quarks, via recent articles and forthcoming papers, when interpreted as actual structure models (and not when used for Mendeleev-type classifications). I would like to stress here that these initiatives are solely motivated in the hope of performing a scientific function. I respect studies on quarks; I favor their continuation; I routinely accept them for publication as an editor; and I often invite papers on quarks. Nevertheless I oppose the restriction of research on the fundamental problem of hadron structure to quark lines only. Instead, I favor the conduction of research on the sector in which all valuable lines are pursued, whether of quarks or non-quark inspiration. I believe that this more balanced conduction of research is recommendable, owing to the complexity of the problems to be yet solved.

The point I would like to indicate is that, in case I join your Department, my studies should not be interpreted as opposing quark oriented research. On the contrary, they should be interpreted as complementing such study. In the final analysis, my primary objectives are the future experimental resolutions of the basic laws for the strong interactions. I am confident that all colleagues at Berkeley, whether of quark or non-quark orientation, will favor these experimental resolutions.

In closing, you might be interested to know that I am a nontenured member of the Department of Mathematics here at Harvard. Nevertheless, I have now completed my desired exposure to advanced mathematics. Also, I am a theoretical physicist, and I am now seeking a faculty appointment in a Department of Physics close to experimental facilities.

page 3.

I have enclosed a list of references in case you desire to contact them directly. On my part, I would appreciate the courtesy of an indication whether my candidacy is actively considered in order to solicite letters of recommendation.

Thanking you for your courtesy and time, I remain

Yours, Sincerely

Ruggero Maria Santilli

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DEPARTMENT OF PHYSICS

BERKELEY, CALIFORNIA 94720

July 31, 1979

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli,

Professor Chew is away from Berkeley until September, as is our current Chairman, Professor Jackson, so I am acknowledging your application for a faculty position in our department. Assuming a position is made available to the department, the process of assessment and selection requires 6 to 8 months.

Thank you for your interest in Berkeley.

Sincerely yours,

Howard A. Shugart

Howard A. Shugart
Acting Chairman

HAS:fs

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

September 18, 1979

Professor G. F. CHEW
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Chew,

As you are eventually aware, I have applied for a position at your department with a letter originally addressed to you of July 21, 1979, and later kindly acknowledged by Dr. Shugart as acting chairman.

I would like to take the liberty of indicating that the Department of Energy is sincerely interested in continuing my grant, including the complete support of my salary, for a limited number of years. The officer in charge of my case is Dr. DAVID C. PEASLEE at the DOE, Tel. 301 353 3624. Please feel free to contact him if you so desire.

I am seeking a three year nontenured appointment with a minimum of stability during this period to justify the relocation of my family, as well as to have a minimum peace of mind to work (I have been living since my arrival in the States in 1967 with the known "one-year-terminal-nontenured-contract" and, with the passing of the years, it is beginning to take its toll).

I am also interested in the possible consideration for tenure at the end of this three year period, or a longer period if necessary. This possibility is quite important for my relocation.

I have taken the liberty of applying to you because you have been indicated to me by a number of colleagues as a scientist with a genuine vision toward fundamental studies (which is a quite rare occurrence in our community). I believe that my type of studies call for such a vision to remain alive.

You might be interested to know that we have achieved considerable progress since the time of my letter of July 21. Hoping that I do not abuse of your time and courtesy, permit me the liberty of outlining them.

Informal statement of the problem . As you eventually know, my group, by now composed of a number of mathematicians and physicists from the USA and adroad (France, Switzerland, Grece, USSR and People's Republic of China-see the enclosed paper for this latter Country) is working on the problem of validity or invalidity of conventional laws for the strong

interactions, with particular reference to Einstein's special relativity, Pauli's exclusion principle and other conventional quantum mechanical laws. Our primary objective is to reach maturity of formulation of experiments. Our belief is that fundamental issues of this type simply cannot remain to be lingering in our community for ever, and should be resolved either way via experiments.

Historical doubts. As you certainly know, we are not the first to have doubts on the validity of conventional laws for the strong interactions and the hadronic structure in particular.

Most famous are Einstein's doubts on the final character of the Undeterminacy principle and other quantum mechanical laws (so vividly depicted in Heisenberg's memoires).

Equally valuable is Pauli's teaching that His principle was conceived and should be considered as applicable under the lack of overlap of the wave packets (atomic structure). Simple calculations show that, as a necessary condition, the hadronic constituents must be in a state of overlap of their wave packets. At the extreme, the current easy application of Pauli's principle, the spin-statistics theorem and all that, by hadron physicists, without a genuine critical inspection, could be qualified as being contrary to Pauli's teaching.

Also known are Fermi's reservations on the validity of conventional geometries and relativities within the region of space occupied by a strongly interacting particle (which he explicitly expressed in his lectures in Nuclear Physics). To my reconstruction, Fermi's argument is that Einstein's special relativity is strictly conceived for point-like particles under the action of forces derivable from a potential. This must be the case because this relativity is simply a relativistic extension of Galilei's relativity. In turn, this latter relativity is crucially dependent on the point-like notion of particle by Newton and Galilei (think at the point approximation of the sun). The point is that when the strong interactions are concerned, the Point-like approximation of hadrons is directly in conflict with the necessary need of overlap of the charge volumes-wave packets (as you know, the "size" of all hadrons coincides with the "range" of the strong interactions). Fermi therefore argued, to my reconstruction, that these conditions of overlapping are bound to result in forces more general than those of the still current simplistic use, $f = -\partial V / \partial r$, resulting in the expected breaking of conventional geometries (and, thus, relativities). The current easy use of the special relativity by hadron physicists without a genuine critical inspection, can be conceived, in the final analysis, as contrary to the teaching of these Masters.

Jordan's arguments on the need to generalize conventional quantum mechanic are equally known, and they lead to the foundations of the Jordan algebras even though their physical applications have not been as expected.

The list of historical reasons of doubts by authoritative physicists could continue, but it would be here inessential. You are certainly aware of them.

The contributions in the Hadronic Journal. When I organized the Journal in April 1978 this was essentially the situation. There were many authoritative doubts. Nevertheless, we lacked the quantitative study of the problem and, most important, we lacked the initiation of the study to formulate experimental tests.

I believe that a considerable progress has been achieved in the Hadronic Journal in this crucial issue. We are now working at the fourth volume of reprinting of articles in the series devoted to this problem "Applications of Lie-admissible algebras in Physics", Edited by Professors H.C.MYUNG, S. OKUBO and myself. It is therefore impossible for me to be technical in a letter (or even in one single seminar).

A conceptual and purely indicative outline of the thought is the following.

(1) We have confronted the problem of the conditions of overlapping of the charge volume (or wave packets, if you prefer) of particles, as necessary to activate the strong interactions. This has resulted in the identification of nonlocal forced nonderivable from a potential (integro-differential equations).

(2) We have identified the implications of these broader forces vis a vis with current theoretical knowledge. Whether believed or not by the orthodoxy, these forces imply the collapse of most of current knowledge. There is the impossibility of introducing all Lie algebras via the brackets of the time evolution law, let alone the SU(2)-spin algebra, the SL(2.c) algebra, the Poincaré algebra, the conformal algebra, the gauge algebra, etc. More deeply, there is the breaking, under the forces considered, of the theoretical tools for the definition of the intrinsic quantities of particles, such as the conventional notion of spin under electromagnetic interactions. The breaking of the SU(2)-spin is much along the breaking of the SO(3) symmetry of the spinning top in Newtonian mechanics as necessary to avoid the perpetual motion. Even though rejected by the orthodoxy at this time (to the risk, quite candidly, of remaining behind), this occurrence is expected on physical grounds. Think at a proton created in the core of a star. We have an extended objected within superdense hadronic matter. Clearly, the physical conditions are different than those of the same proton when it is the nucleus of an hydrogen atom. In particular, such a proton is not expected to "spin" as when in vacuum. Interferences of the extended character of the proton with the hadronic matter are expected to result in a different notion of spin (which we technically realize via the Lie-admissible mutation). This is, conceptually, much along the breaking of SO(3) for the spinning top within Earth's atmosphere. At the hadronic level the situation is similar, as far as the constituents are concerned (also in a state of overlapping of respective wave packets), even though the hadronic densities are smaller. Thus, we expect a weaker

form of SU(2)-spin symmetry breaking for each individual hadronic constituents. At the nuclear level the situation is also similar, although the conditions of overlapping of nucleons is statistically quite small. We then expect an infinitesimal-type of SU(2)-symmetry breaking for the nuclear structure (see below for the experimental profile). With respect to quarks, the question of fractional charges is purely secondary. More deeply, the doubts rest on the conventional notion of spin in the quark model which has been conceived for the electromagnetic interactions and too easily extended to the strong without a critical inspection. In different terms, a fundamental question prior to any meaningful treatment of the hadronic structure is: what is the quantitative formulation of the notion of particle under strong interactions, that is, under the condition of overlapping of the wave packets?

(3) To confront this latter, crucial, issue, we have started a laborious homework beginning necessarily at the Newtonian level. I enclose copy of my Vol. I with Springer-Verlag on "Foundations of Theoretical Mechanics". Vol. II is now in press and copy of the typescript is at your disposal on request. As you can see, these volumes are devoted to the identification of methods for the treatment of forces nonderivable from a potential, as an approximation of nonlocal forces which is customary in Newtonian Mechanics. Think at the motion of Skylab while falling on Earth. Its forces were polynomial expansions in the velocities. Its dynamics was in rather brutal violation of Galilei's and Einstein's relativies because not only the equations of motion are necessarily form noninvariant under conventional space-time symmetries, but the conservation laws are lost. Thus, Skylab was a rather visible proof of the limited validity in physics of conventional relativities. If you go deeper into this situation you can identify the collapse of Galilei's and Einstein's relativity for Skylab as due to the extended size of the object, while these relativities are crucially dependent on point-like abstractions. So, you are back at Fermi's supposed argument.... Even more, Skylab's equations, as well as Newtonian Mechanics in general, are in direct incompatibility with Riemannian geometry, as stressed by Cartan (who advocated for Newtonian Mechanics the affine geometry), trivially, because a necessary condition for physical consistency is that the equations of motion are not derivable from an action principle and, thus, they cannot be all "geometrized". Thus, Skylab was also, at least for me, a clear example of the insufficiency of conventional gravitational models for the interior problem (which are all derivable from an action...). You are then back again to the formally written doubts by Fermi on the validity of conventional geometries within hadronic matter....

(4) The studies of my monographs with Springer-Verlag is only a rather small part of our efforts. A complementary line of studies has been initiated via the Lie-admissible approach to classical mechanics. You should have received complementary copies of the first two volumes of reprints "Applications of Lie-admissible algebras in Physics" I mailed you some time ago. Perhaps, you are familiar with the fact that the

Lie-admissible algebras are a covering of the Lie algebras, and that they emerge for the time evolution law of Hamilton's equations in their original form with external terms (and not the "truncated" form of current use). This line of studies is now in full development. We have published numerous articles by several independent authors along the classical applications of Lie-admissible algebras, and a rather regular flow of papers arrives at my editorial desk.

(5) Finally, we have initiated a vigorous effort aiming at the quantization of local forces nonderivable from a potential, that is, strong interactions in our views, again, as an approximation of nonlocal settings. A crucial point to avoid scientific "hand wavings"(that is, illusions of treating quantum mechanically genuine forces nonderivable from a potential) is that the formulations should verify uniquely a correspondence limit into a bona fide nonconservative Newtonian system. This condition eliminated most of the current efforts in dissipative nuclear physics, by and large, via additive terms in Schrödinger's equations. These terms result in additive terms of the Hamilonian of the Hamilton-Jacobi equation under the correspondence principle. Thus, the forces are derivable from a potential and the system is far from being dissipative.

This line of studies is also in full motion and it is difficult for me to technically outline it here. In essence we have moved along two lines.

- Quantization via generalized Schrödinger's equations. The use of the techniques of the Inverse Problem (my monographs with Springer) allow the computation of generalized Hamiltonian structures. Quantization via the second Beltrami procedure then yields generalized Schrödinger's equations which uniquely satisfy the correspondence principle into the original, genuine, nonconservative equations. The implications for conventional knowledge are conspicuous, when forces nonderivable from a potential are genuinely confronted. For instance, the phase of wave packets must necessarily violate conventional space-time symmetries in order to admit a Schrödinger's equation which, under the correspondence principle, yields genuine Newton's equations with forces nonderivable from a potential. This occurrence is desired to achieve compatibility with classical formulations. After all, at the Newtonian level the equations of motion must necessarily violate Galilei's symmetry in order to admit the forces considered (e.g., polynomial expansions in the velocities). What I am attempting to say here is that the very notion of "wave packet" needs a renspection when considered within hadronic matter. You are perhaps familiar with other breakdowns presented in my papers (i.e., the breakdown of all the technical ingrediants to formulate Pauli's principle, in full agreement, I would like to add, with His teaching).
- Quantization via generalized Heisenberg's equations. We start with Hamilton equations with external term, thus admitting a Lie-admissible (and not Lie) structure. We have identified a mapping to operators which preserves this Lie-admissible structure. This, again, ensures us that, under the correspondence limit, we recover Newton's equations with genuine forces nonderivable from a potential.

In particular, the quantum mechanical time evolution is necessarily nonunitary (Prigogine essentially received his Nobel price in chemistry much along this point). You can easily see it by noting that the time evolution of Hamilton's equation with external terms is necessarily noncanonical. We have then identified covering transformations which we call unitary-admissible and they yield precisely a Lie-admissible algebra for the time evolution law.

By using these two, new, generalized tools, a rather feverish activity is now going on. The ultimate objective is that of achieving a quantitative formulations of the notion of a massive and charged particle under the condition of overlapping of its (Lie-admissible) "wave packet" with hadronic matter (our idea of hadronic constituent).

My personal efforts have been verted in the identification of a possible generalization of Galilei's relativity at both the classical and quantum mechanical level with a Lie-admissible structure. I then use this generalized context to attempt the definition of covering of conventional notions of intrinsic quantities of particles.

More recent developments. Since the time of my letter to you, a genuine progress has been made via the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS held here at Harvard from August 1 to 7. Half of the participants were outstanding mathematicians in nonassociative algebras in general, and Lie-admissible algebras in particular. Half of the participants were qualified physicists (e.g., of the caliber of OKUBO).

In this workshop we first reinspected, particularly in the several days of informal sessions following the formal ones, most of the doubts on the validity of conventional laws for the strong interactions. I am happy to report that these doubts were not only confirmed, but actually enhanced.

In addition, we made genuine progress toward these studies. I am taking the liberty of including, on an informal and confidential basis, copy of my report to the DOE on the outcome of the meeting.

The experimental profile. At this moment we possess two proposals of experimental tests in the Hadronic Journal, although they are at a predictably initial stage, and much remains to be done.

The first is my proposal to test Pauli's principle in nuclear physics for nuclei with volume below that predicted by the proportionality rule with the number of nucleons, and via low energy scattering of hadrons (no leptons!) on nuclei. The prediction of the Lie-admissible formulations is that, under a small state of overlapping of nucleons, there are very small deviations from Pauli's principle which have escaped current inspections, much along the historical (and quantitatively similar) case of the violation of discrete symmetries in particle physics.

These small deviations are predicted from small nonlocal terms in the nuclear force originating from the small state of overlapping of nucleons. Indeed, according to our formulations, these forces imply that nucleons are not exact fermions, thus rendering Pauli's principle statistically inapplicable on a small scale (this is the very small breaking of the SU(2)-spin symmetry indicated earlier).

The second proposal is that by KIM and consists of an experimental measurement of the mean life of unstable hadrons in flight. If the constituents are under nonlocal forces, there are deviations from the value predicted by the special theory. In turn, this test can be conceived as an experimental verification of Einstein's special relativity for the strong interactions.

Most importantly from an experimental profile, we are working at a "nonpotential scattering theory", that is, at the identification of the basic tools for data elaboration (cross section, etc.) for forces non-derivable from a potential. Once we have achieved this objective at least in a preliminary, but solid form, we can formulate a variety of experiments for each and every major aspect. Then, the confrontation of the historical reasons of doubts on conventional laws for the strong interactions will not be procastinated any longer by the orthodoxy in physics, at least this is my hope....

As you can see, the continuation of this line of study demands a suitable Institution and a suitable, stimulating,environment, close to both the mathematical and the experimental circles. Lacking these qualifications, the entire program will be likely delayed years, perhaps decades in time. My application for a position at your Department has been submitted in this spirit.

Hoping that I did not abuse of your courtesy and time, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Professor G.F.CHEW
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Chew,

Hoping that I do not abuse of your courtesy and time, I would like to inform you that I have applied to the Lwarence Berkeley Laboratory, the nuclear physics division, as per enclosed letter. The reason for selecting the nuclear division, after due consideration, is that I am primarily interested in collaborating with experimentalists for the future experimental resolution of Pauli's principle, Heisenberg's principle, and other laws under strong interactions beginning at the nuclear level. In particular, it is at this level where we are closer to maturity, and the experiments are less constly, on a comparative basis with high energy physics.

I also enclose copy of the "Chart 4.9" of my volume II with Springer-Verlag of "Foundations of Theoretical Mechanics", now in press. This chart presents an account, understandable to graduate students, of the intriguing situation of the basic laws for the strong interactions.

I am fully aware that it will be difficult for you to find the time to read it. Nevertheless, you might pass it to graduate students. It is understood that the technical treatment is conducted elsewhere, and it is re-elaborated in the Proceedings of the Second Workshop on Lie-admissible Formulations which will be distributed in Early 1980.

Tnak you for your time.

Sincerely

Ruggero Maria Santilli

RMS/ml
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DEPARTMENT OF PHYSICS

BERKELEY, CALIFORNIA 94720

Nov. 5th 1979

Dr. R.M. Santilli
Science Center, Room 331
Harvard University
Cambridge MA

Dear Dr. Santilli:

In reply to your letter of October 10th. I heard repeatedly Fermi expressing doubts about the Copenhagen interpretation of Quantum Mechanics, but he never was very explicit. He rather smiled and joked. My impression is that he had feelings, but not cogent arguments.

I do not know enough to read your mathematical papers. I would like however to remark that I thought that practically all natural forces ON A MICROSCOPIC SCALE are conservative and that non conservative forces are a suitable schematization on a macroscopic scale of the total effect of the microscopic forces. The bridge is furnished by statistical mechanics. Maybe I am wrong, but I do not know of any not conservative forse on a microscopic scale.

Sincerely yours

Emilio Segre'

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 9, 1979

Professor EMILIO SEGRE
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Segre,

Please accept the sentiments of my sincere appreciation for your kind letter of November 5.

I feel a sense of relief in reading that Fermi had not detailed arguments for his doubts, because I had spent considerable time in searching through his papers and books, yet I was afraid that some important passage to this effect had escaped my attention. Nevertheless, Fermi's doubts, even though only embrionically expressed, have been invaluable for me.

I also appreciate the courtesy of expressing your view on the problem of the forces in the microscopic scale. There is little doubt that your view is the soundest at this moment, on grounds on established theoretical and experimental knowledge.

Nevertheless you might be intrigued to know that a rather feverish research activity is now going on, by mathematicians and physicists, on the study of forces nonderivable from a potential in the microscopic scale. I am personally involved in these studies, and, as editor in chief of the HADRONIC JOURNAL, I am particularly exposed to this recent trend with articles submitted from virtually all developed Countries.

To my reconstruction, this trend was initiated by nuclear physicists. After working with $f = -\partial V/\partial r$ for over half a century, nuclear physicists came to realize the existence of phenomena that simply cannot be effectively treated with these forces. Nowaday, the study of dissipative nuclear phenomena is under a considerable expansion, even though these studies cannot yet be classified as belonging to a discipline, because the problem of the basic quantum mechanical laws and principles under forces structurally more general than $f = -\partial V/\partial r$ is theoretically and experimentally open at this time.

Once the "ice was broken", so to say, in nuclear physics, the transition to hadron physics and astrophysics, again at the microscopic (or local) scale, was expected. Judjing from the flow of papers arriving at my desk, it appears that the trend is that of studying the possible existence of

forces structurally more general than $f = -\partial V/\partial r$ (technically called variationally nonselfadjoint forces) at all microscopic levels of the strong interactions (nuclear, hadronic and astrophysical), and then searching for direct compatibility with corresponding macroscopic settings. On more specific grounds, the trend is that of studying the forces considered in the miscroscopic scale, by following exactly the same pattern of the classical, mascroscopic transition from Newtonian mechanics to statistical mechanics, that is, the broader forces considered are studied first for the behaviour of one particle (under condition of overlapping of its wave packet with that of others), and then searching for a statistically compatible, quantum mechanical formulation, say, à la Prigogine.

If you accept the following data:
(1) all (massive) particles have a wave packet which is extended in size;
(2) all strongly interacting particles have approximately the same size which is of the order of 1F; and
(3) the dimension of a strongly interacting particle coincides with the range of the strong interactions;
a rather intriguing dynamical difference between the electromagnetic and the strong interactions emerge: the former generally occurs without appreciable overlapping of the wave packets (atomic structure-Copenhagen interpretation of quantum mechanics); while the latter demand, as a necessar condition to activate the strong interactions, a state of overlapping of the wave packets - or charge volumes - (new mechanics?).

The trend I am referring to essentially studies the question whether, under the conditions of overlapping of the wave packets, the forces $f = -\partial V/\partial r$ are truly capable of representing the dynamical behaviour or not.

Once this issue is subjected to a detailed, quantitative, treatment, a number of intriguing aspects emerge, all suggesting the presence of forces more general than those of conventional use. The literature in the subject is now quite vast, and expanding rapidly (Professors H.C.MYUNG, S. OKUBO and myself are editing a reprint series in the topic which is now approaching its fourth volume...). As an isolated case, I can mention the expectation that, under the conditions of mutual penetration, the spin-spin couplings are expectedly stronger than those of the atomic mechanics, that is, attainable via $f = -\partial V/\partial r$. If more general forces are admitted, an arbitrary strength of these spin-spin coulings becomes quantitatively possible.

On epistemological grounds, the following possibility is emerging. If all particles are approximated as being point like, then only forces derivable from a potential are possible, classically and quantum mechanically, that is, mascroscopically and microscopically. Indeed, a point can only have action at a distance forces, and these forces can be proved to be variationally selfadjoint (derivable from a potential).

In fact, if you approximate a satellite in earth atmosphere as being a point, it can only have selfadjoint forces and no inelastic collisions. According to established knowledge in space mechanics, a satellite has nonconservative forces while within earth atmosphere because it is an extended object. A technical study of the issue (I have also conducted in my monograph "Foundations of Theoretical Mechanics" with Springer-Verlag) indicates that the actual forces acting on the satellite are nonlocal (volume integrals), and that they can be well approximated as being local via polynomial expansions in the velocity (and thus, highly non-derivable from a potential).

The important fact is that, whenever the satellite exits the earth atmosphere, the actual shape and extension of the satellite do not affect the dynamics. Under these conditions, the satellite can be well approximated as being a point, and all forces are only derivable from a potential. This is the notion that the sun can be approximated as a massive point by Newton and Galilei.

The intriguing interplay electromagnetic/strong interactions seems to indicate the possibility that we may well have exactly the same situation at the miscoscopic level, of course, on conceptual grounds. Under elm interactions at large mutual distances the actual dimension of the wave packet does not affect the dynamics, and all forces are derivable from a potential (satellite moving in vacuum). Under strong interactions, and the apparent necessary condition of overlapping of the wave packets, the actual dimension of the particle seems to be needed to identify the dynamics, and, in any case, point-like abstractions are known to produce only a crude approximation (satellite in earth atmosphere). The broader forces nonderivable from a potential appears to be effective to represent, of course as an approximation of extected nonlocal settings, the dynamical implications of the extended size, e.g., the condition of one extended particle while spinning within another.

This is the reason why the current trend is studying the possible existence of variationally nonselfadjoint forces at the level of one single particle, and then searching for a compatible statistical setting, in such a way that:

(A) the one-particle quantum mechanical description recovers, under the correspondence principle, the one-particle Newtonian description according to Lagrange's and Hamilton's equations in their original conception, that with external terms (rather than the truncated form of these equations of current use); and

(B) the many-particles, quantum mechanical, statistical description recovers under the correspondence principle, the classical statistical description according to the original Liouville's conception, that for arbitrary forces (rather than the truncated form of this conception of current use, that for forces derivable from a potential only).

Independently from these considerations, the studies have indicated that, if the strong interactions are realized via forces that are analytically equivalent to the electromagnetic interactions, this condition necessarily

implies a form of dynamical equivalence between these interactions, which appears to be contrary to the profound physical differences between these interactions as manifested in nature. To put it explicitly, the positronium admits a physically effective, nonrelativistic, quantum mechanical description with $f = -\partial V/\partial r$. If the π^0 is represented as a bound state of two charged particles under forces $f = -\partial V'/\partial r$, the mere change of the potential $V \to V'$ in the transition from the positronium to the π^0 does not appear to be effective to properly represent the differences between these two structure. Besides the use of $f = -\partial V'/\partial r$ implies a host of problematic aspects for the π^0. As an indication, such a (nonrelativistic) model of the π^0 would imply, within quark models, a nonnull probability of tunnel effects of the constituents (which is rather well established recently), and, thus, the existence of the decay

$$\pi^0 \to q + \bar{q}$$

which is contrary to experimental evidence.

On the contrary, if one sees the possibility that, unlike the case of the positronium, the π^0 is made of a bound state of two particles under conditions of mutual overlapping of the wave packets, broader forces, and a consequential broder dynamics, a number of options become possible that would be otherwise precluded.

This is the reason why there is such a feverish activity going on. The use of nonconservative forces in the microscopic scale is stimulated by rather specific, technical, elements, and allows quite intriguing possibilities.

Of course, this crucial alternative ($f = -\partial V/\partial r$ or not) for the strong interactions must be resolved in due time via experiments.

You might be interested to know that I have recently applied to the nuclear physics division of the Lawrence Berkeley Laboratory, according to the enclosed letters to Drs. B. G. HARVEY and R. W. BIRGE. I have also applied some time ago to Dr. G. F. CHEW for a position (or a joint position) at your department, and I am taking the liberty of enclosing copy of my letter in a confidential way.

As you can see, it appears that there is a realistic possibility of resolving the issue of the existence or nonexistence of nonselfadjoint forces in the miscoscopic, one-particle level under strong interaction by conducting more accurate measurements of something taken for granted: the validity of Pauli's exclusion principle in nuclear physics.

Hoping that I did not abuse of your time and courtesy, I remains

Yours, Sincerely

Ruggero Maria Santilli

encls.
RMS/ml

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 9, 1980

Professor E. Segre
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Segre,

I enclose an outline of the Proceedings of our Workshop on the problem of the basic laws of the strong interactions. As you can see, it consists of a considerable effort for over 1,500 pages, by distinguished mathematicians and physicists from the USA, USSR, China, and several other Countries. Part A (the review part) review some 500 theorems, lemmas, etc. of either direct or indirect or potential relevance to the problem of the strong interactions conceived as extended particles under conditions of overlapping of their wave packets. Particularly intriguing are two theorems, one lemma and several propositions invalidating Heisenberg's equations for all Hamiltonians of polynomial order in r and p higher than two (that is, for nonlinear systems). One theorem was proved by Abraham and Marsden, and the other by several physicists. These theorems bring , rather forcefully in my view, into focus the need to subject to an experimental verification the validity of conventional laws for the strong interactions, no matter how sound they may appear at the theoretical level.

The Proceedings focus on the need to subject to an experimental verification Pauli's principle in nuclear physics, as well as on the quantitative study of possible deviations. It essentially emerges that possible very small deviations at the nuclear level open truly intriguing possibilities at the hadronic and the astrophysical level. The possible breakdown of conventional laws for the strong interactions, rather than being a scientific disaster, appears to be truly intriguing for nonincremental advances.

You might also be interested to know that a first contact with experiments is not far in the future In fact, we have now rather accurate experiments being done using a beam of neutrons through selected cristals. The statistics of the neutron (and, thus, Pauli's principle) can be tested via comparison of data under two rotations. Current data, apparently, are affected by an error of 1/2 degree over two rotations. This is certainly not necessarily a deviation from Pauli's principle (although it is quantitatively of the order of that predicted). The point is that, perhaps, the accuracy of the experiments can be estanlished or improved via separate experiments. In this case, we would be finally in a position to asnwer at least the question: what is the quantitative meaning of the currently accepted validity of Pauli's principle in nuclear physics on a comparative basis with the exact validity experimentally established in atomic physics ? If a deviation could be experimentally detected, the momentum for nonincremental advances would be invaluable, in my view.

In case you are interested to be kept informed of these studies, please let le know. Also, please let me know whether you desire a complimentary copy of the two volumes of the Proceedings, and I shall do my best to secure it from the publisher.

I would appreciate the courtesy of your assistance, if at all possible, on my application to join your Department. In essence, I would appreciate the courtesy of the indication of the status of the application, as well as of the projected time for a decision.

page 2.

I do not know whether you are aware of my application. The major lines are essentially the following.

I applied for a position with a letter to Professor Chew of July 21, 1979. I selected Professor Chew because he was somewhat informed of the progress of our studies. The application was passed to Professor Shugart as acting chairman who kindly acknowledged the application on July 31, 1979 and indicated that it was under consideration.

On financial grounds, I do not know whether I have sufficiently conveyed the fact that my possible position at your department does not require funds. In fact, I am fully covered by my grant from the Department of Energy. This grant is at its second year, and it will be likely continued for a number of years. The renewal for two additional years has been confirmed. Please feel free to contact Professor D.C.Peaslee at the DOE, tel 301 353 3624 for confirmation on the availability of the renewal.

In short, I am primarily interested in your Department administering my grant. The funds are confirmed. I am interested in applying as principal investigator jointly with any interested colleague. Nevertheless, in case a senior colleague is interested, I would be happy to apply as coinvestigator.

I understand that several letters of recommendation have arrived and my file is complete.

I would appreciate the indication of the status of the application. Incidentally, I am not interested in suggesting a rapid decision on my case. No. I believe that your Department should take all the necessary time to reach a decision. In fact, any delay will be in my favor, in the sense that the studies we are doing are attracting an increasing interest.

Hoping that I did not abuse of your courtesy and time, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

UNIVERSITY OF CALIFORNIA, BERKELEY

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DEPARTMENT OF PHYSICS BERKELEY, CALIFORNIA 94720

January 23, 1980

Dr. Ruggero Maria Santilli
Harvard University
Department of Mathematics
Cambridge, MA 02138

Dear Dr. Santilli,

Professor Segrè has passed on your recent letter concerning the status of your application for a faculty position at Berkeley. As Professor Shugart wrote to you at the end of July last year, your application had been handed on by Professor Chew to Shugart in my absence and from Shugart's hands to the appropriate faculty search subcommittee.

We have an ongoing search for outstanding young faculty. This search has many components and covers essentially all fields of physics. After a period of advertising, the various subcommittees sift through the applications and select one or more outstanding candidates for a faculty position in their area. The subcommittees submit reports to the departmental Committee on Policy and New Appointments, which folds in considerations of the Department's needs and the relative merits of different candidates from the different fields. Recommendations from this committee and from the search subcommittees then are taken to the full faculty for its consideration.

In this winnowing process one candidate will be approved to be offered the single position which we are allocated for July 1, 1980. As you can imagine, all of this takes a considerable time. I am hopeful that all decisions for the coming year will have been taken by April 15th at the latest.

I note that you have a DOE grant for the present year and anticipate its renewal. This does not, however, affect the considerations of you as a candidate for a faculty position.

Thank you for your interest in Berkeley.

Yours sincerely,

J. D. Jackson
Chairman

JDJ:fs

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 29, 1980

Professor J. D. JACKSON,
Chairman
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Jackson,

Please accept the sentiments of my sincere appreciation for the courtesy of your letter of January 23, 1980.

Please take all the necessary time to reach a decision on my case. Upon consultation with the DOE, I have filed an application for continuation of my grant with a local administration, here in the Boston Area. Jointly, I am taking all the necessary precautions to ensure that I can move the grant to a different Institution, once supported.

In case I can be of any assistance during the consideration of my candidacy, please do not hesitate to contact me.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml

P.S. Independently of my application, you might be intrigued by the enclosed letter I am passing to editors and selected colleagues. It may give you more information on the intriguing (for some) or distressing (for others) state of hadron physics at the editorial level. The understanding is that, to reach a broader audience, I have avoided the use of the technical language of the symplectic quantization and of the broader Lie-admissible quantization. This situation might interest some of your colleagues at Berkeley, and I would be happy to report it in an informal, noncommittal manner, in case you consider it appropriate, at any time of mutual convenience (from mid February to mid-March I will be in Europe to deliver invited talks on the subject).

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 11, 1980

Professor J. D. Jackson, Chairman
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Jackson,

Hoping that I do not abuse of your courtesy and time, I am taking the liberty of enclosing copy of the outline of a course in undergraduate, general physics I taught a few years ago, in case of possible value for the consideration of my candidacy. Please note the amount of time I dedicate in tutoring personally undergraduate students.

In regard to my current contacts with colleagues-editors on the puzzle of papers activating quantum mechanical inconsistency theorems (see my recent letter to you), you will pleased to know that several leading scientists have answered the call for help. For instance, Professors JERROLD MARSDEN and PAUL CHERNOFF of the Department of Mathematics at Berkeley (two leading experts in the problems) have expressed interest in helping us in reaching a mature editorial decision.

The situation is quite intriguing. Indeed, a number of editors (including myself) simply do not know how to handle papers activating the inconsistency theorems because of insufficient technical information either in favor or against. We are therefore grateful for the positive response by mathematicians.

These aspects will be discussed at the THIRD WORKSHOP ON LIE-ADMISSIBILITY I am currently organizing for this coming August. The meeting looks rather promising. In case you desire to be kept informed of the decisions reached at this meeting (irrespective of the consideration and outcome of my application), please let me know. It would be a pleasure for me to provide you with the most relevant data.

Sincerely

Ruggero Maria Santilli

RMS/ml
ecnsl

NO COMMUNICATION OF A FINAL DECISION WAS EVER RECEIVED FROM BERKELEY.

PART IX:

LAWRENCE

BERKELEY

LABORATORY

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Dr. BERNARD G. HARVEY,
Associate Director, Nuclear Science Division
Lawrence Kerley Laboratory
BERKELEY, California 94720

Dear Dr. Harvey,

I am hereby applying for a position in the nuclear science division of the Lawrence Berkeley Laboratory commensurate to my qualification and experience.

My application is specifically referred to and intended for the possible collaboration with interested experimentalists and theoreticians on the experimental test of Pauli's exclusion principle under strong interactions, beginning at the nuclear level, and following my original proposal published in the Hadronic J. 1, 574, 1978, and subsequently elaborated in a number of articles, either alone of in conjunction with mathematicians and physicists.

The main idea is to test the validity of Pauli's principle in nuclear physics to the effect of ascertaining whether this principle is valid in the arena considered in the same quantitative amount as it is valid in atomic physics, or very small deviations are experimentally detectable.

The proposal refers to the use of low energy scatterings of hadrons in the peripheral nucleons of nuclei selected in such a way that their volume is below the value predicted by the proportionality rule of the nuclear volume with the total number of nucleons. The objective, on experimental grounds, is that of ascertaining whether the wave function of identical peripheral nucleons of these nuclei is totally antisymmetric, or very small deviations exist and have escaped current inspections simply because not looked for. Preliminary assessements by nuclear physics indicate that this experimental test is feasible with current technology, with the understanding that it is predictably delicate and will predictably call for additional theoretical studies.

On theoretical grounds, the nucleons of the selected nuclei are in an experimentally established, statistically small, state of penetration of their wave packets. Recent studies have indicated that, under these conditions, we expect the emergence of forces more general than the trivial $f = -\partial V/\partial r$

(the so-called variationally non-self-adjoint forces), according, after all, an old idea since Fermi's time, which has been lately ignored to a considerable extent. In essence, this results in the expectation of an additive term in the (conventional) terms of the nuclear force, this time non-derivable from a potential and with a very small (statistical) coefficient. The structure of this nuclear force prohibits the possibility of representing the nuclear structure with conventional Hamiltonians $H = H_{free} + H_{int}$, by therefore invalidating the applicability of Heisenberg's equations also to a very small measure. Still in turn, this renders applicable the recently identified, broader, quantum mechanical equations of Lie-admissible (rather than Lie) algebraic character. Finally, this settings predicts very small deviations from the applicability of Pauli's principle for the nuclei indicated, that are quantitatively similar to the historical case of the violation of parity under weak interactions.

I would like to inform you that these studies are now conducted by an organized group of researchers including mathematicians and physicists. Some of the mathematicians are:
- Professor MYUNG, of the University of Norther Iowa; Professor TOMBER of the Michigan State University; Professor OEHMKE of the University of Iowa; Professor WENE of the University of Texas at San Antonio; and others.
Some of the physicists actively involved in these studies are:
- Professor OKUBO of the University of Rochester; Professor KOBUSSEN of the Institut fur Theoretische Physik der Universitat Zurich; Professor KTORIDES of the University of Athens in Greece (currently spending his sabbatical as my guest here at Harvard); Professor CANTRIJN and SARLET of the Instituut voor Theoretische Mechanics of the Rijksuniversiteit in Gent Begium (the latter spent the academic year 1978-1979 as my guest here at Harvard); Professor FRONTEAU and TELLEZ-ARENAS of the Université d'Orléans in France; Professor LÕHMUS of the USSR Academy of Science in Tartu; Professor JANG CHUN-XUAN of the People's Republic of China; and others.
Owing to the intriguing character of our studies, the number of mathematicians and physicists joining our efforts is increasing in a promising way. For instance, I just received notice that Professor ELIEZER and his group in Australia have initiated active studies on the so-called SU(2)-admissible generalization of the conventional SU(2)-spin symmetry (the latter being a prerequisite for Pauli principle, and the former a prerequisite for its expected deviations).

The coordination of these efforts is conducted via
- the HADRONIC JOURNAL, of which I am the founder and editor in chief;
- a yearly workshop on Lie-admissible formulations (the first was held here at Harvard in August 1978; the second was held also here in August 1979; and the third is scheduled for August 1980). In addition, a conference is independently under organization by mathematicians;
- two series of research monographs I am currently publishing, one with springer-Verlag under the title "Foundations of Theoretical Mechanics" (Volume I was published in 1978, and volume II is in press); and another with the Hadronic Press under the title "Lie-admissible approach to the Hadronic structure" (Volume I was published in 1978, volume II is in press; and volume III is scheduled for 1980).

The technical profile is essentially set by the so-called no-go theorem of Heisenberg-type quantization of the (pre)symplectic geometry, which is rigorously proved in the recent edition of "Foundations of Mechanics" by Abraham and Marsden. This theorem literally establishes that Heisenberg's equations are inconsistent for all polynomials Hamiltonians of order higher than the second. This exludes trivial, linear, selfadjoint models (in the variational sense, rather than the operator sense), but includes nonlinear conventional models, as well as generalized Hamiltonian structures for forces non-derivable from a potential, as computable via the so-called Inverse Problem of Mechanics (my monographs with Springer-Verlag). Regrettably, this theorem does not appear to be fully known in physics circles, inclusive nuclear physics circles involved in the recent intensification of quantum mechanical dissipative studies. Nevertheless, the theorem exists, it has been independently established by mathematicians experts in quantization, and it is stirring up a feverish activity.

These technical aspects have been considered in detail at our recent SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, attended by mathematicians and physicists from the U.S.A., France, Switzerland, Israel, and with corresponding participants from the USSR and the People's Republic of China. The proceedings will be published in the December issue of the HADRONIC JOURNAL, for distribution in early 1980.

For a preview of these proceedings, I enclose "Chart 4.9" from my Volume II with Springer-Verlag (now in press). Essentially, this is an account of the problem considered in a form understandable to graduate students, as well as researchers without a technical knowledge of symplectic quantization, and of the broader Lie-admissible quantization.

I would like to draw your attention on part 9 of this chart, pages 343-349 on the quotation of the rather numerous, historical, authoritative, voices of doubt on the practically implemented (these days), terminal character of conventional quantum mechanics. In particular, I would like to draw your attention on what we call a LEGACY BY EINSTEIN: the fact that he refused to believe up to his death in the terminal character of conventional quantum mechanics, as touchingly depicted by Heisenberg in his memoires "Physics and Beyond" (in Heisenberg's words, Einstein could at most tolerate quantum mechanics as a "temporary expedient").

I would like to express that this legacy by Einstein is closely related to my application and, in particular, to the fact that I have applied to the nuclear division of your laboratory. In essence, the studies indicated earlier have taken this legacy by Einstein seriously and worked out the implications to all possible details. In a few non-technical terms, it turns out that the conventional uncertainty of quantum mechanics is invalid under the conditions of overlapping of the wave packets and broader forces more general than those $f = -\partial V/\partial r$ of current use. The number of studies conducted on this legacy are numerous, independently based, and all leading to the same conclusion. For instance, the time evolution under variationally

non-self-adjoint forces is non-unitary . This implies (according to established theorems) that, assuming the conventional uncertainty is valid at a given time, it is not preserved in time. The mechanisms of departure from conventional setting is exactly the same as that of the departure from Pauli's principle which are expected under nonselfadjoint forces, thus confirming Einstein's vision for a genuine advancement. In particular, the departures from these conventional settings are differentiated into:

- the level of the nuclear structure, where at most very small departures are admitted, owing to the very small overlapping of the wave packets;
- the level of the structure of astrophysical bodies, where large deviations are expected, owing to the large state of penetration of the wave packets of the constituents one within the other due to the extremely high pressures and densities; and
- the level of the structure of the hadrons, where intermediate deviations are conceivable, as a result of these studies.

Nevertheless, in my view, the fundamental starting point is that at the nuclear level, where theoretical views by individuals or groups of researchers can be resolved at the experimental level in an expectedly clearer form and in an expected shorter period of time. It is understood that the experimental identification of very small deviations at the nuclear level would promote nonincremental studies at the hadronic level (beginning with a mathematically and physically consistent definition of constituent, that is, of a particle under joint elm and strong interactions and the state of mutual mpenetration of the wave packets) and at the astrophysical level (where a fundamental assumption would be invalidated, that the geometries for the interior problem are Lorentz in local character). This is the reason for my desire to be associated with the nuclear physics devision, although I would like to release to your laboratory any final decision, in case my candidacy is actively considered.

For your amusement, permit me to quote the following passage by Heisenberg:

> "In science it is impossible to open up new territory, unless one is prepared to leave the safe achorage of established doctrine and run the risk of a hazardous leap forward";

to which he adds immediately after:

> "However, when it comes to new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do."

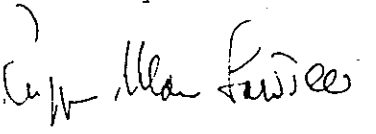
I have taken the liberty of quoting these passages by Heisenberg as one way to indicate that I am fully waware of the inertia inherent in our community toward studies of this type. Nevertheless, one of the duties of our profession is that of not taking for granted general beliefs, even when they are fully sound, and establish our knowledge via experiments. When referring to the problem of the basic physical laws for the strong interactions, I think that the implementation of this duty is essential.

On administrative grounds, you might be interested to know that my studies are conducted with support from the DEPARTMENT OF ENERGY, under grant no. AS02-78ER04742. It appears that the DOE is pleased with the output of my research and it is interested in continuing this support under the administration of a qualified Institution. In case my application is seriously considered, please feel free to contact the High Energy Physics Division of the DOE, e.g., Dr. DAVID C. PEASLEE, tel. 301 353 3624.

In relation to the letters of recommendations, I would like to bring to your attention that I have applied for a position at the Department of Physics of the University of California at Berkeley. Professor HOWARD A. SHUGART, Acting Chairman, has in his file a number of letters of recommendation by distinguished scolars. In case acceptable by your Institutions, I would appreciate the courtesy of the use of these letters. But, if this procedure is not acceptable, please let me know, and I shall solicite letters of recommendation addresses to the person indicated by you . Almost needless to say, on my part I would be simply honored for the consideration of a joint appointment.

I enclose for your consideration my curriculum, a list of references, and some informative material on my recent research-teaching-editorial activities, while I remain at your disposal for any assistance you might need.

Sincerely



Ruggero Maria Santilli

RMS/ml
encls.

c.c.: Professor H.A.SHUGART, Department of Physics
University of California at Berkeley

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 22, 1979

Dr. ROBERT W. BIRGE, Director,
Physics Division
Lawrence Berkeley Laboratory
BERKELEY, California 94720

Dear Dr. Birge,

I am taking the liberty of informing you that I have recently applied to the nuclear physics division of your laboratory for a position commensurate to my qualification and experience. In essence, I am interested either to a tenured appointment, or to an appointment with a serious consideration for tenure after the needed number of years of service. Also, I am the recipient of a DOE grant (now at its second year), and I am particularly interested in being able to apply, as principal investigator, for the continuation of this grant, either alone, or jointly with interested colleagues. I am also the editor in chief of the HADRONIC JOURNAL (now at its second year of bimonthly publications, all on full schedule), and I would like to continue this function at my new Institution. Finally, I am the organizer of a yearly meeting called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (the first two were held here at Harvard in August 1978 and 1979), and I would like the opportunity to continue this meeting at my new Institution (although no expenses from your laboratory would be expected).

I enclose copy of my original application to Dr. BERNARD G. HARVEY of your Division of Nuclear Physics, as well as my curriculum and illustrative material on my recent research-teaching-editorial activities. In relation to the letters of recommendations, I have asked the kindness to Dr. SHUGART at the Physics Department to provide copy of the letters in his file, to expedite procedures. Nevertheless, I remain at the disposal of your laboratory to solicite additional letters from distinguished scolars upon request. A list of my references is enclosed.

I hope you will be amused and intrigued to see that my application is specifically intended to test something taken for granted by the physics community at large: the validity of Pauli's exclusion principle in nuclear structures.

In essence, a number of mathematicians and physicists, including myself, have initiated a long term, in depth, study, via the use of the most advanced possible mathematical techniques, of a number of "legacies" by the Founding Fathers of contemporary physics, such as Fermi, Einstein, Jordan, von Neumann, Wigner, Pauli, etc., that are, in our view, basically

open at this time. These legacies essentially deal with historical, authoritative, voices of doubt on the validity for the strong interactions of the basic physical laws of the electromagnetic interactions.

I am a theoretical high energy physicist. I have experienced, personally, the elements of crisis in the current state of the art in strong interactions. In particular, I have tried for years to reach a structure model of hadrons via quark conjectures that was mathematically and physically consistent according to my personal standards, without publishing any paper at all. What I was after, and I failed completely, was an apparently "simple" task: a structure model of the lightest known hadrons-the octet of light mesons- that (1) is of quantitative character, that is, it is realized by equations of motion obeying physical laws, and, as such, it can be confronted with experiments (I was not interested in phenomenological models); (2) reproduces all the known physical data of the hadrons considered-the intrinsic characteristics and the decays, as well as their related fractions; and, last but not least, (3) achieves a strict form of confinement, that is, the equations of the bound state of a quark and an antiquark imply an identically null (and not an approximately null) probability of tunnel effects, to prevent decays such as

$$\pi^0 \longrightarrow q\bar{q}$$

which simply do not exist in nature.

This "simple" task was essentially inspired by the desire to follow Bohr. He considered the lighest known atom (and not, say, the palladium); he searched for a quantitative model of structure via equations of motion (Schrödinger's equation); and he believed in his finding because the model was able to account for all data known at the time, including all spectral lines (and not part of the spectral lines).

As sayd earlier, I failed completely in this task, despite years of trials. The inconsistencies I found in the conduction of this study were multiplying in time, rather than decreasing. I finally reached the conclusion that a quantitative structure model of the light mesons with a strict confinement of the assumed quark constituents is virtually unfeasible in the context of nonrelativistic and relativistic quantum mechanics.

I then discovered that I was not alone in this crisis. Several mathematicians and physicists have joined me in the attempt to reach a new, nonorthodox, critical, inspection of the situation.

My position both as an individual researcher and as editor in chief of the HADRONIC JOURNAL is the following.

(1) I believe in the final character of unitary models and QCD for the classifications of hadrons only (or, you may say, for the "exterior" or "chemical" or "Mendeleev-type" classification of hadrons);
(2) I support openly and sincerely the continuation of studies along these models when they are interpreted as jointly providing a structure model of each individual element of a unitary multiplet (for example,

I have recently organized, as part of the functions of the Hadronic Journal, a new series of reprint of papers along quark lines, and I have appointed Professors D.B. LICHTENBERG and S.P.ROSEN as independent Editors of this yearly series); but,

(3) I favor the conduction of studies also of fundamentally different orientation on the structure problem, under the condition that they are capable of achieving compatibility with the established unitary models of classification.

What I am trying to convey is that, according to our studies, it is possible that the historical dichotomy classification/structure that was necessary for the atoms, may result to be also necessary for the hadrons.

After having identified the established part (of current trends in hadron physics) in their classification content, that is, after having separated facts from beliefs, we have passed to a critical inspection of the structure problem.

However, unlike most of our colleagues, we have initiated this study by conducting an in depth analysis of the teaching by the Founding Fathers of contemporary physics. We have discovered in this way a number of crucial "legacies" that, in our view, are directly related to the problem of hadron structure. I am referring here to

Fermi's legacy: he clearly espressed doubts on the validity of conventional geometries (and, thus, conventional laws) for the region of space occupied by a strongly interacting particle; in particular, he thought that, under the conditions of overlapping of wave packets (as apparently necessary to even activate the strong interactions), particles experience forces more general than the simplictic $f = -\partial V/\partial r$ of current use (we call these forces these days variationally nonselfadjoint integrodifferential forces, that is, superposition of local and nonlocal forces derivable and nonderivable from a potential). This implies the lack of capability to represent the interactions via a Hamiltonian; the consequential lack of existence of Lie algebras at the level of the time evolution laws; the consequential inapplicability of conventional relativity and physical laws (the very Lie algebra of the Poincaré or Galilei group cannot be even defined); and the consequential need to do a serious homework, that is, to reinspect the foundations of contemporary physics. We believe that Fermi's legacy is fundamentally open at this time..

Einstein's legacy: he becane famous for not believing in the terminal character of quantum mechanics in general, and of the conventional indeterminacy of quantum mechanics in particular. In Heisenberg's words (see "Physics and Beyond", page 81), Einstein could at most tolerate quantum mechanics as a "temporary expedient". As also reported by Heisenberg, Einstein tried up to his death to identify counterexample to the conventional uncertainty. We believe that Einstein's legacy is also open

at this time, and it is particularly related to Fermi's legacy.

Jordan's legacy:Jordan became famous for not believing, this time, in the fundamental algebraic structure of quantum mechanics, the universal enveloping associative algebra of the Lie algebra of operators, as characterized in Heisenberg's representation (in our contemporary language). He then recommended a generalization of this associative algebra into a nonassociative form he selected of commutative type for statistical considerations (the celebrated Jordan algebras).We believe that Jordan's legacy is also open at this time, and deeply linked to Fermi's and Einstein's legacies.

Pauli's legacy: he made it quite clear in his historical papers and lectures that his exclusion principle was conceived under the lack of overlapping of the wave packets (the atomic structure). Indeed, when the wave packets overlap, he expected much "stronger" forces that generally prohibit the separability of the wave function, let alone the identification of the possible antisymmetric character. We believe that Pauli's legacy is also fundamentally open at this time, and deeply linked to Fermi's, Einstein's and Jordan's legacies.

Cartan's legacy: he made it quite clear that the Riemannian geometry can incorporate only part of Newtonian mechanics, that is, only the part compatible with Galilei's relativity. In particular, Newtonian mechanics includes systems with forces that are simply not geometrizable "a la Riemann". This legacy directly touches the current beliefs in the interior problem of gravitation (only). We believe that Cartan's legacy is still open at this time and deeply related to the other legacies.

What we have done is to conduct an in depth, technical study of these legacies. Even though much work remains to be done, one think has transpared clearly from these studies. Apart from historical reasons, these legacies appear to have a sound, strong, clear, physical foundation for the interior problem of hadronic matter, whether a hadron or a star. If you accept the experimental data that all hadrons have approximately the same charge volume, that the dimension of this volume coincides with the range of the strong interactions, and that the wave packets of these particles is exactly of that range of dimension, these legacies are simply anavoidable. Indeed, a necesaary condition to even activate the strong interactions is that particles enter into a state of mutual penetration or overlapping of the wave packets.

We have then initiated studies of forces structurally more general than the trivial $f = -\partial V/\partial r$ of current use, within the context of

- advanced mathematics (including the use of the theory of nonassociative algebras, functional analysis, and differential geometry);
- individual branches of physics (including: Newtonian Mechanics, quantum mechanics, classical and quantum field theory, classical and quantum statistical mechanics; etc.).

page 5.

The literature already accumulated on the technical studies considered here is so large to discourage an outline, and to simply prohibit it in a letter. To give you an idea,

(a) I have written two research monographs with SPRINGER-VERLAG under the title FOUNDATIONS OF THEORETICAL MECHANICS (Volume I-1978-volume II-in press), entirely devoted to the identification of rigorous methods for the treatment of the forces considered);

(b) I have written an additional series of monographs with the HADRONIC PRESS entitled LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE (Volume I was published in 1978, Volume II is in press, and volume III is scheduled for 1980); plus numerous articles on the problem; and

(c) we have a series of yearly reprint volumes of all articles in the field entitled APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS, edited by Professors H.C.MYUNG, S. OKUBO and myself (the first two volumes were printed in 1978, and we are working at two additional volumes of reprints of articles by mathematicians and physicists).

In addition we have the PROCEEDINGS OF THE WORKSHOP ON LIE-ADMISSIBILITY, plus the proceedings of a conference under independent organization by mathematicians.

I am fully aware that the reading of all this material is a task simply beyond your time, as well as the time of my colleagues at your Laboratory. To assist you, I have therefore enclosed copy of the "Chart 4.9" of my Volume II with Springer-Verlag. It presents an outline of the problem in a language readable by graduate students and researchers without a technical knowledge of the simplectic quantization and of the broader Lie-admissible quantization.

I would like to stress, however, that the technical presentation is elsewhere.

I hope you will have the time of reading this "chart" (in its nautical sense...) and have the elements, in this way, to reach an independent assessement of these legacies (they are outlined in the final part, Part 9, pages 343-349). I hope you will have in this way a chance to share our views, that is, the need to resolve experimentally the problem of the basic physical laws for the strong interactions, as an item of first research priority, with intriguing historical implications, whether the test are positive or negative for conventional laws. The problem of the structure of hadrons is, in our view, of only secondary physical relevance.

After due consultation with the members of our group, I have selected my application to your Laboratory for the test of Pauli's principle (amoung numerous tests under consideration) for several reasons. The first is that this test is the most close to maturity of formulation. The second is that it will cost expectedly less than a comparative test in high energy physics, and will call for expectedly less time. The third is that the dynamics of the test has been conceived and patterned along

the historical legacies indicated earlier. Indeed, we have selected for the test nuclei whose charge volume is below the value predicted by the proportionality rule with the total number of nucleons. For these nuclei, nucleons are in a statistically small state of penetration of their wave packets. In turn, this

- implies expected, small, additional, terms in the nuclear of force that are not derivable from a potential (Fermi's legacy);
- implies a nonunitary character of the time evolution law, by therefore invalidating, under these conditions, the preservation of the conventional uncertainty in time, assuming that it holds at one given value of time (Einstein's legacy);
- demands a generalization of the associative character of the envelope to technically represent the broader forces admitted (Jordan's legacy);
- predicts a very small departure from the Fermionic character of identical nucleons of the selected nuclei, via forces that imply a small departure from the exact separability (Pauli's legacy)
- and implies forces that are not geometrizable "a la Riemannian", thus implying a direct test of a central assumption of current trends in the interior problem of hadronic matter; that the geometry is locally Lorentz in character (Cartan's legacy).

In conclusion, we believe that the historical, authoritative, voices of doubts on the use of conventional physical laws, algebraic structures, and geometries for the strong interactions, are such to deserve a serious consideration by our community of experimentalists, beginning most importantly with our community of nuclear experimentalists. After all, we are currently spending truly large amounts of taxpayer's money in experiments in strong interactions. What we are interested in is, whether I join your Laboratory or not, that, jointly with the conduction of these valuable experiments, we also begin the experimental verification of the basic laws for the strong interactions, irrespective of how sound they may appear to the theoreticians.

In closing, permit me to candidly express my view for the implications of these experiments "vis-a-vis" with current quark conjectures for the hadronic structure.

Apart isolated exceptions,physicists academically and financially committed to quarks generally dismiss these experiments as without sufficient motivation or, in some extreme cases (not so unusual), as without physical values.

This is due to a number of circumstances. The first is that physicists, these days, are accustomed to reach judgments on a paper by spending 60, at most 90 seconds on it. I therefore believe that quark committed physicists simply have not the technical knowledge of the studies by mathematicians and physicists on these historical legacies. I also believe that they are in good faith. The fact remains, in my view, that the 60-to-90 seconds rule simply cannot be applied to our studies to

reach a mature, technical, professional judgment (only the methods of my monographs with Springer-Verlag call for a two-semester seminar course for a detailed presentation, and this excludes the most important tool used in our studies: the Lie-admissible algebras).

Also, I believe that quark committed physicists see, in experiments that might even remotely invalidate conventional laws for the strong interactions a "threat" to the quark conjecture.

The reason why I am candidly touching this argument is to indicate that this is not the case and that, again, this conceivable negative attitude (I have personally experienced several times) is only the result of the lack of adequate technical knowledge.

Physical results of the type of the prediction and discovery of the Ω^- and, more recently, of the J/ψ particles are now part of the history of physics and will remain part of it. No further advancement in knowledge can potentially invalidate them.

It is true that, in case the legacies by Fermi, Einstein, Jordan, Pauli, Cartan and others will eventually result to be true, the notion of quark as a hadronic constituent, cannot be even defined, let alone technically treated. But this is no disaster for the quark models, in my view. The true essence of physics is a sequence of improvements of our approximation of nature. Einstein's relativity did not "invalidate" Galilei's relativity. It merely identified the physical arena in which it provides a good approximation. These relativities are crucially dependent, conceptually and technically, on point-like approximations of particles and action at a distance only ($f = -\partial V / \partial r$). Apart extreme technical complications (e.g., renormalization), this notion by Galilei and Newton persists in its entirety in the most advanced contemporary treatments, such as QCD. Par contre, we are trying to do the expected next step, a first, but realistic, representation of particles as extended objects, when in interactions at distances smaller then their dimension. Assuming that our studies are true, the physical validity, effectiveness and value of the quark models will remain unaffected. We would have simply identified their arena of applicability as a first approximation: the point like interpretation of hadrons and of their constituents.

In conclusion, what I am trying to convey for the quark community of your Laboratory is that the conduction of the experiment I have proposed, under no circumstance should be interpreted as intended to test the credibility of their views. After all, in Bohr's words, we are all participants and spectators of a continuing scientific process.

In this spirit, I would like to express my appreciation for your time and consideration.

RMS/ml, encls.
c.c.: Drs.B.G.Harvey and
H.P.Stapp.

Sincerely

Ruggero Maria Santilli

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

Area Code 617
495-2170

Science Center
One Oxford Street
Cambridge, Massachusetts 02138

January 9, 1980

Dr. B.G. Harvey
Associate Director
Nuclear Science Division
Lawrence Berkeley Laboratory
Berkeley, California 94720

Dear Dr. Harvey,

Hoping that I do not abuse of your time and courtesy, I would like to add additional data to my application of October 10, 1979 for a position at your Laboratory in regard to theoretical and, in due time, experimental studies for the experimental verification of the validity or invalidity of Pauli's exclusion principle in nuclear physics.

Theoretical aspect. The studies are progressing on schedule. I enclose copy of the Table of Contents of the Proceedings of our Second Workshop on the problem. As you can see, it is a considerable effort for over 1,500 pages by distinguished mathematicians and physicists from the USA, USSR, China, and other Countries. Part A on the review of the state of the art presents, without proof, some 500 theorems, lemmas, propositions, etc. all of either direct or indirect relevance to the problem considered.

Particularly intriguing are two theorems of invalidation of Heisenberg's equations for all Hamiltonians of polynomial order in r and p higher than the second (that is, for nonlinear equations of motion). One of them has been proved by mathematicians (Abraham and Marsden) and the other by physicists (Hood, Hellman, Kobussen, and myself). By keeping in mind that nonlinear effects are rather natural in strong interactions, according to a growing consensus, these theorems are suggesting, rather forcefully, the experimental verification of the basic laws. The theorems in question are reviewed in details in my state of the art memoir of Part A of the proceedings, jointly with their technical and historical implications.

In case you desire a complimentary copy of these proceedings, please let me know, and I shall do my best to secure it from the publisher.

Experimental aspect. I am not an experimentali st and, thus, I cannot provide you with a technical assessement of this profile. Nevertheless, permit me to express on an informal basis the remarks I have received by a number of colleagues.

You are eventually aware of the experiments currently under way by Dr. C.G.Shull of MIT and a number of other participants, via neutron beams on selected cristals. Pauli principle can be (apparently) tested via these experiments via a double rotation of the cristals, either magnetically or mechanically, and then comparison of the data. If neutrons, when passing through the cristal, are exact fermions, the data should coincide after a double rotation, apart experimental errors. It appears that current initial readings give the same data, up to 1/2 degree. This, of course, does not establish a deviation from Pauli's principle (even though that predicted is of this order of magnitude). In fact, it may be due to the error for the experimental set up available.

Nevertheless, the view expressed to me by a number of colleagues is that experiments of this types can be, in principle, improved considerably in their approximation via auxiliary means.

page 2.

I do not know whether this is indeed the case .The point is that, in case the approximation of the experiment can be independently established, the deviation from the exact 720^{0} rotation may be subjected to a quantitative experimental study, rather than to guessing.

At any rate, what I would like to convey is that the experimental contact of my studies may well occur considerably earlier than what we currently believe (on my part, I was expecting no contact of this type for years, and I have been surprised when informed by independent collegues of the experiments indicated above and their potential).

Administrative aspect. As you know, I am the recipient of a grant from the Department of Energy that covers completely my salary and all my research expenses. This grant is now at its second year. I have received verbal communication of the renewal of the grant. DOE is rather pleased of the outcome of the studies (particularly of the increasing number of qualified scientists all over the World they are attracting). According to all indications available, this grant will be not only continued for a number of years, but likely increased whenever the actual contact with experiments is achieved.

My application to join your Department is therefore primarily for the administration of my grant, if at all compatible with your administrative practices. I hope in this way to achieve some stability which appears to be essential for the type of study under consideration, not only because of inherent long term character, but also because potentially open to interferences from vested interests of different orientation.

In closing, permit me to suggest that you take all the necessary time to reach your decision on my application. I am renewing my grant via local administration, so that I am not in need of a rush decision. On the contrary, I would like to rely on your judgment in regard to the best time for reaching a decision.

Hoping that I can visit you some time* and have the pleasure of meeting you, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS|ml
encls.
c.c.: Drs. Birge and Trippe

* Perhaps the theorems of invalidation of Heisenberg's equations might interest your division. I understand that a number of Editors are considering a sort of moratorium on all papers activating these theorems.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

Area Code 617
495-2170

Science Center
One Oxford Street
Cambridge, Massachusetts 02138

January 30, 1980

Dr. B. G. HARVEY
Associate Director
Nuclear Science Division
Lawrence Berkeley Laboratory
BERKELEY, California 04720

Dear Dr. Harvey,

You might be intrigued and amused by the enclosed letter I am passing to editors and selected colleagues. It may give you information on the status of the strong interactions at the editorial level, because of the lack of, not only sufficient theoretical insight, but also of experimental information on the basic laws.

The understanding is that, to reach a broader audience, I did not use the language of the symplectic quantization and of the broader Lie-admissible quantization.

Best Personal Regards

Ruggero Maria Santilli

RMS/ml
encls.

c.c: Drs. R.W.BIRGE and T.G.TRIPPE

ABSOLUTELY NO ACKNOWLEDMENT WHATSOEVER WAS RECEIVED FROM THE LAW. BERK LAB FOR ALL THE ABOVE CORRESPONDENCE (SECT. X) AND SCIENTIFIC ENCLOSURES.

PART X

U.S. NATIONAL LABORATORIES

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

February 7, 1978

Dr. R.M. Santilli
Massachusetts Institute of Technology
Department of Physics
Cambridge, Massachusetts 02139

Dear Dr. Santilli:

We here in the theory group at Fermilab wish to thank you very much for your application for a position with us. At this time it is not possible for us to offer you such a position, though the competition was strong and your name was always under careful consideration.

Sincerely yours,

Henry D.I. Abarbanel
Theoretical Physics Department

HDIA/em

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 22, 1978

Dr. ROBERT WILSON,
Director,
FERMI NATIONAL ACCELERATOR LABORATORIES
P.O. Box 500
BATAVIA, Illinois 60510

Dear Dr. Wilson,

Just a few words to inform you that the first issue of the HADRONIC JOURNAL has been printed in April 1978, on schedule, and distributed to subscribers.

A copy of the cover of the issue is enclosed for your consideration. I hope you will be pleased with the results of our efforts.

To my knowledge and understanding, it appears that potentially new approaches to hadron structure are now surfacing, with intriguing experimental and theoretical, potential implications. I shall take the liberty of keeping you personally informed in case they materialize.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS|is
encl.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 22, 1978

Professor WOLFGANG K. H. PANOFSKY,
Director,
STANFORD LINEAR ACCELERATOR CENTER
Stanford University
STANFORD, California 94305

Dear Professor Panofsky,

Just a few words to inform you that the first issue of the HADRONIC JOURNAL has been printed in April 1978 and distributed to subscribers on schedule.

A complimentary copy was mailed to Professor Joseph BALLAM and a regular subscriber's copy has been mailed to your library. A copy of the cover of the issue is enclosed for your consideration. I hope you will be pleased with the results of our efforts.

Again, I would like to thank you for the courtesy of your assistance.

To my knowledge and understanding, it appears that potentially new approaches to hadron structure are now surfacing, with intriguing, experimental and theoretical, potential implications. I shall take the liberty of personally informing you, in case they materialize.

Very Truly Yours

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS|is
encl.

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

February 3, 1978

Dr. Ruggero Maris Santilli
Editor-in-Chief
Hadronic Journal
Department of Physics
Harvard University
Cambridge, Mass. 02138

Dear Dr. Santilli:

Thank you for your letter of January 29 in which you request me to identify an experimental high-energy physicist to serve on the Editorial Council of the "Hadronic Journal." I would like to suggest Professor Joseph Ballam. Dr. Ballam is the Associate Director of the Research Division of SLAC and is well qualified to serve in such a position.

Sincerely,

W. K. H. Panofsky
Director

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

February 6, 1978

Dr. W.K.H.PANOFSKY,
Director
Stanford Linear Accelerator Center
Stanford, California 94305

Dear Dr. Panofsky,

I would like to express the sentiments of our appreciation for the courtesy of your letter of February 3, 1978.

I enclose copy of our invitation to Dr. Joseph Ballam for membership in the Editorial Council of the Hadronic Journal.

Best Personal Regards

Ruggero Maria Santilli

RMS|is

enc.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

February 6, 1978

Professor JOSEPH BALLAM
Associate Director of the Research Division
Stanford Linear Accelerator Center
Stanford, California 94305

Dear Professor Ballam,

we are taking the liberty of informing you of the recent organization of a new journal in high energy physics, called HADRONIC JOURNAL, according to the enclosed announcement.

It is our firm determination to come out with a quality journal which, in due time, could represent a valuable medium of publication of original contributions in fundamental issues of high energy physics. But, in order to achieve this objective, we need qualified advice.

We are therefore setting up the EDITORIAL COUNCIL of the journal. As customary, members of the Council are expected to provide confidential advice to the editors on papers and matters which go beyond the normal referee routine. Regrettably, we do not have a budget for a honorarium in 1978, although it appears that we will likely have a budget for logistic expenses. The projected yearly time per member will be quite minimal, but quite valuable for the journal.

Following a kind indication by Dr. W.K.H. Panofsky, we are here inviting you to became a member of this Editorial Council, jointly with a number of collegues from selected U.S. and foreign Institutions.

Thanking you for your consideration, we remain at your disposal for any additional information you might need.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli, for the
HADRONIC JOURNAL

c.c.: Dr. W.K.H.Panofsky, Director
SLAC

encl.

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

February 15, 1978

Professor Ruggero Maria Santilli
Lyman Laboratory of Physics
Harvard University
Cambridge, Ma. 92138

Dear Professor Santilli:

I am happy to accept your invitation to become a member of the Editorial Council of the Hadronic Journal.

Sincerely yours,

J. Ballam
Professor and
Associate Director
Research

JB:hm

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

February 23, 1978

Professor Joseph Ballam,
Associate Director of Research
Stanford Linear Accelerator Center
P.O.Box 4349
Stanford, California 94305

Dear Professor Ballam,

It is a pleasure to express the sentiments of our appreciation for your acceptance of the membership in the Editorial Council of the Hadronic Journal.

We are currently studying the best way to keep you fully informed of the initiatives and activities of the Journal while taking the least possible of your time.

We contemplate to consult with you in this respect in the near future.

Best Personal Regards

Howard Georgi

Ruggero Maria Santilli

HG-RMS/cgg

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 3, 1978

Professor JOSEPH BALLAM,
Associate Director of Research
SLAC
Stanford, California 94305

Dear Professor Ballam,

You will be pleased to know that, in addition to your partecipation, we have the following members of the EDITORIAL COUNCIL of the HADRONIC JOURNAL
- Nobel Laureate CHEN NING YANG,
- Professor ROBERT MERTS, Director of the Instituut voor Theoretische Mechanica (an old European school in theoretical Mechanics) and
- Professor HYO CHUL MYUNG of the Univ. of Northern Iowa, Department of Mathematics (a leading expert in abstract algebras).

Additional memberships are expected and I shall inform you as soon as they are finalized.

The first issue of the Journal is also approaching finalization. Some of the confirmed papers are the following:
- S. STERNBERG, Harvard, Dept. of Math, on a geometric approach to Yang Mills,
- T. KATO, Berkeley, Dept. of Math., on certain aspects of scattering theory (imprimitivity theorems),
- S. OKUBO, Rochester, on certain restrictions for semisimple gauge groups,
- C.N.KTORIDES, Univ. of Athens, Greece, on a stimulating conjecture for Lie-admissible quantization in field theory (for couplings not derivable from a potential),
- H.C.MYUNG, on a review of the state of the art in mathematical literature on Lie-admissible algebras,

as well as possible additional papers from Lyman Laboratory.

I should add that these papers have been confirmed by their authors but I have not received them as of now, with few exceptions. If you want to have a preview of any of these papers, please let me know, I would be happy to mail them to you. In any case you will receive a complimentary subscription from the publisher with prompt mailing of the issues for your file.

As for the subsequent issues, I would appreciate the courtesy of a confidential indication of collegues at Stanford who are about to complete valuable papers for my independent invitation. You can trust in my utmost discretion.

I am also contacting you because I would greatly appreciate your confidential advice on the following delicate project.

We have all followed with interest the valuable studies by Fairbank at Stanford. I am, hovever, under the impression that our community is somewhat misinformed because of the dilution of the issue in specialized journal as well as nonspecialized magazines (see the MORPURGO-FAIRBANK letters in the Dec. 1977 issue of PHYSICS TODAY).

I believe that the presentation in the HADRONIC JOURNAL of an accurate state of the art in the search for quarks, whether possible or otherwise recommendable and, particularly, if contain an identification of the controversial points, would be a service to our community. In particular, it could stimulate subsequent studies for a possible future resolution.

I would therefore greatly appreciate your advice on the following issues:
- do you think that it is advisable at this moment to attempt this identification of the state of the art in the search for quarks?
- If so, do you think we should invite contributions from different currents for joint publication (that is, invite Fairbank as well as Morpurgo and other qualified contributors)?

My instinct tells me that the publication of only one view would imply partisanship by the HADRONIC JOURNAL in the issue and therefore could be in the short or long run counterproductive. To be more specific, I believe that we should either publish jointly representatives of different currents for the independent assessement by the interested readers, or skip the issue at this time. But, quite frankly, I do not know whether I can achieve a mature judgement alone. Your confidential advice would be therefore greatly appreciated.

In conclusion, if you think that it is advisable at this time, I could personally invite FAIRBANK as well as MORPURGO and any other researcher you might suggest to write a review paper on their studies under the proviso that
- each researcher is made aware of the invitation for the other for joint publication, and that
- each researcher has full opportunity to inspect the paper by the other prior to the joint publication.

If you think that such an attempt is advisable at this time, I could draft a letter of invitation for your confidential inspection. In essence, I believe that MORPURGO could be interested in such a project, but I have no indication with respect to FAIRBANK.

Almost needless to say, particularly in view of the delicate nature of the issue, you can trust in my utmost confidentiality. To be more specific, I intend to disclose any possible advice from you to no third party, unless specifically authorized by you.

Please take all the necessary time to consider this issue because it is not of utmost priority. Rather than anwering by letter, please feel free to call me at any time of your convenience. My office phones are (617) 495 3212 (Lyman) and (617) 495 2170 (Dept. of Math. -most of the time) although it is sometime difficult to reach me during day time. You can always reach me at home (617) 969 3465 from 7 p.m. on, Boston time.

Again, I have no words to express my appreciation for your partecipation.

Sincerely

Ruggero Maria Santilli

P.S. You will be pleased to know that a research grant application I submitted with Shlomo Sternberg to the Department of Energy (formerly ERDA) on the old idea that the strong hadronic forces are not derivable from a potential, has been recently funded. As an incidental note you might be interested to know that the Journal was launched after an informal clearing with DE (David Peaslee).

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

July 19, 1978

Professor WOLFGANG K. H. PANOFSKY, Director
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Panofsky,

I am contacting you for a respectful , but open and passionate appeal on the current situation of hadron physics, as well as for your advice and assistance on a promising but delicate step I have implemented in the HADRONIC JOURNAL. The situation of hadron physics is, in my view, so grave, to demand the use of a candid, nonacademic language as a prerequisite for the identification of the current problems in a way as clear as possible.

As we all know, gigantic financial investments have been devoted during the last decade to theoretical and experimental studies on the problem of hadron structure. The net outcome of these investments is essentially given by the current quark models. Permit me the liberty of indicating that, in my view, these models are fundamentally removed from the traditional values of physical research, that is, the manifestation of a scientific truth. Instead, they are the expression of mere opinions by individual or groups of researchers, such as the opinion that the quarks are the physical constituents of hadrons, complemented by the opinion that they confine, complemented by the still further opinion of rather complex decay processes sometimes mediated by additional yet unidentified particles, etc.

But, perhaps, these are opinions with only secondary implications. What I consider more fundamental is the tacitly implemented opinion that the relativity and quantum mechanical laws which have proved so effective for the atomic (as well as nuclear) constituents necessarily apply also to the hadronic constituents in their currently known form, without even considering the problem of their direct, explicit and unequivocal experimental verification.

I am confident of your agreement that we simply cannot continue indefinitely to conduct basic research on the basis of mere beliefs by individual physicists on fundamental issues. It is time to subject the current epistemological, theoretical and experimental attitudes to severe scrutinies and profound revisions.

The promising, but delicate step implemented in the first issues of the HADRONIC JOURNAL by a number of collegues (Professors Engels, George, Henin, Ktorides, Mayné, Myung and Nobel Laureate Prigogine) and myself consists of a tentative identification of the revisions considered which appear advisable. Additional papers by other authors (e.g., Professor Kim) along the same lines are likely to appear in the subsequent issues. I outline below my personal impressions on the outcome of these studies.

THE EPISTEMOLOGICAL PROFILE. As we all know, high energy physics is currently plagued by a sea of minute incremental contributions which has lately reached alarming proportions. Jointly, we have experienced the complete lack in recent times of any fundamental progress which is even partially comparable to the great achievements of the first part of this century.

Hoping in a benevolent impunity, I intend to be on record by indicating that the current grave situation of high energy physics is a direct consequence of a widespread resistance at a number of decisional levels against the consideration of truly fundamental issues, essentially motivated by the strictly antiscientific belief that we have reached the terminal laws of the physical universe.

I have experienced numerous occurrences of this nature during my past academic life. For instance,

my attempts in the past of establishing a dialogue on the need for an experimental verification of Einstein's relativity and Pauli's principle for the hadronic constituents have met with a categorical refusal to even consider the issue. Similarly, I know of a number of collegues with young brilliant minds who have been forced to truncate their promising studies on fundamental issues to academically survive.

In the interest of the pursuit of human knowledge, it is vital that we reestablish the traditional priorities of basic research which have produced such fundamental contributions up to the earlier part of this century and lately abandoned, via the traditional way of their conduction, that is, via criticisms of experimentally unverified basic laws.

According to this view, utmost priority should be given to the studies of the problem of the basic physical laws which are applicable to strong interactions in general and to the hadronic constituents in particular. The studies of the construction of specific models of hadronic structure is of purely secondary physical relevance. I am confident in your scientific vision to see that, after all, established laws may well result to be inapplicable in their currently known form in the arena considered according to much of the historical occurrence at the atomic level. In the final analysis, there exist profound physical differences between the electromagnetic and strong interactions which may well result in the need of the courageous construction of covering laws for the latter interactions. In turn, if this resultsto be the case, it will inevitably imply profound differences in the quantitative characterization of "constituents under electromagnetic interactions", that is, atomic constituents, and "constituents under strong and electromagnetic interactions", that is, hadronic constituents under covering laws. I am sure you realize that, if the established laws of the atomic phenomenology will result to be inapplicable to the hadronic structure, the quark conjecture is ruled out in a final form. The need for a revision of current trends is then simply imperative.

The HADRONIC JOURNAL was born for the specific objective of initiating the restoration of traditional priorities of basic research. Indeed, I give utmost priority to studies on fundamental issues, while papers of minute incremental nature on established trends are generally reroued to other journals. I sincerely hope that independent and more effective restorations of these crucial priorities is initiated also in other Institutions as soon as possible.

THE THEORETICAL PROFILE . As we also know, the quark conjecture is literally dominating the current hadron physics. This situation, in my view, has also reached alarming proportions, to the point that the current technical and review literature either explicitly or implicitly exclude the possible study of fundamentally different models. There is no doubt that the quark models are scientifically valuable, deserve due attention and their study must be continued. However, the current virtual restriction of studies on the fundamental problem of comporary physics along only the quark conjecture is, in my view, strictly antiscientific.

Also hoping in a benevolent impunity, I intend to be on record by indicating that this grave situation is a direct consequence of a widespread resistance at a number of decisional levels against the support, promotion and guidance of any research on hadron structure other than quark oriented. I have personally experienced numerous occurrences of this type in my past academic life and I know of other occurrences experienced by other researchers.

To avoid a monopolistic condition and conduction of research on the fundamental problem of contemporary physics, ultimately based on mere opinions by individual researchers, it is vital to implement a well balanced community of basic studies in which, jointly with the continuation of efforts along the quark conjecture, fundamentally different approaches are attempted, solicited and guided for a comparative confrontation with physical reality. To be specific in this other crucial point, it is not sufficient that responsible officers at the decisional levels express a benevolent form of reception toward the construction of truly new models of structure, as conventionally stated, when they have reached maturity (which is the equivalent of the truncation of such studies). Instead, it is essential that they give proof of promoting, guiding and supporting them in the same measure as that for any other plausible approach.

The second objective of the HADRONIC JOURNAL is precisely that of initiating a nonmonopolistic presentation of research on hadron structure in which, jointly with papers on the quark models, you can see basically different approaches. Again, this is not the result of a benevolent, passive attitude on my part. Instead, it is the result of a laborious effort of promotion and support for valuable studies irrespectively of whether quark or non-quark oriented, because the problem of hadron structure is fundamentally open at this time and the "authority of a thousands" (in GALILEI's words) of quark believers is not sufficient to establish a scientific truth. I sincerely hope that independent and more effective achievements of a well balanced condition and conduction of research on hadron structure is implemented also in other Institutions as soon as possible.

THE EXPERIMENTAL PROFILE. As we also know, the experimental efforts of the last decade have produced clear contributions to human knowledge, such as the discoveries of the Ω^- and J/ψ particles. However, these efforts have failed to produce the final solution of the problem of hadron structure, they have failed to provide clear means of selection on clear physical grounds among an ever increasing number of quark models, and they have failed to give even a minimal but effective guidance for the final orientation of theoretical studies on structure.

It is at this point where my appeal for benevolent impunity reaches its climax. I intend to be on record by indicating that, unless a profound revision of the current experimental trends is implemented as soon as possible, the responsible officers at the decisional levels will acquire a historical responsibility of delaying the advancement of fundamental human knowledge.

As we all know, the current experimental trends are essentially restricted to the identification of new particles and their data in symbiotic condition with unitary models. I believe that these unitary models with their impressive experimental backing have produced a Mendeleev-type classification of hadrons of clear physical relevance and of virtually conclusive character. However, I believe that the problem of structure demands fundamentally different (although compatible) approaches and, thus, fundamentally different experiments according to exactly the same dichotomy classification-structure which resulted as necessary at the atomic level. I simply do not see how the discovery of new particles can provide the final solution of the problem of hadron structure (besides a further proliferation of different unknown quarks and, thus, a further abandonment of traditional scientific values), when so many already discovered particles have failed to do so.

The third and most delicate objective of the HADRONIC JOURNAL is to attempt the identification of the future orientation of experimental high energy physics which is needed to achieve a truly effective conduction of research on the problem of hadron structure.

Copies of the first issues of the HADRONIC JOURNAL are enclosed for your consideration. Additional copies of representative papers have been separately mailed to you, in case you intend to submit them to some of your associates, in addition to the regular subscriber 's copy which should be in your library. Additional reprints are at the disposal of interested collegues within the limitation of my budget.

The revision of the future orientation of experimental high energy physics I have proposed (HADRONIC JOURNAL 1, 223-423 (1978) and 1, 574-901 (1978)) is that utmost priority be given to the experimental tests of the validity or invalidity of established relativity and quantum mechanical laws for the strong interactions in general and the hadronic constituents in particular. According to this proposal, the discovery of new particles and the problem of the right model of structure are of purely secondary priority.

Permit me to indicate that my rudimentary and speculative studies on the quantitative representation of the possible invalidity of Einstein's relativity and Pauli's principle for the hadronic constituents (A) are primarily intended to stress the need of their verification; (B) are deficient and incomplete on technical grounds, being the results of an isolated researcher; and (C) are not intended to express the sole approach to hadron structure. Nevertheless, you should be aware that these papers have been specifically written in a provocative language as a result of a laborious search to stress the grave situation of hadron physics and to emphasize the need of the revisions here outlined.

You might be also interested to know that Nobel Laureate Prigogine and his collaborators have presented an independent analysis (HADRONIC JOURNAL 1, 520-574 (1978)) based on a different technical profile (thermodynamical), but I believe inspired along similar lines: it is time to subject the basic physical laws used for hadron structure to a severe scrutiny. The problem of the right model of structure is of purely secondary priority.

Permit me to confess that, as Editor in Chief of the HADRONIC JOURNAL, I am facing a responsibility (I have, after all, created), which is growing considerably beyond my knowledge and possibilities. Indeed, to achieve the well balanced condition and presentation of research stressed earlier, I have solicited contributions along two opposite approaches to the same fundamental issue: validity and invalidity for the strong interactions of the experimentally established knowledge for the electromagnetic interactions. As a result of this action, intriguing, but quite delicate papers are likely to arrive in the near future.

Owing to this situation, I would be grateful for your advice and assistance on the following points.

I - GUIDANCE ON EDITORIAL ASPECTS. I would be grateful whether you can let me know your viewpoint on the following issues.

I-A. Do we possess at this time direct, explicit and unequivocal experimental evidence that the basic physical laws of the electromagnetic interactions (Einstein's special relativity and Pauli's exclusion principle, in particular) are verified for the hadronic constituents?

I-B. If not, do you think that a proper presentation of opposite viewpoints on this fundamental issue is scientifically valuable and effective? and

I-C. If yes, do you have specific guidelines in which the debate should be contained?

II - ASSISTANCE TO ACHIEVE A WELL BALANCED PRESENTATION OF RESEARCH . In essence, my speculative, quantitative studies on a possible inapplicability of established laws for the hadronic constituents have apparently stirred up an interest beyond my cautious expectation. The net result is that a number of authors are apparently working on contributions along the idea of the inapplicability of established laws. In turn, I have reason to expect that the next few issues of the HADRONIC JOURNAL will present contributions along only this profile. This is contrary to the principle of a well balanced conduction and presentation of research to which I have been educated.

I would therefore be grateful whether you can assist me in the identification of theoretical high energy physicists interested in balancing this situation with articles in the HADRONIC JOURNAL or in any other journal of their preference specifically devoted to the presentation of the quantitative reasons why, according to the quark models, the basic laws of the electromagnetic interactions also apply to the hadronic constituents.

In essence, I am asking that quark believers sit down, organize their thoughts, eventually reinspect available experimental data (e.g., from deep inelastic scatterings), and provide the scientific community with the quantitative reasons why they expect the validity of established relativity and quantum mechanical laws within a hadron.

Am I asking too much?

III - ASSISTANCE ON THE TRULY IMPORTANT ASPECT, THE EXPERIMENTAL TESTS OF ESTABLISHED RELATIVITY AND QUANTUM MECHANICAL LAWS FOR THE HADRONIC CONSTITUENTS.

As the recipient of a research grant from the Department Of Energy, it is my duty to identify the problem considered and provide my contribution for its future resolution either in favor or against established laws. Permit me to confess that, after having spent a number of years on this issue, I have reached the conclusion that the issue considered cannot be resolved either way at the theoretical level only, beyond the level of personal opinions, or viewpoints, or conjectures by individual physicists which in any case remain far from a scientific truth. This is due to the fact that, as we know, a complex topology of assumptions (e.g., in the current so-called experimental tests of QCD), in my view, does not establish an unequivocal scientific truth. This is here not intended

to diminish the scientific values of theoretical studies, which are and will remain essential. But, being a theoretician, I must stress that the final resolution of the fundamental issue considered must be achieved via clear, direct and unequivocal experiments.

The outcome of my tentative studies on this issue is essentially the following.

It appears that the first experimental tests on the problem of the physical laws for the strong interactions can be conducted by restarting at the nuclear level. In nontechnical terms, in my second paper (Section 4-21 and page 882) I have proposed the experimental test whether Pauli's exclusion principle is exactly valid for the nuclear constituents (that is, it is valid in the same measure as that for the atomic constituents), or very small deviations can be experimentally established (e.g., of the same order of magnitude as those of some of the violations of discrete symmetries in particle physics). The indication of some experimental nuclear physicist interested in considering such a test would be appreciated.

Owing to a number of technical reasons, possible very small deviations from Pauli's principle at the nuclear level offer the intriguing perspective of large deviations at the hadronic level. The technological aspect, however, now becomes considerably more involved. After all, I am here referring to the transition from the experimental detection of a hadron as a whole, to that of the behaviour of its constituents. For this reason I have absteined from suggesting specific tests in my papers at this time, a part from generic proposals presented in Section 5.5 of my second paper. Specific proposals will appear in the subsequent issues of the HADRONIC JOURNAL by other authors, and others are available in the existing literature, although they have been ignored until know, to my knowledge.

I would therefore appreciate the indication of experimental high energy physicists interested in

III-A: conducting a preliminary, orientational study for a more adequate identification of the problem;

III-B: studying the possibility whether available experimental data can be effectively used as tests; and

III-C: conducting a feasibility study whether new experiments can be specifically conceived and realized within the context of the currently available technology on the fundamental issues, that is, validity or invalidity of Einstein's relativity and Pauli's principle for the hadronic constituents.

Being supported by DOE, as indicated earlier, I am taking the liberty of sending copy of this letter to Drs. DEUTCH, HILDEBRAND, KANE, PEASLEE and WALLENMEYER of such Governmental Agency. I am also taking the liberty of sending a courtesy copy of this letter to Drs. KRUMHANSL and BARDON of the National Science Foundation.

In case your polyhedric duties and responsibilities will allow you to answer at some future time of your convenience, I would appreciate whether you can send a courtesy copy of your letter also to the indicated officers.

In closing, permit me to express the sentiments of my most sincere esteem for your person and of gratitude for the consideration, courtesy and time you have already provided for me during other occasions. Under no circumstances you should identify in the open language of this letter my intention of being offensive. If I failed to realize this point, please accept my most sincere apologies.

As a personal note, permit me also to express my unlimited faith that the U.S. community of basic studies will indeed achieve the common objectives either as outlined in this letter, or in a more mature form resulting from the possible contribution by other collegues. After all, despite my past difficulties recalled earlier, I am happy to testify that Harvard University and the U.S. Department of Energy have given clear proof of scientific vision by allowing the conduction of my unconventional, conjectural and speculative studies at the frontiers of knowledge.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli

RMS|cgg

NOTE OF JUNE 1, 1984: ESSENTIALLY THE SAME LETTER WAS MAILED TO:

Dr. R. R. WILSON, DIRECTOR OF FERMILAB, and

Dr. G. H. VINEYARD, DIRECTOR OF BROOKHAVEN NATIONAL LABORATORIES

July 1978

RUGGERO MARIA SANTILLI

Biographical notes

Santilli, a member of the American Physical Society, the Italian Physical Society and the Society of the Sigma Xi, received his Ph.D. in physics at the Theoretical Physics Institute of the University of Torino, Italy, in 1966. He then was visiting scientist at a number of international institutions, including the International Centre for Theoretical Physics of the IAEA of Trieste, Italy, the Centre Europeenne pur la Reserche Nucleare of Geneva, Switzerland, the Institut Henri Poincaré of Paris, France, and the Center for Theoretical Studies of the University of Miami, Florida. From 1970 until 1976 he was in the faculty of the Department of Physics of Boston University, Boston, Massachusetts, where he left after promotion to associate professor of physics (without tenure). From January 1976 until August 1977 Santilli was visiting scientist at the Center for Theoretical Physics of the Massachusetts Institute of Technology. Since September 1977, Santilli is a research scientist at the Science Center of Harvard University and corecipient of the grant from the Department of Energy No. ER-78-S-02-4742.A000. It should be here indicated that Santilli's position at Harvard is of non-teaching, non-tenured and terminal nature.

Santilli is the author of some fifty papers published in various journals of theoretical physics and in differentiated topics. In axiomatic field theory Santilli performed the extension of the PCT theorem to all discrete space-time symmetries (Phys. Rev. D10, 3396 (1974)) a generalization of the Haag theorem to the first eight vacuum expectation values of scalar fields (Phys. Rev. D7, 2447 (1973)) and a U(3.1) analytic extension of vacuum expectation values (Nuovo Cimento 2A, 965 (1971)). In mathematical methods in theoretical physics, Santilli identified a covering of the Lie algebras called Lie-admissible (Nuovo Cimento 51A, 74 (1967) and Supplemento al Nuovo Cimento 6, 1225 (1968)) and pointed out their direct physical application in Newtonian mechanics via the brackets of the time evolution law for Hamilton's equations with external forces nonderivable from a potential (Meccanica 1, 3 (1969))as well as in field theory for couplings nonderivable from a Hamiltonian density (contributed paper in "Analytic Methods in Mathematical Physics", Gilbert-Newton Editors, Gordon and Breach (1970), proceedings of the Indiana conference of June 1968). In elementary particle symmetries, Santilli presented a derivation of Poincaré covariance from causality requirements in field theory (Int. J. Theor. Phys. 3, 233 (1970)), causality restrictions on relativistic extensions of internal symmetries (Int. J. of Theor.Phys. 2, 201 (1969)) and other contributions. In plasma physics Santilli identified the phase-space symmetries of a relativistic plasma (Nuovo Cimento 56A, 323 (1968)), worked out a Lie-admissible model wereby instabilities are linked to dissipative effects (Lett. Nuovo Cimento 2, 449 (1969)) and other contributions. In general theory of gravitation, Santilli presented an attempt of removing the problem of unification,consisting of the quantitative construction of the exterior gravitational field of hadrons with the sole use of the structure fields and proposed the experimental test of the prediction of current theories according to which any electromagnetic field is the source of the gravitational field via the use of available giant magnets (Annals of Physics, 83, 108 (1974)). In Classical Field Theory Santilli proved a theorem on the necessary and sufficient conditions for a system of second-order partial differential equations to admit a Lagrangian representation, via the implementation of the conditions of variational selfadjointness within the context of the calculus of exterior forms in general and the converse of the Poincaré lemma in particular; identified a computerazable method for the construction of a Lagrangian for the representation of systems with arbitrary local couplings, when its existence is guaranteed by the integrability conditions; and pointed out a number of applications of the underlying methodology to engineering via the optimal control theory, to space mechanics and missile control, nonconservative nonlinear plasma physics, Newtonian Mechanics, and high energy physics. This resulted in a methodology now known under the name of

the Inverse Problem of Classical Mechanics (Annals of Physics, 103, 354 (1977), 103, 409 (1977), 105, 227 (1977), MIT-CTP preprints Nos. 606, 607, 608, 609 and 610 (1977), also see below in the monograph section). In Newtonian Mechanics, Santilli made his first comprehensive contribution as a result of a project conducted since the times of his graduate studies, the construction of a generalization of the Galilei relativity for Newtonian systems which are nonconservative and Galilei form-noninvariant. The emerging covering of the Galilei relativity is nonrelativistic and classical and, as such, it is independent from existing relativistic and quantum mechanical extensions. This work therefore indicates the possible existence of generalizations of established relativity ideas for the case of forces nonderivable from a potential. The proposed covering relativity was the result of the previous identification of two complementary methodologies for the representation of the broader systems considered, that of the Inverse Problem and that via the use of the Lie-admissible algebras, the latter having the dominant constructive role (Hadronic Journal 1, 223-423 (1978), also, see below in the monograph section). In hadron physics Santilli made his most speculative contributions. He identified a possible physical origin of strong hadronic forces as being local but more general than the Lorentz force, that is, nonderivable from a potential, and then pointed out in details that for these broader structure forces the established relativity and quantum mechanical formulations are inapplicable. Therefore, he proposed the experimental test of established laws for the hadronic constituents as the crucial prerequisite for any meaningful orientation of effective studies on the structure. He then worked out the rudiments of a Lie-admissible quantization of strong hadronic forces nonderivable from a potential and of his covering of the Galilei relativity. Finally, he applied these broader formulation for the explicit construction of a structure model of mesons and the confrontation of the predictions with experimental data. It essentially emerged that if conventional laws are assumed as valid for the hadronic constituents as in current unitary trends, the known problematic aspects on the identification of the constituents with physical particles (or for their confinement) appear to be unavoidable. If instead, broader acting forces and covering Lie-admissible formulations are assumed, the hadronic constituents can be identified with physical particles directly produced in the spontaneous decays (Hadronic Journal 1, 574-901 (1978)).

Santilli is also the author of a number of research monographsand lecture notes. Springer-Verlag, Heidelberg, is currently printing in the series "monographs in physics" the works Foundations of Theoretical Mechanics, Volumes I and II. Hadronic Press Inc., Nonantum, Ma, is printing the series Lie-admissible approach to the hadronic structure, Volumes I, II and III. Santilli's lecture notes were printed in Italian by the University of Torino (on Lie algebras) and by the Institute A. Avogadro of Torino (in nuclear physics).

Santilli has a number of years of teaching experience in prep courses (in Italy), undergraduate courses (in Italy and at Boston University), graduate courses (at Boston University on Classical Mechanics, Quantum Mechanics, mathematical methods in theoretical physics and Calculus of Variations) and seminar courses (at Boston University on Hadron Physics, Regge pole theory, and Interacting systems with nonintegrable subsidiary constraints). His last seminar course has been delivered at Harvard University in the fall term of 1977 on the methodology of the Inverse Problem and its applications.

Santilli has conducted referee work for a number of journals, including Phys. Rev., J. Math. Phys., Annals of Physics, J. of Physics, Nuovo Cimento and others. He is the founder and the Editor in Chief of the Hadronic Journal.

Santilli is married with two cildren age 9 and 11. He is a permanent resident of the States since 1967. Santilli's extracurriculum interests are also varied. Among other activities, he has been chairman of the Board of Directors of a Massachusetts Corporation for four years (from 1970 until 1974).

Ruggero Maria Santilli

Foundations of Theoretical Mechanics I:

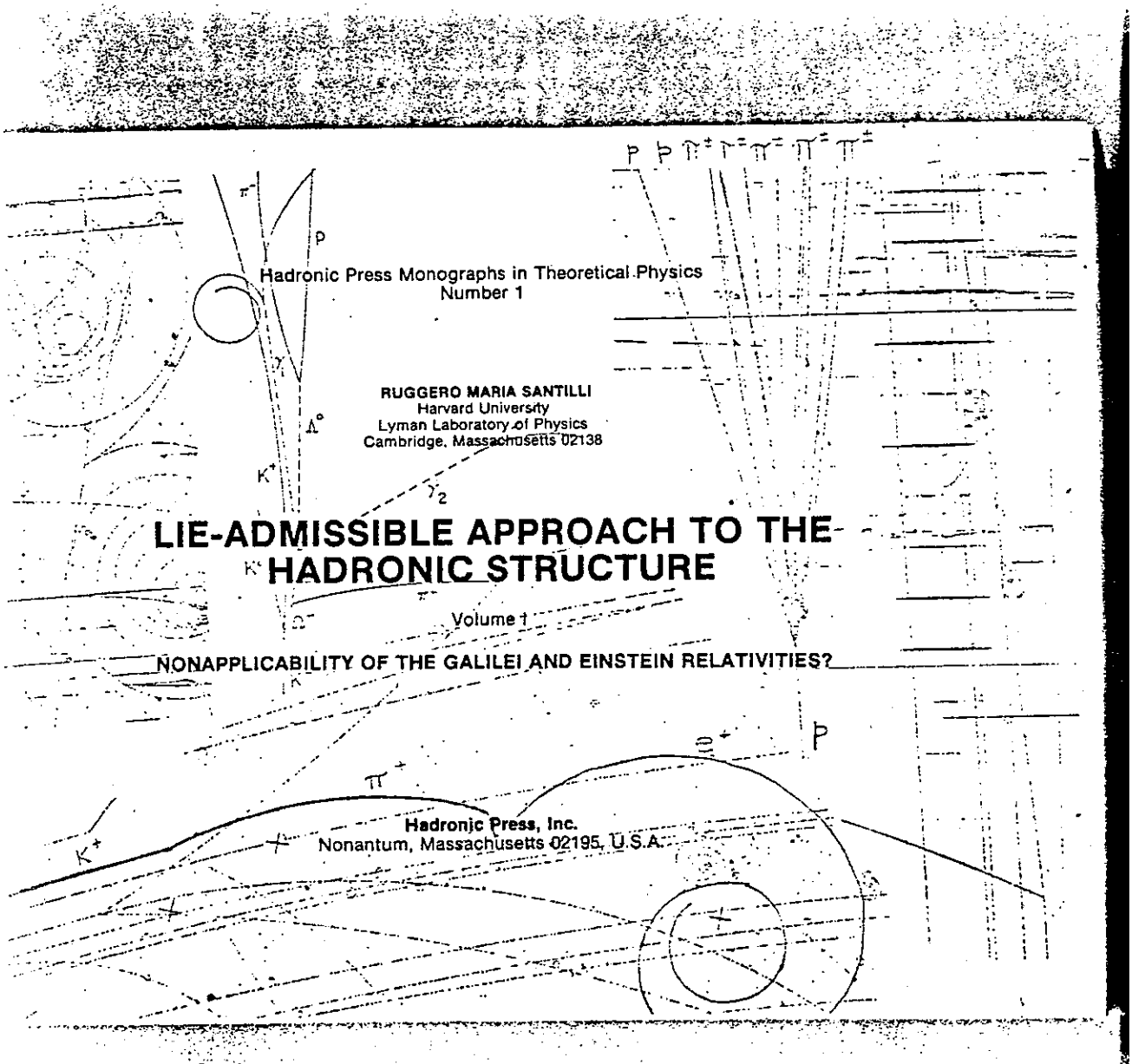
The Inverse Problem in Newtonian Mechanics

Texts and
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W. Beiglböck
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Springer-Verlag
New York Heidelberg Berlin



Hadronic Press Monographs in Theoretical Physics
Number 1

RUGGERO MARIA SANTILLI
Harvard University
Lyman Laboratory of Physics
Cambridge, Massachusetts 02138

LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE

Volume I

NONAPPLICABILITY OF THE GALILEI AND EINSTEIN RELATIVITIES?

Hadronic Press, Inc.
Nonantum, Massachusetts 02195, U.S.A.

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

July 19, 1978

Professor JOSEPH BALLAM,
Associate Director of Research
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Ballam,

I enclose a complimentary copy of the second issue of June of the HADRONIC JOURNAL. You will be pleased to know that the journal is picking up momentum as a result of its formula. As you know, we provide the fastest possible distribution of important papers without regards to length (our current average between reception and distribution of articles is 45 days) as well as without publication charges. Also, the journal might well become, in due time, that with the highest percentage of rejection in the trade owing to a number of factors, such as (a) our lack of interest in minute incremental contributions in established trends which are rerouted to other journals; (b) our emphasis in thought provoking, potentially innovative papers in relevant issues; as well as (c) a limitation on the number of papers per issue we have to comply with.

In this latter issue we have implemented a promising, but delicate step, as presented in details in the enclosed letter of July 19, 1978 to Professor W. K. H. PANOWSKY.

Your consideration of this material would be very much appreciated. Your advice and guidance in the future orientation of the journal would be gratefully acknowledged. In particular, please let me know whether you want to personally inspect possible future papers, prior to their publication, on the intriguing debate under way: validity or invalidity for the strong interactions of the experimentally established knowledge for the electromagnetic interactions.

Please keep notice of my new address for the forthcoming academic year, as given above.

Sincerely

Ruggero Maria Santilli

RMS|ts

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

July 19, 1978

Dr. H. D. I. ABARBANEL,
Theoretical Division
FERMILAB
BATAVIA, Illinois 60510

Dear Dr. Abarbanel,

I acknowledge receipt of your recent communication to the effect that my application for a research position at the Theoretical Physics Division of Fermilab for the study of the problem of the experimental verification of Pauli's exclusion principle and Eisntein's special relativity for the hadronic constituents, had been declined.

I sincerely hope that this refusal was the result of insufficient funding or other reasons not related to the topic of my studies. In any case, you can rest assured that I understand and respect in full such decision.

I do feel obliged, however, to clearly and openly express my utmost concern on the current conduction, operation and policy of the Theoretical Division of FERMILAB. I believe that this division is:

- monopolistic, in the sense that it has only conducted research based on the conjecture that quarks are the constituents of hadrons;
- unbalanced, because of the literal lack of diversification of studies on the fundamental problem of contemporary physics; and
- of marginal effectiveness, in the sense that the virtual entire theoretical production on the problem of hadron structure conducted in this division in recent times is devoted to minute aspects along mere opinions by groups of physicists, without any direct consideration of truly fundamental physical problems.

For more details on my view, you may consult my recent letter to Professor WILSON, copy of which is enclosed.

I would like to stress that this candid expression of my concern is not related to the negative decision on my application. To understand this, you must realize that I have written similar letters also to other Institutions in which I have been invited with full support. Also, just while receiving your letter, I was in the process of writing you to ignore my application. I hope you understand that I simply do not have time to visit your division now, owing to my research committments with a grant from a U.S. governmental agency, my contractual committment with Springer-Verlag and Hadronic Press to release for printing a number of research monographs, my duties as Editor in Chief of the fast growing HADRONIC JOURNAL, my accademic involvement at Harvard, etc.

Very Truly Yours

Ruggero Maria Santilli

c.c.: Professor R. WILSON, Fermilab.

RMS|cgg

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

July 19, 1978

Professor SIDNEY D. DRELL, Deputy Director
Stanford Linear Accelerator
STANFORD, California 94305

Dear Professor Drell,

Permit me the liberty of respectfully, but candidly expressing my irreconciliable disagreement with the content and inspiration of your article in the June issue of PHYSICS TODAY entitled "When is a particle?".

In essence, I would have accepted your article in its entirety if inspired by the clearly stated objective of presenting only one plausible view on the problem of hadron structure. On the contrary, in reading the article I reveiced the perhaps erroneous feeli ng of expression of your personal viewpoint that the quark conjecture is the only possible approach to hadron structure and of your personal epistemological efforts to justify it, which I consider unproductive as far as basic research is concerned, particularly in relation to young readers.

In case you are interested to my viewpoint, I enclose copy of a letter of July 19, 1978 to Professor W. K. H. PANOFSKY, which is precisely intended as a candid expression of criticisms toward the currem monopolistic attitude of quark supporters. In case you are interested in technical profiles, I enclose copies of my papers in this topic.

Almost needless to say, any criticism would be received with noting but sincere gratitude.

In closing, permit me to confess that the writing of this letter has been for me reason of considerable regret. Via your invaluable prior writings, you have been one of my theachers of theoretical physics. I have noting but sentiments of sincere esteem in your person. Therefore, your should not identify, under any circumstance, my intention of being offensive. I simply felt a moral obligation of candidly expressing my viewpoint to you on this controversial issues of the ever increasing plurality of unidentified, different quarks, in the hope of only stimulating a moment of reflection.

Very Truly Yours

Ruggero Maria Santilli

RMS|cgg

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

July 27, 1978

Dr. Ruggero Maria Santilli
Science Center, Room 331
1 Oxford Street
Cambridge, Mass. 02138

Dear Professor Santilli:

Thank you very much for your letter of July 19, 1978 which I read with great interest. Let me say first that I do not view the situation in hadron physics as being as grave as you indicate. On the contrary speaking as an experimentalist, I am impressed by the recent, almost explosive evolution of new experimental information and it comes as no surprise to me that the theoretical fraternity is not united in its approach to dealing with this new flood of data.

You are criticizing the fact that the majority of theoretical physicists are focusing their calculations bearing on these recent results on variants of a quark model, with particular emphasis being put on the question of confinement. You point out correctly that this assault by the majority is going on, notwithstanding the fact that the validity of some of the fundamental laws, in particular quantum mechanics and some facets of relativity, have not been unambiguously established on the hadron scale. I don't see why this should be a surprise to you; after all, quantum mechanics has been and is being applied constructively on an atomic scale, notwithstanding the fact that controversy persists and people continue to write and publish papers on the axiomatic basis of quantum mechanics and diverse interpretations of that basis.

You complain that the majority effort of theorists is going in a direction not meeting your sense of priorities. Scientific interest, in particular by theorists, cannot be directed but should be triggered by the challenge of the results themselves. This is a task for you to undertake through your work in research and publication, and not by appeal to government agencies, editors or laboratory directors.

I am not a theorist. Speaking as an experimentalist I feel that you profoundly misinterpret both the experimental status of elementary particle physics and the methods of conducting experimental investigation. You criticize that "current experimental trends are essentially restricted to the identification of new particles and their data in symbiotic conditions with unitary models." Most experimentalists would be highly astonished in seeing their work described in this manner. Experimental work is largely

conditioned by technological opportunity and is not as programmatic or directed as your description implies. The striking discoveries of new particles, states, and cross sections have indeed been interpreted by the majority of theorists along the lines you indicate but this does not mean that this has been the directed motivation of experimentalists. Moreover, there have been extensive experiments not meeting your description at all, such as the thrust to explore the limits of quantum electrodynamics, the exploration of violation of parity in electromagnetic interaction, the examination of jet structure in fundamental annihilation processes, etc., etc. The fact that it is your desire to have unambiguous proof of the validity of quantum mechanics on the scale of individual hadrons does not mandate that it shall be possible for an experimentalist to design an experiment which will unambiguously answer that question. What specific experiment(s) do you propose?

You are contrasting the current situation in physics as being unfavorable relative to that pertaining in the early part of the century. I disagree. I venture to suggest that you have read few, if any, of the experimental papers which were published in the scientific journals prior to the discovery of quantum mechanics. The Ruggero Maria Santilli of the first part of the century would have accused the experimentalists of sponsoring experimental fashions restricted to the identification of new spectral lines, or the investigation of uninteresting atomic phenomena, in disregard of fundamental considerations. Yet it was the accummulation of large masses of spectroscopic data and other atomic physics experiments of those days which led to the formulation of quantum mechanics in three alternate forms, which were then later shown to be equivalent. However, as I mentioned before, even to this day complete understanding on the most profound level of the basis of quantum mechanics remains a subject of some controversy even though it has formed a highly successful calculational basis of all the physics as uncovered to date.

To summarize, I see little merit in your arguments. You are clearly entitled to your opinion that the majority of your theoretical colleagues are pursuing goals less important than those represented by your own scientific interests. On the other hand, I feel your letter reflects a complete misunderstanding of the role of the experimenter in pursuing his work and I believe that most of your theoretical colleagues are ahead of you in appreciating the enormously fundamental importance of the recent experimental discoveries in hadron physics.

With best wishes,

W. K. H. Panofsky

cc: Dr. J. Kane
Dr. B. Hildebrand
Dr. D. Peaslee
Dr. W. Wallenmeyer
Dr. J. Deutch
Dr. J. Krumhansl
Dr. M. Bardon

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

July 31, 1978

Professor W. K. H. PANOFSKY
Director
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Panofsky,

I would like to express my sincere appreciation for the courtesy of your letter of July 27, 1978. I am also grateful for the "benevolent impunity" I am under the impression you have granted me for candidly expressing my concern.

I am in agreement with most of your letter and, in particular, with your remarks related to the experimental profile. There is no doubt among theoreticians that the situation of experimental hadron physics is in no grave situation at all. As a matter of fact, I am an admirer of current experimental results, to the point that I consider experimental hadron physicists considerably ahead of theoreticians. You have misinterpreted my letter to you of July 19 if you believe that I do not appreciate the fundamental relevance of recent experimental discoveries. The experiments you indicate on the limits of quantum electrodynamics, violation of parity in electromagnetic interactions and jet structure in annihilation processes, were completely extraneous to the content of my letter, which was solely devoted to experiments of direct relevance for the problem of hadron structure. It is understood that such a notion is debatable and that any experimental data on hadron is useful for the structure problem. I simply attempted to present in my letter the experimental profile which, in my view, can really produce an unequivocal orientation of theoretical studies on hadron structure from a fundamental profile, that of the basic physical laws.

Being a theoretician, the primary area of my concern is restricted to the current status of theoretical studies on the problem of hadron structure. It is understood that this is also a debatable notion. Indeed, it demands a differentiation between the problem of classification and structure. It appears that the consciousness of this possible differentiation is simply lacking at this time, to the best of my knowledge. For the sake of clarity, permit me to restress that, in my view, the unitary models have achieved a Mendeleev-type classification of hadrons of virtually conclusive character. The situation in regards to the quark models of hadron structure is, also in my view, fundamentally different. But your letter is silent on the problematic aspects of current theoretical studies.

Permit me, however, to disagree with certain parts of your letter and, in particular, with your objection of my appeal to laboratory directors and governmental agencies. The central point of my letter was to attempt to create an awareness on the need to subject to a critical scrutiny, theoretical study and experimental finalization the fundamental physical laws used in current hadron physics. You are eventually aware that these laws are simply assumed as valid in the virtual totality of the rather vast literature on quark models in a fully tacit form. A problem of this nature cannot be resolved by individual researchers and demands, in my view, the participation of the physics community at large. I strongly oppose the idea that

laboratory directors and governmental agencies should be excluded by scientific debates of orientational nature on fundamental issues.

I believe you have answered negatively the crucial question I have posed to you:

> "Do we possess at this time direct, explicit and unequivocal experimental evidence that the basic physical laws of the electromagnetic interactions (Einstein's special relativity and Pauli's exclusion principle, in particular) are verified for the hadronic constituents?

Permit me to confirm the answer as suggested to me by a number of experimentalists: NO! This literally implies that the rather massive theoretical efforts during the last decade on the problem of hadron structure are based on experimentally unverified, fundamental physical laws. You add to this my exposure, as editor, to an apparently mounting study on the invalidity of these laws within the arena considered (which I felt obliged to report to you). I believe that there is sufficient reason for concern. But you see little merit in the consideration of the issue.

Permit me to confess that my personal concern is increased, rather than decreased, by your letter. Indeed, I am under the perhaps erroneous impression that you do not see the need of reorienting the current experimental trends for the primary objective of achieving the direct, explicit and unequivocal experimental resolution of the validity or invalidity of the basic physical laws for the hadron structure. There is no doubt in my mind that, until this problem is confronted and, in due time, resolved, the problem of hadron structure will remain fundamentally unresolved and all theoretical efforts will remain in limbo. But this this is a problem which cannot be effectively resolved at the level of the individual, theoretical or experimental researcher only, and demands the joint consciousness at the decisional level. My expectation was that the participation, council and wisdom of laboratory directors and officers of governmental agencies in the decisional process would be invaluable.

In relation to the specific tests you refer to, I have proposed only one test at this time: the experimental verification whether Pauli's exclusion principle is exactly valid for $_7N^{15}$ and other nuclei, or very small deviations at this nuclear level are experimentally detectable. There exists a number of proposals in the available literature in relation to specific tests of Einstein's special relativity for the hadronic structure by other physicists (see, for instance, ref. 1, page 883 of the HJ and related literature which I see no point in recalling here in details). Additional specific tests are apparently forthcoming in future issues of the HADRONIC JOURNAL, as well as in other Journals, to provide even a wider selection for the interested experimenters.

As a matter of fact, permit me to disclose that the primary reason of my concern (as well as of the candid language of my letter to you) is precisely due to the fact that these proposals for the experimental verification of fundamental physical laws within a hadron have been made a number of years ago, but, despite their fundamental character, they have remained ignored until now, to my knowledge. From your letter I am under the impression that they may remain ignored for a number of years to come.

Very Truly Yours

Ruggero Maria Santilli

RMS|cgg

c.c.: Drs. J.DEUTCH, J.KANE, B.HI LDEBRAND, D.PEASLEE, W. WALLENMEYER of DOE

Drs. J. KRUMHANSL AND M.BARDON of NSF.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

August 8, 1978

Professor JOSEPH BALLAM,
Associate Director of Research
Stanford Linear Accelerator Centre
STANFORD, California 94305

Dear Professor Ballam,

it is a pleasure to invite you for an informal workshop on the problem of the physical laws for the strong interactions which will be held here on August 24 and 25.

I am sorry for the insufficient notice, but I received only today the acceptance (and encouragment) by Ktorides (from Greece) and Myung (from IOWA). It is also with regret that I must acknowledge my inability to provide support for any of the participants. My grant with Shlomo Sternberg is virtually restricted to my salary and extremely few items, while the situation for university funds is predictably equivalent.

I enclose an outline of the meeting with additional information. From the indications available, it appears that it could be intriguing indeed. As you can see, we did and we will carefully avoid matter of minute incremental nature, while we are interested in though provoking speculations on fundamental issues. Also, we have avoided topics of direct structural character for hadrons. Thus, topics such as quarks, partons (and eletons) are not expected to play a fundamental role. This is also in line with our view, by now familiar to you, that we should first look at the laws for strong interactions, and then at the problem of structure.

I would like to add that the workshop is a truly working-research session and not a conference. H.C.Myung has some new, quite valuable results in Lie-admissible algebras (the August issue of the HADRONIC JOURNAL will publish his second, quite long paper-monograph on Lie-admissible algebras of nonflexible type -- the first written by a mathematician on this topic). C.N.Ktorides (who, as you eventually know, is an axiomatician) has some recent arguments on the possible inapplicability of the spin-statistics theorem for the strong interactions which I consider intriguing. Stephen Adler has a quite intriguing approach to classical chromodynamics and I hope he can attend. I keep myself busy with my interest in attempting a Lie-admissible covering of Einstein's special relativity (but it will take some time before I can publish anything). D.Y.Kim has some specific proposals for experimental tests (partly his and partly review of proposals by others). Regrettably, he cannot attend because of lack of funds, since he is now in Cambridge, England, on sabbatical. But, perhaps, we can expect some communication by him.

I believe that your participation would be invaluable, not only for the scientific aspect per se and for the possibility of knowing each other, but also for the future of our Journal.

Please kindly extend this invitation to Professor Panofsky and Drell.

Sincerely

Ruggero Maria Santilli

RMS|cgg

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

August 2, 1978

Professor W.K.H. PANOFSKY,
Director
Stanford Linear Accelerator Center
STANFORD, Ca 94305

Dear Professor Panofsky,

As a follow up to our recent correspondence, permit me to express my apologies in case I have been of any inconvenience to you.

Also, I would like to indicate that I have no intention of doing any follow up and, from now on I shall behave, shall I say, like "a good boy". After due consideration, I simply felt the need to express my sincere concern on theoretical hadron physics for whatever its value is, via my letter to you and my papers. From now on I would like to abstein from any direct partecipation in possible debates outsides regular publications on the rather controversial quark conjecture. If my seeds have any value, they will grow in due time via an orderly and conventional scientific process.

Permit me to close with a humouristic note. I enclose two "vignette" depicting certain crucial aspects of current theoretical physics. They have amused (although, I believe, left skeptical) my Lyman collegues. I present them to you in the hope that you will smile.

With my warmest regards, I remain

Sincerely Yours

Ruggero Maria Santilli

RMS|cgg

C.C. Professor S. Drell and J. Ballam (only).

INTERNATIONAL ATOMIC ENERGY AGENCY
UNITED NATIONS EDUCATIONAL, SCIENTIFIC AND CULTURAL ORGANIZATION

INTERNATIONAL CENTRE FOR THEORETICAL PHYSICS
34100 TRIESTE (ITALY) - P.O.B. 586 - MIRAMARE - STRADA COSTIERA 11 - TELEPHONES: 224281/2/3/4/5/6
CABLE: CENTRATOM - TELEX 46392 ICTP

DIRECTOR
ABDUS SALAM

10 August 1978

Dear Santilli,

I am deeply grateful for your kind letter and the thought-provoking enclosures. They deserve a very careful study and a careful answer. Regretfully, I am just now writing up the Tokyo talk and will then go to Tokyo, China, Korea - and even the US (Ohio 27,28 and 29 September) - most of the journey is in aid of keeping the Centre here alive and well. (Incidentally I appreciate your kind remarks about the Centre.)

Is there any chance of our meeting in the US or here and talking about these matters? If not, I shall write in detail later.

With my appreciation, again, for your thoughtful and stimulating letter.

Yours sincerely,

Abdus Salam

Professor R.M. Santilli
Harvard University
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138
USA

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

August 18, 1978

Professor ABDUS SALAM, Director
International Centre for Theoretical Physics
34100 - TRIESTE, MIRAMARE, Italy

CONFIDENTIAL

Dear Abdus,

I would like to express my sincere appreciation for your understanding attitude in relation to my letter of July.

In actuality, the occurrences which preceded my letter to Professor Panofsky were considerably more alarming of what disclosed in my preceding letter to you. In essence, it was my understanding that some collegue had reached such a level of disappointment, to hire attorneys for legal actions on research-grant related issues. An action was needed to prevent events of unpredictable outcome.

I was in a rather unique situation because I am the recipient, jointly with a distinguished mathematician of Harvard, of what appears to be the first research grant from the Department of Energy which is specifically intended to be of non-quark orientation, that is, an attempt at hadron structure other than of unitary character.

It appears that I did succeed with my letter to reach the objective intended. Indeed, I did succeed in full to quite down excessive disappointments and delay unnecessary action. My only disappointment is that I do not know whether I was able to preserve my good relationship with Professor Panofsky. Indeed, all this background is entirely unknown to him, to my knowledge. At the same time, the writing of the letter in question was for me reason of considerable regret (although it was absolutely necessary in my view), because I have a sincere esteem for Professor Panofsky as well as a sincere gratitude for the assistance I received from him during the organization of the HADRONIC JOURNAL.

In any case, it appears that the wheels for achieving a more balanced conduction of research in hadron structure are in motion. As you know, the proposals originate from quite responsible physicists. They desire that primary funding along unitary trends must continue and that, jointly, a minor funding of possible alternatives should be initiated. Recent events were due to the truly pre occupaying level of monopoly in fundings of the unitary trends. In any case, I am happy to see that, after my letter (which, as you know, was mailed also to Governmental Officers), I did receive for refereeing research grant proposals of non-quark inspiration, with indication of a serious intent of consideration. This was very welcome indeed. It gave me additional essential elements to quite down spirits, by indicating that the wheels for a sincere desire to improve the dispersal of research funds were in motion. In the final analysis, these responsible collegues wanted noting more.

In conclusion, I am happy to report to you that, to my knowledge and understanding, things are back to normal. Almost needless to say, I do not intend in the future to enter into disputes related to fundings of quark-oriented research. I did it once and I think I should not do it again.

I am grateful for your kind consideration of my studies. As I have formally presented them, they are "an exercise of scientific curiosity" intended for the primary purpose of calling contributions from collegues on the problem of the basic physical laws for the strong interactions.

Again, I am warmly encouraging you to an active participation in this issue, by presenting your viewpoint on the issue, possibly, either in favor or against established laws. I believe that your knowledge and wisdom would be invaluable to achieve a resolution of the issue. Almost needless to say, at the HADRONIC JOURNAL we are solely interested to the pursuit of the scientific truth and not to that of specific insights. As a result, I beg you to feel completely free to criticize my papers (as well as those of the other collegues) in the form and substance you desire.

Thank you for your interest in a verbal discussion of these matters. Regrettably, I am unable to come to Europe for numerous committments. Neverthless, since you will be in the States in September, It would be very nice indeed, if you could spend a brief visit at Harvard, either before the Ohio trip or after (you could return to Italy via Boston, Logan Airport-direct flight to Milano).

In the event that this is indeed possible, I have enclosed a formal invitation from the Department of Mathematics (I am a member of this Dept). I am sure that the Lyman collegues would welcome you too.

There is a feverish activity going on in relation to the study of a possible generalized nature of the strong hadronic forces. Next week we will have here at the Dept. of Math. an informal workshop on Lie-admissibility (Ktorides from Greece, Myung from Iowa and few other will attend). Intriguing, but delicate papers are also forthcoming on the Hadronic Journal. I shall provide my best efforts to keep you informed of relevant events.

I have no words to express my esteem and admiration for your person and your accomplishments.

Sincerely

Ruggero Maria Santilli

RMS|cgg
encl.

THIS LETTER
REMAINED UNACKNOWLEDGED

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

Directors Office

September 27, 1978

Dr. Ruggero Maria Santilli
Harvard University
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

This is an apology for not having answered your thoughtful letter of last July. Somehow in the confusion of my stepping down as Director of Fermilab, the letter, because of its format, ended up in my non-personal file.

The kind of questions you have raised are not answered easily, and since I am no longer the Director, there seems little point in trying. You do make some pretty harsh charges regarding our Theory Department. Generally speaking, we have tried to hire the best people available based on the advice of the best theorists in the country. A broad range of theorists come to visit Fermilab for various periods to supplement the efforts of the Fermilab theorists. Having done that, as Director, it would never occur to me to try to influence or restrict their work. Although the tragic death of Ben Lee set us back, I have been satisfied with and proud of our theoretical department.

The theorists do not determine the experimental program at Fermilab except as they are able to influence on a logical basis the experimenters themselves. We have a Physics Advisory Committee with a broad membership from all parts of the country and which represents many specialists including theorists. Again those theorists and some of our own join in the debate that leads to the eventual decisions. The constant flux in the membership of the PAC means that many points of view have been represented.

It is by influencing the physics community directly with your arguments, as you are appropriately trying to do by your letters and with your new journal, that you should expect your views to have an effect.

With regard to your various specific suggestions and questions, it seems more appropriate that I turn your letter over to my successors, so that they may consider them.

Again, may I apologize for not having answered your letter more promptly.

Sincerely,

R. R. Wilson

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

STRICTLY CONFIDENTIAL

October 2, 1978

Dear Professor Wilson,

I simply have no words to express my appreciation for the courtesy of your letter of September 27, as well as for the "benevolent impunity" you have apparently granted to me in relation to the content of my letter of July 19, 1978.

Almost needless to say, I am entirely in agreement with the content of your letter, to the point that I have no further or counter-comment.

What I would like to indicate to you on a confidential basis is the background which lead to my letter to you. I am sure you have sensed that my letter was not the result of a one day decision. In actualy, it was the climax of a series of events which left me no other alternative to serve our community in the form which appeared to me as more appropriate under the circumstances.

I am sure you are aware of the delicate situation of our community owing to insufficient funding, insufficient openings, lack of tenure, etc. This situation has apparently exasperated spirits. A number of physicists have lost tenure, jobs etc. allegedly because of lack of funding of their research proposals. Out of this tense moment a specific accusation was reported to me, namely, that of a scientific and (most insidiously) financial monopoly in the sector of theoretical hadron physics by quark oriented studies.

Additional events urged me to attempt a PREVENTIVE action, that is, the prevention of gestures which could be detrimental for our community because could create shadows on the ethical profile of funding in the sector. My letter to you was conceived and used as such preventive tool. I am happy to report to you that I did apparently succeed in calming down excessive malcontemt. The reason is that our malcontent collegues are, in my view, quite responsible physicists. It is the irresponsible (also in my view) behaviour of other collegues currently "under the money tree" which has created this situation. In essence, my letter has stimulated an orderly scientific process of consideration of the issue, thanks also to the invaluable partecipation by Dr. Krumhansl. This was fully sufficient for the objective at hand.

I would appreciate the courtesy of your conveying this background to the new Director of FERMILAB (I do not know at this moment his name). Please also feel free to show him this letter with the understanding that it should remain strictly confidential. There is no need to answer to my letter. If the new director feels that an asnwer is due, I would appreciate the indication of the main point, namely that the points raised are under an orderly consideration.

What I am more interested is in the possible, direct involvement of FERMILAB in the rather exiting things going on as a result of this "vigorous call for a moment of reflection on quarks". In short, it appears that a number of experimental physicists are seriously considering the undertaking of an initial feasibility study for the experimental test of Pauli's principle in nuclear physics (according to my proposal of the H.J. 1, 574 (1978)) as well as of the Special Relativity in hadron physics (see, for instance, ref. 1 and quoted paper of same reference).

Almost needless to say, these are noting but initial steps of a rather formidable experimental problem. Some of the initial results or proposals will be (proudly !) presented in our Journal.

Also, these studies are due to courageous physicists who have been capable of overcoming what I call the "QUARK SINDROME", that is, the extreme repugnance at the mere idea that QCD, in the final analysis, could be experimentally proved to be invalid via a possible experimental verification of the inapplicability of the Pauli's principle and the spin-statistics theorem under strong interactions.

Permit me to close with a candid confession in relation to the theoretical division of FERMILAB. It is that, being an Italian, I am emotionally attached to the Laboratory bearing the name of ENRICO FERMI. As such, more than for other Laboratories, I am sincerely interested in yours being at the frontiers of the pursuit of physical knowledge, both theoretically as well as experimental because, as you know, this was the rather unique dual quality of Fermi.

In relation to the fundamental problem of current theoretical and experimental physics, hadron structure, permit me to acknowledge my emotional state in seing the studies only restricted to the quark conjecture and numerous (if not all) experiments essentially interpreted only via such conjecture.

The appeal of my letter to you which was genuinely felt is to consider the implementation of a more balanced conduction of research consisting of the necessary continuation of studies along quark lines, jointly with the beginning of the inclusion of fundamentally different approaches.

My peculiar position of Editor of a Journal already known as devoted to the nonmonopolistic presentation of research on hadrons, as well as being the recipient of what appears to be one of the first research grants of non-quark inspiration, has exposed me to a number of situations which were perhaps unknown to you. I am here referring, for instance, to a strong increase in very recent times of highly qualified physicists who, after so many years of yet inconclusive efforts, simply do not believe in quarks. All these indications suggested to me the proposed implementation of research into a well balanced form of quark and NON-quark inspiration. Quite candidly, I do see this implementation (or at least its orderly study) as a necessary prerequisi to avoid a substantial, potential deterioration of the current situation. This was, after all, the intend of my action.

In closing, I would like to beg you not to consider my letter as offensive for your conduction of FERMILAB. You have been Director of this laboratory in one of the most difficult moment of its history until now. You have my unconditional esteem for the sole courage you proved to possess simply in holding this post. Also, you have acquired the respect of our entire community via appropriate warnings on the current situation. Under no circumstances you should consider my letter as a form of criticism for the past, especially in view of the fact that certain informatbon was still unknown to you. As a matter of fact, I have selected you for my call for a moment of reflection precisely because fully aware of the fact that my letter would leave your scientific, ethical and human stature entirely unaffected.

Hoping to have the pleasure of meeting you some time, I remain

SINCERAMENTE TUO

~~Sincerely Yours~~

Ruggero Santilli

Ruggero Maria Santilli

State University of New York
at Stony Brook
Stony Brook, New York 11794

StonyBrook

Institute for Theoretical Physics
telephone: (516) 246- 6701

September 26, 1978

Professor Ruggero Maria Santilli
Harvard UniversityScience Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Prof. Santilli:

Could you please send me a copy of your letter to Panofsky of July 1978?

Yours sincerely,

C. N. Yang

Chen Ning Yang

CNY:ct

c.c. Prof. Gursey

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

September 18, 1978

Professor CHEN NING YANG
State University of New York
Stony Brook, N.Y.

and

Professor JOSEPH BALLAM
Stanford Linear Accelerator Center
Stanford, Ca

Dear Professors Yang and Ballam,

Following a meeting with HOWARD GEORGI, I would like to take the liberty of providing you with additional comments in regards to my letter to WOLFGANG PANOFSKY of July 1978.

I am sure you have realized the great emotional state which brought me to this letter. It was due to the current condition of our community of basic research which appeared to me as quite delicate and, as such, demanding a call for a moment of reflection. I do not know whether my means have been proper or improper, but I can assure you that my intentions were solely devoted to the interest of our community. Also, permit me to reassure you on my unconditional loyalty to Governmental Agencies (after all, I am completely supported by research funds).

In any case, it appears that I have achieved my objective of quieting down excessive malcontent. To the best of my knowledge, it appears that an orderly scientific process has been implemented (e.g., a consideration of the situation by the NSF Advisory Committee). As you also know, I have made it clear to all parties that I do not intend to provide another intermediary action, as far as the future is concerned. As a matter of fact, I believe that once the situation has been properly studied, it will be improved and no such action will be needed.

I hope that my letter, written as an individual physicist, will not affect the HADRONIC JOURNAL. In any case, it was not intended to express the viewpoint of other independent members of the Editorial Organization of our Journal.

As you know, the Journal is devoted to the presentation of a primary line of mature papers on quark models. This line is under the complete, independent and final editorial control by Howard Georgi. The Journal then presents a secondary line of speculative papers on possible alternative approaches to hadron structure under my supervision. Finally, the Journal presents a third line of non-conjectural, rigorous papers by mathematicians on techniques of potential interest to hadrons.

I believe that this structure fullfils a precise function in relation to the delicate moment of our community. Indeed, as indicated in my preceding communications to you, our community is under an apparent accusation of an alleged scientific and financial monopoly by quark-oriented studies in the sector of theoretical hadron physics. I believe that the HADRONIC JOURNAL is the best evidence of the lack of existence of such an alleged monopoly, at least for its scientific part. I sincerely hope that the Journal can continue to benefit of your council and advice, particularly in this delicate moment of our community, so that it can continue its function.

In case I can be of any assistance for additional information, please do not hesitate to contact me (Office 617 495 33 52, home 617 969 3465).

Very Truly Yours

Ruggero Maria Santilli

c.c.: Prof. H. GEORGI

RMS|cgg

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 15, 1978

Professor CLIFFORD WILL
Department of Physics
Stanford University
STANFORD, California 94305

Dear Professor Will,

As a follow up to my recent letter to you, I enclose copy of a review-comment by Professor D.Y.KIM (now at Cambridge-England) on the problem of the experimental verification of Einstein's relativity at small distances.

I would appreciate the courtesy of an indication whether you can participate in this scientific effort by presenting your assessement of the state of the art.

Sincerely Yours

Ruggero Maria Santilli
HADRONIC JOURNAL

RMS/cgg
encl.

P.S. Please read first, for your amusement, the very last sentence of Kim's paper.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

February 28, 1979

Professor WOLFGANG K. H. PANOFSKY, Director
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Panofsky,

You might be amused to know that the enclosed flier has been printed and distributed in 60,000 copies to the mathematics and physics communities.

I always remember you with sincere pleasure and gratitude.

Very Truly Yours

Ruggero Maria Santilli

RMS/se

HADRONIC PRESS presents the reprint series

APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS

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Department of Physics and Astronomy
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and

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Volume II (1979), 595 pages, $55.00

Excerpts from the prefaces of these volumes

The Lie-admissible algebras constitute a generalization of the Lie algebras and, as such, have a far reaching mathematical potential. Their study was initiated by A. A. ALBERT in 1948, . . . but it has been only sporadically pursued by mathematicians until now.

The recent surge of interest in the Lie-admissible algebras appears to be due to the intriguing possibilities of physical applications. It was first pointed out by R. M. SANTILLI in 1967 that the Lie-admissible algebras arise in a natural way in Newtonian Mechanics via a generalization of Hamilton's equations for the representation of forces nonderivable from a potential. Since that time, a number of physicists have made contributions which have brought to light other applications. These include: the treatment of open systems in continuum mechanics; the quantum mechanical descriptions of forces nonderivable from a potential; the characterization of broken Lie symmetries and supersymmetries, etc. . . . Strong interactions at both the nuclear and hadronic levels have long been suspected of being nonderivable from a potential, as an approximation of nonlocal settings. Thus, the Lie-admissible algebras have recently emerged as being potentially significant for nuclear and hadron physics, in general, and the problem of the structure of hadrons, in particular.

Owing to these possibilities, a considerable effort to promote the study of the Lie-admissible algebras in both mathematics and physics has been implemented via the HADRONIC JOURNAL since its first issue of April 1978.

A primary objective is to appeal to the most advanced possible mathematical tools to study, formulate and promote the experimental verification of the validity or invalidity for the strong interactions of fundamental physical laws, such as Einstein's special relativity, Pauli's exclusion principle and the spin-statistics theorem, which are experimentally established until now only for the electromagnetic interactions. It is understood that the problem of the structure of hadrons can be confronted in a truly effective way after the achievement of these basic experimental resolutions.

In summary, the papers presented in the first two volumes of this series are sufficient to indicate the existence of a hierarchy of Lie-admissible algebras of increasing mathematical complexity, such as the Lie algebras (which are trivially Lie-admissible), the flexible Lie-admissible algebras and the general Lie-admissible algebras. Intriguingly, this hierarchy emerges as a conceivable algebraic counterpart of a hierarchy of forces in the physical universe of increasing structural complexity and methodological needs.

Almost needless to say, a long way remains to be covered to reach a sufficient mathematical and physical maturity in this new scientific horizon, . . . But this is in line with the primary objective of this reprint series: to stimulate the participation of a broader number of mathematicians, physicists and engineers in the study of issues of fundamental character for basic research.

HADRONIC PRESS, INC., NONANTUM, MASSACHUSETTS 02195, U.S.A.

(Tables of Contents of Volumes I and II and sale terms are listed on the back of this leaflet).

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

May 7, 1979

Professor WOLFGANG K. H. PANOFSKY, Director
Stanford Linear Accelerator Center
STANFORD, California 94305

CONFIDENTIAL

Dear Professor Panofsky,

Apparently, the situation in hadron physics has deteriorared considerably since the time of my warm appeal to you of July 19, 1978. The enclosed paper is a manifestation of this situation. It has been released for wide distribution (15,000 copies via the Hadronic Press) *to indicate to quark-committed colleagues that a critical inspection of quark conjectures is in motion on a world wide scale, jointly with the study of fundamentally different lines. If they have technical arguments to disprove these criticisms, they must publish them in scientific papers. The sometime used corridor-type talks on quark-non-oriented studies by quark-committed physicists are nowaday ineffective.

The only way to defuse this deteriorating situation is the experimental way.

I would like to take the liberty of warmly encouraging again the initiation at SLAC of studies for the experimental verification of the basic physical laws currently used in strong interactions, with particular refererence to Einstein's special relativity and Pauli's exclusion principle. Even the activation of an initial feasibility study at SLAC would be invaluable, provided that its conduction is not restricted to quark supporters only.

I am confident that you will see that the protraction of the current situation in hadron physics may invite a crisis. I am referring here to the current investments of truly large amount on money on strong interactions, all based on the mere belief of the validity of the basic laws, without jointly conducting their experimental verification. Quite frankly, I am seriously concerned that the protraction of such a situation may imply a process to our scientific accountability.

I think that we still have time to prevent further deteriorations. But we simply cannot continue to effectively conduct studies in hadron physics on the basis of mere beliefs by individual physicists on fundamental issues. The return to the traditional conduction of physics, that via experiments, is, in my humble view, much needed and needed soon.
Again, with my sincere esteem, I remain

Very Truly Yours,

Ruggero Maria Santilli

* The paper should be soon distributed to SLAC

RMS/ml

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 7, 1979

CONFIDENTIAL

Professor GEORGE H. VINEYARD, Director
Brookhaven National Laboratory
UPTON, Long Island, N.Y. 11973

Dear Professor Vineyard,

Apparently, the situation in hadron physics has deteriorated considerably since the time of my warm appeal to you of July 19, 1979. The paper enclosed is a manifestation of this situation. It has been released for wide distribution (15,000 copies via the Hadronic Press)*to indicate to quark-committed colleagues that a critical inspection of quark conjectures is in motion on a world wide basis, jointly with the study of fundamentally different lines. If quark-committed physicists have technical arguments to disprove these criticisms, they must publish them in scientific papers. The often used corridor-type of talks is no longer effective.

The only way to defuse this deteriorating situation is the experimental way.

I would like to take the liberty of warmly encouraging again the initiation at BROOKHAVEN NATIONAL LABORATORY of studies for the experimental verification of the expected invalidity (according to some) or possible validity (according to others) for the strong interactions of the currently used basic physical laws, with particular reference to Einstein's special relativity and Pauli's exclusion principle. Even the initiation of a feasibility study for these experiments would be invaluable, provided that its conduction is not limited to quark believers only.

I am confident you will see that the protraction of this situation indefinitely may invite a crisis. I am referring here to the current investments of truly large amounts of money on strong interactions, all based on the mere belief of the validity of the basic laws, without jointly conducting their experimental verification. Quite frankly, I am seriously concerned that such a propraction may invite a process to our scientific accountability.

I think that we are still in time to prevent further deteriorations. But, the return to the traditional conduction of physics on fundamental issues, that via experiments, rather than beliefs, is much needed and needed soon.

Again, with my most sincere esteem and gratitude for your past courtesy, I remain

Very Truly Yours,

Ruggero Maria Santilli

* The paper should be soon distributed to Brookhaven

RMS/ml

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 7, 1979

Professor JOSEPH BALLAM
Stanford Linear Accelerator Center
STANFORD, Ca 94305

Dear Professor Ballam,

A number of elements has suggested that I anticipate my plans for promoting a moment of reflection on quarks. They also originate in Washington, and are related to an understandable need to revitalize the conduction of research in hadron physics.

I have therefore prepared the enclosed article of review on the criticisms on quarks. I have provided my sincere efforts to reach a balanced presentation (by even avoiding the presentation of additional, technical criticisms). Nevertheless, I do not know whether I did succeed in this difficult task.

I would appreciate your advice whether the publication of this article in the HADRONIC JOURNAL is appropriate or not. I do not have personal preferences.

Almost needless to say, any critical comment for bringing this paper to the necessary maturity, would be gratefully appreciated.

If, for any reason, you do not have the time to look at this paper, simply ignore this letter.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

May 7, 1979

Professor ROBERT WILSON, Office of Directors
Fermi National Accelerator Laboratory
BATAVIA, Illinois 60510

CONFIDENTIAL

Copy has been sent by mail

Dear Professor Wilson,

Apparently, the situation in hadron physics has deteriorated considerably since the time of my warm appeal to you of July 19, 1978. The paper enclosed is a manifestation of this situation. It has been released for wide distribution (15,000 copies via the Hadronic Press)*to indicate to quark-committed colleagues that a critical inspection of quark conjectures is in motion on a world wide basis, jointly with the study of fundamentally different lines. If quark believers have technical arguments to disprove these criticisms, they must publish them in scientific papers. The often used corridor-type of talk on quark-non-oriented studies by quark-committed physicists is no longer effective.

The only way to defuse this deteriorating situation is the experimental way.

I would like to take the liberty of warmly encouraging again the initiation at FERMILAB of studies on the experimental verification of the expected invalidity (according to some) or possible validity (according to others) of the basic physical laws currently used in strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.Even the activation of an initial feasibility study would be invaluable, provided that its conduction is not restricted to quark believers only.

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I think that we still have time to prevent further deteriorations. But the return to the traditional conduction of physics, that via experiments rather beliefs on fundamental issues, is much needed and needed soon.

Again, with my most sincere esteem, I remain

Yours, Very Truly

Ruggero Maria Santilli

* The paper should be soon distributed to Fermilab

RMS/ml

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

19 March 1980

Dr. R. M. Santilli
Harvard University
Science Center, Room 331
One Oxford Street
Cambridge MA 02138

Dear Dr. Santilli:

Since I will be away from the U. S. for a year beginning this summer, I would like to resign from the Editorial Council of the Hadronic Journal.

I believe my resignation is appropriate at this time, particularly since the articles in the Journal have been theoretical in content while my own expertise is in experimental particle physics.

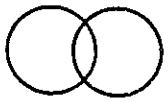
Sincerely yours,

J. Ballam
Professor

JB:alb

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138



Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

Professor MELVIN B. GOTTLIEB, Director
Plasma Physics Laboratory,
PRINCETON, New Jersey 08544

May 31, 1981

Dear Professor Gottlieb,

I would appreciate the consideration of selecting or otherwise recommending a representative from your Laboratory to our forthcoming
- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS to be held here in Cambridge from August 3 to 7, 1981 under partial support by the Department of Energy.

In these yearly Workshops we gather experimentalists, theoreticians, and mathematicians to study the problem whether the intrinsic characteristics of hadrons (magnetic moments, spin, space parity, etc.) are preserved or altered in the transition from the conditions they have been measured until now (long range electromagnetic interactions), to the different physical conditions of the strong interactions. To put it in different terms, we are interested in achieving direct or otherwise clear experimental, theoretical, and mathematical information on the magnetic moment, spin, parity and other intrinsic characteristics of nucleons under the high pressures, densities, and temperatures of the controlled fusion.

In a few nontechnical words, the alternatives are essentially the following. Alternative I. The conventional point-like approximation of hadrons (and their constituents) is effective for the strong interactions; the views currently prevailing in high energy physics are correct; and the underlying mathematical structure (that of local differential character) is truly valid. Under these assumptions, there is no reason to suspect any fundamentally new or otherwise different occurrence, besides well known effects (such as the transition from spin 1/2 to 5/2 due to resonances). Alternative II. Hadrons under strong interactions are truly represented according to experimental evidence, that is, as wave packets in necessary conditions of mutual penetration and overlapping (recall that the range of the strong interactions coincide with the size of hadrons). In this case there is the need of fundamentally new mathematical formulations, e.g., of nonlocal type because of the need to represent the interactions at all points of the volume of mutual wave penetration. Also, there is the need of new physical approaches. In fact, the interactions are (partially) of nonpotential type because the notion of potential has no physical foundation for contact interactions, whether Newtonian or quantum mechanical, and discrete or continuous. But nonpotential forces imply nonunitary time evolutions. In turn, nonunitary time evolutions imply rather profound departures from the electromagnetic characteristics of particles, and new situations become conceivable as internal dynamical effects for strong bound systems which are not detectable from the outside. For instance, the spin and magnetic moment of the proton while the nucleus of an hydrogen atom have the well known perennial values. However, if the proton exits the atom , and enters into conditions of mutual wave overlapping with other nucleons, the"spin" becomes dependent on time and local conditions, the magnetic moment can vary (up to 50 % and more), the discrete symmetries are broken, etc.
A copy of my review presentation at the Clausthal Conference of 1980, as well as at our Third Workshop of the past summer is enclosed.

The greatest majority of experiments currently available in strong interactions have been conducted via the assumption of conventional electromagnetic characteristics in their data elaboration. As a result, these experiments, even though valuable for other objectives, are not suitable for our purposes. Our problem simply calls for direct measurements, that is, measurements under strong interactions. A rather in-

tensive effort is under way to formulate experiments of this type, for the future resolution of the problem considered either in favor of the electromagnetic characteristics, or in favor of new ideas. Intriguingly, as you can see from the enclosed paper, the available direct information in nuclear physics either favors variations of the electromagnetic characteristics, or the variations cannot be excluded.

A number of members of our group are experts in nonpotential, classical and quantum mechanical, statistical mechanics. However, none is an expert in the plasma physics which is needed for the controlled fusion.We would therefore appreciate the participation by one or more members of your Laboratory, either of theoretical or experimental orientation (or both). The problems we would like to study are the following.

1. Identification of the state of the art of our experimental and theoretical knowledge on the intrinsic characteristics of nucleons and underlying time evolution for the conditions of the controlled fusion;

2. Identification of experimental and theoretical implications for the controlled fusion of the possibility that nucleons evolve according to a nonunitary time evolution because of wave overlappings, with consequential local variations from intrinsic characteristics of electromagnetic-Lie type; and

3. Identification of the orientation of the research conducted by our group which is most effective for energy related profiles.

For your information, our Workshops are not conferences, but actual workshops, that is, we gather to conduct actual research in a friendly relaxed atmosphere. Each participant presents a partially or fundamentally open problem and benefits from the presence of a combined group of mathematicians, theoreticians, and experimentalists.

A registration form is enclosed for your convenience, while I remain at your disposal for any needed additional information.

A formal presentation of the results of the Workshop of this year (as well as of the preceding three Workshops) will be conducted at the
- FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, to be held at the University of Orléans, France, from January 5 to 7, 1982, under financial support by the French Government.

Thanking for your consideration and time, I remain

Yours Very Truly

Ruggero Maria Santilli
Organization Committees of the Fourth Workshop and First International Conference on Nonpotential Interactions

Tel (617) 964 1684

cc.Drs. D.J.GROVE, H.P.FURTH, P.J.REARDON, R.A.ROSSI, P.H.RUTHERFORD, T.H.STIX, E.C.TANNER, M. SHOAF, G.V. SHEFFIELD, J. JOYCE, R.G.MILLS,K.E.WAKEFIELD,E.B. SIMON, J.A.SCHMIDT, R.LITTLE,D.M.MEADE, F.W.PERKINS, A.R. DE MEO,R.J.GOLDSTONE, D.L.JASSBY, M. OKABAYASHI, D.E.POST, J.C.SINNIS, G.M.BROWN, C.NUSHNELL,F.K. BENNETT, R.B.FLEMING,G.H.RAPPE, at the Plasma Physics Laboratory.

RMS-ml; encls.

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

tel (617) 964 1684

July 2, 1981

Professor WOLFGAND K. H. PANOFSKI, Director
Stanford Linear Accelerator Center
Stanford University
SANFORD, California, 94305

Dear Professor Panofski,

During the past years I have contacted you at the rate of less than once per year to solicite the initiation at SLAC of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

This is my letter of solicitation for 1981. Again, permit me to recall that I do not recommend specific tests. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians with the task of ascertaining the feasibility of new experiments, assessing the possible re-elaboration of existing data, and, of course, evaluating the available experimental information. More specific recommendation on the composition and function of this committee are at your disposal on request.

For your information, we have made some progress on the problem in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists actively working at the problem. In particular, we have identified experimental information such as:
- the apparent, quite large, deviation of the magnetic moments of hadrons under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large, deviation from the predictions of the conventional laws in the optical activity of neutron beams under strong nuclear interactions, according to the experiment by Forte et al;
- the apparent, also substantial, breaking of the T-symmetry under strong nuclear interactions, according to the experiment by Conzett et al;
- the apparent, also considerable, deviations of experimental points from the behaviour of the exact SU(2)-spin symmetry, according to the experiment by Rauch et al; and other data.

Admittedly, the experimental information is still preliminary; all data could be manipulated to force compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experimental information is sufficient to establish the fact that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions, which, if confirmed by future measurements, would imply the irreconciliable invalidations of the entire Poincaré symmetry. The understanding is that the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capabilities.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincaré symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

I presume you are familiar with the basic theoretical alternatives. If the familiar point-like abstractions of hadrons are truly effective for the strong interactions, there is no ground to expect deviation from conventional laws. In fact, points can only interact at a distance; the forces are then necessarily of potential type; and the familiar, local, Poincaré covariant, Lagrangian theories are consequential. BUT, all hadrons have a dimension of the order of the range of the strong interactions, and they are constituted by wave packets (rather than points). As a result, strong interactions demand the mutual penetration of wave packets for their activation. This, in turn, is a typical contact interaction in an extended region of space for which local/differential models are excessively approximative, and the notion of potential has no physical basis. Still in turn, nonlocal nonpotential interactions demand a nonunitary time evolution under which the electromagnetic characteristics of particles are not conserved, with consequential, irreconciliable invalidation of the entire (connected and discrete) Poincaré symmetry, and the need for broader physical laws.

A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

When seen from the outside, the time evolution is canonical, and the total energy is conserved. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by experimentalists in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming

- FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, which will be held at the Université d'Orléans, France, from January 5 to 7, 1982, under financial support by the French Government via local Institutions.

Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml, encls.

cc. Professors SIDNEY D. DRELL, JOSEPH BALLAM, RICHARD BARR NEAL, and JOHN R. REES.

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

tel. (617) 964 1684

July 2, 1981

Professor LEON LEDERMAN, Director
Fermi National Accelerator Laboratory
P.O.Box 500
BATAVIA, Illinois 60510

Dear Professor Lederman,

During the past years I have contacted your Laboratory (with letters addressed to ROBERT WILSON and other executives) at the rate of less than once per year, to solicite the initiation at FERMILAB of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with paricular reference to Einstein's special relativity, Pauli's exclusion principle, and other laws.

This is my letter of solicitation for the year 1981. Again, permit me to recall that I do not recommend specific tests. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians, with the task of ascertaining the feasibility of new experiments, assessing the possible re-elaboration of old data, and, of course, evaluating the available experimental information. More specific recommendations on the composition and function of this committee are at your disposal upon request.

For your information, we have made some progress on the problem in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists working at the problem. In particular, we have identified experimental information such as:

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Admittedly, this experimental information is still preliminary; all data could be manipulated to force compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experimental information is sufficient to establish that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions which is theoretically quite plausible (see below), and which, if confirmed by future experiments, would imply the irreconciliable invalidation of the entire Poincaré symmetry. The understanding is that the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincaré symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

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A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

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A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming
- FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, which will be held at the Université d'Orléans, France, from January 5 to 7, 1982, under financial support by the French Government via local Institutions.

Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml, encls.

cc.: Professors P.V.LIVDAHL, J.R.ORR, F.R.HUSON, C.N.BROWN, C.QUIGG, B. CHRISMAN, R.P.JOHNSON, D.D.JOVANOVIC, T.B.KIRK, R.A.LUNDY, E.I.MALAMUD, P. MC INTYRE, J. PEOPLES, R. CARRIGAN, W.B.FOWLER, K.STANFIELD, L.C. TENG, D.E.YOUNG, S.I.BAKER, M.F.GORMLEY, Q.A.KERNS, M.E.JOHNSON, E.T. NASH, F.A.NEZRICK, A. TOLLESTRUP, D.A.BAUER, D.A.EDWARDS, H.T.EDWARDS, L.V.COULSON, A.E.BRENNER, T.YAMANOUCHI, P.LIMON,

Courtesy copy to R. WILSON and H. ABARBANEL

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees
tel. (617) 964 1684

July 2, 1981

Professor GEORGE H. VINEYARD, Director
Brookhaven National Laboratories
UPTON, Long Island, New York 11973

Dear Professor Vineyard,

During the past years, I have contacted you at the rate of less that once per year to solicit the initiation at BROKHAVEN NATIONAL LABORATORIES of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other conventional laws.

This is my letter of solicitation for the year 1981. Again, permit me to recall that I do not recommend specific tests. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians, with the task of ascertaining the feasibility of new experiments, assessing the possible re-elaboration of old data, and, of course, evaluating the available experimental information. More specific recommendations on the composition and function of this committee are at your disposal upon request.

For your information, we have made some progress on the problem in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists working at the problem. In particular, we have identified experimental information such as :

- the apparent, quite large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large, deviation from the predictions of the conventional laws in the optical activity of neutron beams under strong nuclear interactions, according to the experiment by Forte et al;
- the apparent, also substantial, breaking of the T-symmetry under strong nuclear interactions according to the experiment by Conzett et al;
- the apparent, also considerable, deviation of experimental points from the predictions of the exact SU(2)-spin symmetry, according to the experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data could be manipulated to force compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experimental information is sufficient to establish that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions which is theoretically quite plausible (see below), and which, if confirmed by future expeiments, would imply the irreconciliable invalidation of the entire Poincare symmetry. The understanding is that the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincaré symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

I presume you are familiar with the basic theoretical alternatives. If the familiar point-like abstractions of hadrons are truly effective for the strong interactions, there is no ground to expect deviation from conventional laws. In fact, points can only interact at a distance; the forces are then necessarily of potential type; and the familiar, local, Poincaré covariant, Lagrangian theories are consequential. BUT, all hadrons have a dimension of the order of the range of the strong interactions, and they are constituted by wave packets (rather than points). As a result, strong interactions demand the mutual penetration of wave packets for their activation. This, in turn, is a typical contact interaction in an extended region of space for which local/differential models are excessively approximative, and the notion of potential has no physical basis. Still in turn, nonlocal nonpotential interactions demand a nonunitary time evolution under which the electromagnetic characteristics of particles are not conserved, with consequential, irreconciliable invalidation of the entire (connected and discrete) Poincaré symmetry, and the need for broader physical laws.

A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

When seen from the outside, the time evolution is canonical, and the total energy is conserved. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming
- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming
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Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml, encls.

cc.: Professors RONALD R. RAU, JAMES R. SANFORD, NICHOLAS P. SAMIOS, LYLE W. SMITH, ARTHUR Z. SCHWARZSCHILD, RONALD T. PIERLS, and M. GOLDHABER

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Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

tel (617) 964 1684

July 2, 1981

Professor HERMAN POSTMA, Director
Oak Ridge National Laboratories
Union Carbide Corporation
OAK RIDGE, Tennessee 37830

Dear Professor Postma,

I am contacting you to solicite the initiation at OAK RIDGE NATIONAL LABORATORIES of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

Permit me to indicate from the outset that, despite the availability of several proposals of specific tests, I do not recommend a specific test. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians (see below why), with the task of ascertaining the feasibility at your laboratories of new experiments, assessing the possible re-elaboration of old data, and, of course, evaluating available experimental information. The understanding is that the tests, to be accepted by the scientific community at large, should be as direct as possible, and should avoid unverified conjectures in their data elaboration, such as those of quark type. Additional suggestions on the composition and function of the committee are at your disposal.

For your information, we have made some progress in the topic in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists working at the problem. In particular, we have identified experimental information such as:
- the apparent, quite large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large deviation from the predictions of conventional laws in the optical activity of neutrons under strong nuclear interactions, according to experiments by M. Forte et al;
- the apparent, also substantial violation of the T-symmetry under strong nuclear interactions, according to experiments by Conzett et al;
- the apparent, also considerable deviation of experimental points from the predictions of the exact SU(2)-spin symmetry, according to experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data can be manipulated to force compatibility with orthodox laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. However, the experimental information is sufficient to establish that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions which is theoretically quite plausible (see also below), and which, if confirmed by future experiments, would imply the irreconciliable invalidation of the entire Poincaré symmetry, and the thrust toward a fundamental advancement. Understandably, the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincaré symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

I presume you are familiar with the basic theoretical alternatives. If the familiar point-like abstractions of hadrons are truly effective for the strong interactions, there is no ground to expect deviation from conventional laws. In fact, points can only interact at a distance; the forces are then necessarily of potential type; and the familiar, local, Poincaré covariant, Lagrangian theories are consequential. BUT, all hadrons have a dimension of the order of the range of the strong interactions, and they are constituted by wave packets (rather than points). As a result, strong interactions demand the mutual penetration of wave packets for their activation. This, in turn, is a typical contact interaction in an extended region of space for which local/differential models are excessively approximative, and the notion of potential has no physical basis. Still in turn, nonlocal nonpotential interactions demand a nonunitary time evolution under which the electromagnetic characteristics of particles are not conserved, with consequential, irreconciliable invalidation of the entire (connected and discrete) Poincaré symmetry, and the need for broader physical laws.

A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

When seen from the outside, the time evolution is canonical, and the total energy is conserved. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming

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Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml, encls.

cc. Professors A.ZUCKER, P.H.STELSON, H.N.HILL, F.C.MAIENSCHEIN, H.E.TRAMMEL, R.S.LIVINGSTON, D.B.TRAUGER, M.K.WILKINSON,

Courtesy copy to Professors P.D.MILLER and W.B.DRESS

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Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees
tel. (617) 964 1684

July 2, 1981

Professor DAVID A. SHIRLEY, Director
LAWRENCE BERKELEY LABORATORIES
KERKELEY, California 94720

Dear Professor Shirley,

During the past years I have been contacting your Laboratory at the rate of less than once per year, to solicit the initiation of suitable experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

This is my letter of solicitation for the year 1981. Permit me to recall that, despite the availability of a number of proposals as well as of initial tests, I do not recommend any specific test. Instead, I recommend the setting up of a committee composed of experimentalists, theoreticians, and mathematicians, with the task of ascertaining the feasibility of new experiments at the LAWRENCE BERKELEY LABORATORIES, assessing the possible re-elaboration of old data, and, of course, evaluating available experiments. Predictably, for the experiments to be accepted by the scientific community at large, the data should not be elaborated via unverified conjectures, such as those of quark and other type. Additional suggestions for the structure and function of the committee are at your disposal on request.

You might be interested to know that we have made some progress in experimental nuclear physics. In fact, we now have in the field a coordinated group of mathematicians, theoreticians, and experimentalists actively involved. In particular, we have identified experimental information such as:

- the apparent, quite large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits and other data;
- the apparent, also substantial, deviation from the prediction of orthodox laws in the optical activity of neutrons beams under strong nuclear interactions, according to the experiments by Forte et al;
- the apparent, also quite large, violation of the T-symmetry under strong nuclear interactions, according to the experiments by Conzett (at your Laboratories) et al;
- the apparent, also considerable, deviation of experimental points from the predictions of the exact SU(2)-spin symmetry, according to the experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data might be re-interpreted to reach a form of compatibility with conventional laws; and all experiments may, in the final analysis, be disproved by future more accurate measures. However, the experimental information, when taken globally, points toward the alteration of the space-time characteristics of particles under strong interactions, which is quite plausible theoretically (see below), and which, if confirmed by future experiments, would imply the irreconciliable invalidation of the entire Poincaré symmetry, as well as the thrust toward fundamental advancements (despite conceivable opposition to achieve new fundamental knowledge). Evidently, the Poincaré symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincare' symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

I presume you are familiar with the basic theoretical alternatives. If the familiar point-like abstractions of hadrons are truly effective for the strong interactions, there is no ground to expect deviation from conventional laws. In fact, points can only interact at a distance; the forces are then necessarily of potential type; and the familiar, local, Poincare' covariant, Lagrangian theories are consequential. BUT, all hadrons have a dimension of the order of the range of the strong interactions, and they are constituted by wave packets (rather than points). As a result, strong interactions demand the mutual penetration of wave packets for their activation. This, in turn, is a typical contact interaction in an extended region of space for which local/differential models are excessively approximative, and the notion of potential has no physical basis. Still in turn, nonlocal nonpotential interactions demand a nonunitary time evolution under which the electromagnetic characteristics of particles are not conserved, with consequential, irreconciliable invalidation of the entire (connected and discrete) Poincare' symmetry, and the need for broader physical laws.

A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

When seen from the outside, the time evolution is canonical, and the total energy is conserved. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

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Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml, encls.

cc.: Professors E.K.HYDE, R.K.WAKERLING, R.W.BIRGE, E.L.ALPEN, A.W.SEARCHY, G.C.PIMENTEL, W.D.HARTSOUGH, H.A.GRUNDER, W.A.LESTER, E.J.CAIRNS, J. HOLLANDER, J.CERNY, L.W.ALVAREZ, G.T.SEABORG
Courtesy copy to Professor H.E.CONZETT, as well as to R.L.KELLY, A.RITTENBERG, and T.G.TRIPPE.

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Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

tel. (617) 964 1684

July 2, 1981

Professor DONALD M. KERR, Jr., Director
Los Alamos Scientific Laboratories
Los Alamos, New Mexico 87545

Dear Professor Kerr,

I am contacting you to solicite the initiation at LOS ALAMOS SCIENTIFIC LABORATORIES of suitable experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

Permit me to indicate from the outset that, despite the availability of several old and new proposals as well as initial tests, I do not recommend any particular test. Instead, I suggest the setting up of a committee composed of experimentalists, theoreticians, and mathematicians with the task of ascertaining the feasibility at your laboratories of new tests, assessing the possible re-elaboration of old data, and, of course, evaluating available experimental information. Predictably, for the tests to be accepted by the scientific community at large, the data should avoid unverified conjectures in their elaboration, such as those of quark and other type. Additional suggestions on the composition and function of the committee are at your disposal.

For your information, we have recently made some progress in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists working at the problem. In particular, we have identified experimental information such as:

- the apparent, rather large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits and other data;
- the apparent, also quite substantial, deviation from conventional predictions in the optical activity of neutrons under strong nuclear interactions, according to the experiments by Forte et al;
- the apparent, also substantial violation of the T-symmetry under strong nuclear interactions, according to the experiment by Conzett et al;
- the apparent, also considerable, deviation from the predictions of the exact SU(2)-spin symmetry under strong nuclear interactions, according to the experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data can be re-interpreted to achieve a sort of compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experiments are such to establish that the exact validity of conventional laws under strong interactions is a mere belief at this time by individual groups of researchers. In fact, the information, once seen globally, points toward the alteration of the space-time characteristics of particles under strong interactions which is quite plausible theoretically (see below), and which, if confirmed, would imply the irreconciliable invalidation of the entire Poincaré symmetry, as well as the thrust toward a fundamental advancement (despite conceivable opposition to achieve new knowledge). Needless to say, the Poincaré symmetry would preserve physical value, but only as a crude approximation of a reality beyond its technical capability.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincaré symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

I presume you are familiar with the basic theoretical alternatives. If the familiar point-like abstractions of hadrons are truly effective for the strong interactions, there is no ground to expect deviation from conventional laws. In fact, points can only interact at a distance; the forces are then necessarily of potential type; and the familiar, local, Poincaré covariant, Lagrangian theories are consequential. BUT, all hadrons have a dimension of the order of the range of the strong interactions, and they are constituted by wave packets (rather than points). As a result, strong interactions demand the mutual penetration of wave packets for their activation. This, in turn, is a typical contact interaction in an extended region of space for which local/differential models are excessively approximative, and the notion of potential has no physical basis. Still in turn, nonlocal nonpotential interactions demand a nonunitary time evolution under which the electromagnetic characteristics of particles are not conserved, with consequential, irreconciliable invalidation of the entire (connected and discrete) Poincaré symmetry, and the need for broader physical laws.

A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

When seen from the outside, the time evolution is canonical, and the total energy is conserved. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming
- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming
- FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, which will be held at the Université d'Orléans, France, from January 5 to 7, 1982, under financial support by the French Government via local Institutions.

Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml, encls.

cc. F. ZACHARIASEN, Associate Director
(names of divisional directors not available, but copies of this letter are available on request)
Courtesy copy to R.A.HARDEKOPF, P.W.KEATON, P.W.LISOWSKI, and L.R.VEESER

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

4-th of July, 1981

Professor SYDNEY MESHKOV
National Beaureau of Standards
WASHINGTON, D.C.

Dear Professor Meshkov,

I am here inviting you to become an EDITOR FOR THEORETICAL PHYSICS of the HADRONIC JOURNAL, in the specific field of quark models, QCD, and gauge theories, to continue the editorial function by HOWARD GEORGI, who served as an editor from the initiation of the Journal in 1978 to his promotion to full professor of physics at Harvard in the past fall. This invitation originated via a kind, informal indication by GINO SEGRE at the University of Pennsylvania, and was subsequently approved by representative members of the current editorial organization of the Journal, copy of which is enclosed. The following information may assist you in your decision.

General information on the Journal. It was initiated in 1978, and since then, it has been published exactly on schedule. We are now at the fourth volume (which is just about to be completed). Originally, the Journal published only articles. More recently, the Journal initiated the publication of a selected number of Proceedings of Workshops, Conferences, and Summer Schools. This service has been well accepted by the community and it is becoming a success due to the virtual impossibility, lately, to locate articles in Proceedings of this type (to have an idea of the numbers, there are some 125 graduate schools in physics in the U.S.A., more than half of which are under serious financial restrictions—therefore, the sale of 45-to-50 Proceedings is outstanding these days...). By having the Proceedings appear in a regular Journal, the problem of the location of articles is considerably alleviated. Copy of the Table of Contents of some of the Proceedings we have published is enclosed. More recently, the Journal has initiated the publication of a considerable series of reprint volumes in hadron physics. They are:

- "Developments in the Quark Theory of Hadrons"; Edited by DON B. LICHTENBERG of the University of Indiana, and PETER S. ROSEN of Purdue University; and
- "Applications of Lie-admissible Algebras in Physics"; Edited by Professor HYO C. MYUNG ofthe University of Norther Iowa, Professor SUSUMU OKUBO of the University of Rochester, and myself.

You have perhaps seen the first volume of the first series (if not, please let me know, and I shall let you have a complimentary copy). Don and Peter are now working at the second volume, and we, at the Journal, are ready to publish it whenever they have finished their (rather coplex) work of selecting good articles. The second series is a sort of negative of the first, and viceversa, in the sense that it publishes articles in hadron physics which are not of quark inspiration. The idea is that, via the two combined series, we may have a record of valuable research in this last part of our century which may acquire value in time.

Thanks to these various scientific initiative, as well as, more importantly, to the invaluable contribution by all members of the Editorial Organization, the Journal has now reached financial selfsufficiency, as well as world wide distribution (for example, all research libraries of China subscribe to our Journal). It should be said that the primary strength of the Journal is abroad, owing to the disastrous financial condition here at home. Particularly rewarding is the request of reprints from virtually all over the world that our Authors receive.

General information on the Editors. Each Editor has complete, final, and independent scientific authority. In particular, the Publisher is obliged to publish all articles accepted by each editor. The Editors often consult each other, but decisions are taken individually. Also, the Journal is known to avoid the criptic anonimity so conducive

to academic mumbo-jumbo. The Editors select valuable articles for study, and return those considered not suitable with a nice letter of recommendation for other Journals. The few articles selected are personally studied by the Editors almost all of the cases, occasionally, with outside consultation when needed. The Editors therefore write directly to the Authors, by communicating their comments. In this way, we have established a nice record of excellent relationship Editor-Author, which is friendly, cooperative and most effective. The Editorial Load per author is truly minimal. In fact, we study an average of three articles per month in our field. Those are articles which we would study anyhow, whether Editors or not. The position of Editor does not carry a salary at this time, although the Journal reimburses all direct expenses, such as secretarial, postage, and phone. If the subscriptions will increase to the point of rendering the Journal profitable, each Editor will have a salary/honorarium. Each Editor has a complimentary subscription to the JOurnal, as well as a free copy of all publications of the Hadronic Press.

Journal's scientific attitude. We have a record of publishing articles for their scientific contents, and of being totally insensitive to ethnic, religious, political, or other aspects. A nice episode is that of a refusenik from the U.S.S.R., Dr. Y.A.GOLFAND. FRITZ ROHRLICH called me one day indicating the search by this author of a western Journal to publish one of his articles, and his difficulties with another qualified Journal. I stressed to Fritz that our Journal is keenly open to Authors for technical assistance, and that is what happened. Dr. Golfand received scientific assistance by a number of us to reach the utmost possible maturity, and his article was subsequently published in Hadronic J. 2, 261 (1979). More receintly, Dr. Golfand was reinstated in his post, and I received very nice and rewarding letters of thanks by Jewish organizations in the U.S.A. for the little our Journal had done. We would like to keep our function of a medium open to all, and I believe that you would be particularly qualified for the continuation of this function.

Also, we are particularly interested in avoiding a monopolistic restriction of articles along quark lines only. I have personally invited several authors for papers along quark lines. Yet, I have made an effort in publishing, jointly, papers along different lines, so that the Journal is open to all potentially valuable lines of studies on the fundamental problem of hadron structure (the terminal character of unitary models for hadron classification is out of the question for us, and the dubious aspect is only for the jcint structure). This attitude is received with criticism by physicists financially committed to quarks. Yet, the scientific community at large seems to be particularly receiptive to the uncommitted character of the Journal. As one distinguished scientist put it to me, the publication of articles based on quark assumptions for the hadronic structure, but in their current status of lacking a true, strictly rigorous confinement, is literally equivalent to the publication of articles in mathematical journals stating that $2 + 2 = 138792.345$. At our Journal we therefore make a point in acknowledging the scientific value of papers along quark lines, but, at the same time, we equally make a point in focusing their yet incomplete and nonterminal character.

I believe you would be an excellent editor for such a scientific function. You have a proved record of ethical standard. I therefore believe you would see with grace the publication of articles accepted by you, while subsequent issues could publish articles expressing different or oppositive viewpoints. At any rate, this is the only way for the true pursuance of physical knowledge.

Journal's primary objective. In reaching your decision, you should know that the primary reason why I initiated the laborious process of setting up a new Journal is to promote the experimental verification of the validity or invalidity (exact or only approximate validity, if you prefer) for the strong interactions of the basic physical laws of the electromagnetic interactions, with particular reference to the entire (connected and discrete) Poincaré symmetry, the gauge symmetry, and Pauli's exclusion principle.

Thanks to our yearly WORKSHOPS ON LIE-ADMISSIBLE FORMULATIONS, this problem is now studied by a coordinated group of mathematicians, theoreticians, and experimentalists. We have made considerable progress in experimental nuclear physics via the identification of experimental information indicating the invalidity, such as

- the apparent quite large deviation of magnetic moments under strong nuclear interactions according to the data of the Schmidt limits;
- the apparent, also quite large, deviation from conventional predictions (including those of the Weinberg-Salam theory) for the angle of precession of the optical activity of neutron beams under strong nuclear interactions, according to an experiment by Forte et al;
- the apparent, also substantial, breaking of the T-symmetry under strong nuclear interactions, according to the experiment by Conzett et al; and other.

We shall soon initiate studies in experimental high energy physics. The objective here is to formulate direct experimental tests(say, of intrinsic quantities of hadrons under strong interactions) without experimentally unverified theoretical assumptions in the data elaboration. Specifically, to reach the needed final character, the elaboration should not be based on quark conjectures (it would be, in this case, only a vague argument of plausibility under a serious and strict scientific scrutiny). It is a long way to go, but we are determined to follow it to its end, that is, to the end of this truly unique episode of the history of physics whereby huge amounts of human (and financial)resources are spend in the development of the theory of the strong interactions, by virtually ignoring the verification of the basic physical laws. As an historian put it to me, unless this situation is corrected soon, and direct experiments are at least started to be considered, there is little doubt that future historians will have a severe judgment of the ethical standards of contemporary high energy physicists. After all, the open character of the basic laws is known from open legacies of the founders of contemporary physics, let alone a large series of (completely ignored) articles.

My recommendation. In case you are seriously interested in considering the position of Editor of the Hadronic Journal, I would like to recommend that you participate as an auditor to our FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which we will have here in Cambridge from August 3 to 7, 1981. At our Institute we have funds from the DEPARTMENT OF ENERGY to support (most of) your expenses. This would give you an opportunity to listen and meet several members of our group.

You could then initiate your function of EDITOR FOR THEORETICAL PHYSICS beginning from the first issue of Volume 5, that of December 1981.

In case you need any additional information, please feel free to call me (tel(617) 964 1684) or HOWARD GEORGI (tel. (617) 495 3908), or any other member of our Editorial Organization.

Sincerely

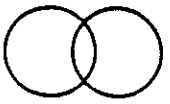
Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS-ml
encls.

cc. Professors HOWARD GEORGI, Harvard Univ. and GINO SEGRE, University of Pennsylvania

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138



Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

July 15, 1981

Professor S. MESHKOV
National Beaureau of Syandards
WASHINGTON, D.C.

Dear Professor Meshkov,

In the afternoon of July 29, 1981, the buildings on Harvard Grounds known as the PRESCOTT HOUSE were acquired to provide permanent facilities for the new INSTITUTE FOR BASIC RESEARCH.

Ruggero and Carla Santilli cordially invite you to attend

THE INAUGURATION CEREMONY
Monday, August 3, 1981
from 9 a.m. to 9.30 a.m.,

and

THE AFTER-DINNER PARTY
Thursday, August 6, 1981
from 8 p.m. to 10 p.m.

R. S. V. P.
(617) 964 1684

MESHKOV NEVER ACKNOWLEDGED ANY OF THESE INVITATIONS.

R.M.S.

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

July 13, 1981

Professor Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Mass. 02138

Dear Professor Santilli:

Thank you very much for your letter of July 2 which you describe as the annual letter "to solicit the initiation at SLAC of experimental studies on the validity or invalidity for the strong interactions of . . ."

You correctly refer to the fact that the experimental information is still preliminary; in fact all experimental information is preliminary in the sense that it can and will be superceded by newer results. You also say "All data could be manipulated to force compatibility with conventional laws." Your principal proposal is that I should convene a meeting of leaders of our laboratory and in the field to consider experiments to specifically test your hypotheses.

Experiments are not conceived or designed in committee; rather, individual initiative arises from the scientific community and from that initiative results a proposal for a specific undertaking which appears technically feasible to the laboratory. The laboratory directors have little and should have little influence over this process. Therefore the only recourse you have is to disseminate your theoretical deliberations to as wide an audience of experimentalists as possible in a manner such that they can extricate easily the experimental implications of the theory.

With best personal regards,

Wolfgang K. H. Panofsky

Wolfgang K. H. Panofsky
Director

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

Professor WOLFGANG K.H. PANOFSKY, Director
Stanford Linear Accelerator Laboratories
Post Office Box 4349
STANFORD, California 94305

July 20, 1981

Dear Professor Panofsky,

I would like to express my appreciation for the courtesy of your recent letter, as well as for elucidating a number of points.

I agree that the decision to undertake an experiment is taken by one or more individual experimentalists, rather than a committee. Please keep in mind that a number of specific proposals by independent authors are available for experimentalists with the necessary courage and determination. I have recommended a committee of study because the selection of the best and most effective (technically and administratively) proposal appears to be rather complex, and demanding a variety of skills (from experimental aspects to mathematical profiles, all mostly new), which go beyond the capability of one single person these days. The study of the committee should therefore only pave the way to the actual, subsequent selection by experimentalists.

I am also in agreement, of course, with the fact that Laboratory Directors should have little influence, if any, in the selection of experiments.Since you have a better knowledge of the members of your laboratory, you could perhaps pass the information to potentially receptive experimentalists in a way much more effective of that we might do from far away. At the same time, I am sure you will agree that Laboratory Directors should be kept fully informed of primary aspects, particularly those with potentially delicate administrative implications.

I am flattered by your nice attitude to give me the paternity of the ideas. However, please keep in mind that the mathematicians and physicists of our group are simply trying to study as seriously as possible the teaching of our scientific fathers such as FERMI (strong interactions should contain a nonpotential component due to contact effects which are absent in electromagnetism), EINSTEIN (the uncertainty of the Copenhagen School is only a temporary episode in physics), JORDAN (the envelope of quantum mechanics should be generalized form the current associative form AB to a non-associative one $A \cdot B$), etc.

Please let me know in case you visit the Boston area (or simply pass by). It would be a sincere pleasure to meet you, show you the facilities of our new Institute, and, depending on your availability of time, have you as our guest for dinner. Several people tell me that I am much nicer and respectfull in person than in letters (which still suffer of my classic studies at a European lyceum).

Sincerely,

Ruggero

Ruggero Maria Santilli,
Director
RMS-ml

P.S. H. RAUCH, Director of the Atominstitut in Wien has just informed me that, following our work, his peaople in Wien have reviewed and updated their data elaboration regarding the results of their 4π experiment and, to his surprise, the new experimental number is $\delta = 715.87 \pm 3.8^{\circ}$ (the old number was $\delta = 716.8 \pm 3.8^{\circ}$ as reproduced in the paper I mailed you). As a result, there exist no experiment currently available which is capable of reproducing the 720° which are needed to establish scientifically (that is, outside academic politics) the SU(2)-spin symmetry under strong nuclear interactions.

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 · Batavia, Illinois · 60510
312-840-3211 FTS 370-3211

Directors Office

July 28, 1981

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Your letter of 2 July has raised procedural problems we have no way of addressing. This Laboratory provides facilities for carrying out experiments in High Energy Physics - orthodox or not - as long as the Physics Advisory Committee deems the proposal of sufficient scientific merit.

The main point is that this Laboratory does not do experiments. These are proposed to us by users groups at Harvard, Caltech, and some 100 institutions in the U.S. and abroad. We would be happy to receive unorthodox proposals for research to which we can react. We do not have any mechanism to set up committees to address the kind of tasks you outline. This would have to be done at your initiative outside of the activities of Fermilab.

Sincerely,

Leon M. Lederman

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees
tels. (617) 964 9859
(617) 964 1684

August 12, 1991

Dr. LEON M. LEDERMAN, Director
Fermi National Acceleration Laboratory
P.O.Box 500
BATAVIA, Illinois 60510

CONFIDENTIAL

Dear Dr. Lederman,

I would like to express my appreciation for your kind letter of July 28, 1981. However, permit me the liberty of expressing concern for its content.

Truly large financial and human resources have been spend through the years and are currently spent at FERMILAB in strong interactions, all under the assumption of the validity of conventional laws, and despite the knowledge, repeated through the years, that possible modifications of the basic laws imply such technical consequences to result in different numbers for the same experiments. The seriousness of the problem is then self-evident.

On my part I have simply accomplished the scientific duty of bringing to the attention of Fermilab (to Dr. Wilson first, and now to you) the existence of a rapidly growing community of scientists and observers calling for the experimental verification of the basic laws, irrespective of its result (whether in favor or against), as well as, perhaps equally importantly, the achievement of a more balanced use of public funds.

My concern for FERMILAB has been increased considerably by your letter because Harvard, Caltech, and all the other academic Institutions you mention <u>are not</u> responsible for the situation. In fact, these institutions have good reasons to resist any intrusion in their own internal decisional processes. As a result, the entirety of the responsibility of the situation is viewed to rest on you, as well as all the other executives at FERMILAB and other national laboratories. The fact that, according to your letter, FERMILAB does not have mechanisms to set up committees of study, can aggravate the situation, but cannot eliminate your responsibility. To be specific, if fifty colleges propose independently exactly the same experiment, they infringe no rule. It is the responsibility of bodies such as FERMILAB to prevent that public funds are wasted by unnecessarily repeating the same experiment fifty times. If all the colleges affiliated with FERMILAB abstein from proposing a needed experiment, they also violate no rule. In fact, if the experiment is needed to provide credibility to others, or for any other scientific reason, its promotion is expected from laboratories such as FERMILAB.

It is usually difficult to predict the future, and it is more so in this case. This means that everything may continue to function smoothly and orderly for years, or a serious crisis may be triggered a few months from now by malcontent or other unforeseable reasons, particularly in this delicate moment of considerable scrutiny on the use of public funds.

What appears recommendable in view of the circumstances, is to initiate all possible preventive measures to ensure the orderly continuation and function of our community. But, despite my best personal intentions, as well as the proved best intention of all colleagues I am in touch with, this objective simply cannot be accomplished in a way completely outside

FERMILAB, as your letter appears to suggest. For instance, no physicist will expectedly spent his time to conceive an experiment for a machine at FERMILAB without the collaboration of an experimentalist fully familiar with that machine.

In view of these and other aspects (which we should eventually discuss verbally), permit me the liberty of suggesting the following course of action.

1. Select at least one theoretician employed by FERMILAB with the task of studying the literature. This person would be welcome at any time and for any length of time as a guest at our Institute following your personal recommendation (I am told that most of the literature is not available at Fermilab).

2. Upon achievement of a minimum but sufficient knowledge of the literature, have this person attend the forthcoming International Conference at Orleans, France (January 1982), where he or she can speak directly with the various originators of the studies. Subsequent to that, have this person prepare a report to you on the issue. The understanding is that a copy of the report will be released to our Institute as a condition for participation.

3. In case of confirmation of the need to conduct direct experimental tests of the basic laws under strong interactions by this independent person, as expected and following full scientific documentation, FERMILAB will either make available, or recommend informally, by next Spring one or more experimentalist, whether employed by Fermilab or not, who are familiar with the machines at FERMILAB. The committee of study indicated in my letter of July 11, 1981 will then be set up by our Institute, and will include this (or these) experimentalists, as well as any other scientist recommended (formally or informally) by FERMILAB.

Whatever your final decisions are, you can count on my best possible understanding. However, prior to reaching such a final decision, permit me to recommend that you meditate a moment on the implications for a lack of any action on the problem.

Sincerely,

Ruggero M. Santilli

RMS-ml

P.S. You might be interested in the following results which have occurred since my letter of July 11. This may give you an idea of the proliferation of studies in the sector in only one month.

1. Professor H. RAUCH (Director of the Atominstitut of Wien, Austria) and his collaborators have revised their measures on the spinor symmetry under strong nuclear interactions via neutron interferometers (Z. Physik B29, 281 (1978)). The old measure was 716.8 ± 3.8 deg, and, thus, it inluded the 720° needed for the exact SU(2)-spin symmetry (as reported in p.76 of my paper enclosed with my letter of July 11). The new measure is 715.87 ± 3.8 deg and, as such, it does not contain the 720°. This is the ONLY direct measurement of spin under strong interactions. It then follows that there exist no experiment at this moment which is capable of establishing

the exact character of the SU(2)-spin symmetry under strong interactions on true scientific grounds (that is, by excluding academic politics). The need to conduct new experiments is self-evident for all physicists in the field.

2. Professor G. EDER (author of a celebrated texbook in nuclear physics by The MIT Press) has completed extensive theoretical studies on the test of the SU(2)-spin symmetry via neutron interferometers.His results are numerous and each of them is substantial. First, he has proved that, even assuming the preservation of the conventional eigenvalues of the magnitude of the spin and of the third component, the SU(2)-spin symmetry can still be broken by non-Hamiltonian (as well as Hamiltonian) forces capable of distorting (mutating in our language) the charge distribution of the neutron. The breaking is quantitatively identifyable in the other components of spin, as well as in higher powers of the Casimir, and it is realized via the proposal of 1978 (replacement of the conventional associative envelope with a nonassociative, Lie-admissible, mutational algebra). After applying this generalized/broken SU(2)-spin structure to several aspects of nuclear physics, Prof. Eder concludes by pointing out that, when neutrons propagates through matter, the electromagnetic field produced by the electron clouds inside an atom is so strong that might produce a distorsion of the spin of the neutron of the measurable order of one per-cent (which is exactly in lines with the experimental results of point 1). For your information, at the time of my letter to you of July 11, 1981, we had doubts on the measurability of the mutation of spin via neutron interferometers because we considered only the nucleon-nuclei interactions. In short, not only the available direct experiments do no reproduce the conventional theory of spin, but the deviations are in full agreement with the theory.

3. Professor R. MIGNANI (of the Theoretical Physics Institute of the University of Rome, Italy, and well known for his studies on unitary symmetries for hadrons) has recently identified the foundations for a nonpotential scattering theory, including the basic lines for the computation of the cross section under contact effects due to mutual wave overlapping. Predictably, the construction appears to be a theoretical version of the experiment by Conzett et al on the violation of the T-symmetry. The theory appears to confirm, this time from a scattering profile, the expectation that under mutual penetration of wavepackets, the entire Poincaré symmetry (connected AND discrete part) is broken. The orthodox physicist will predictably have a difficult time in reaching credible dismissals. In fact, the theory is of direct interest to experimenters, and embodies basic physical notions which are so natural in all branches of physics except high energy physics. In fact, we must violate the T-symmetry in Newtonian mechanics to prevent perpetual-motion-approximations. The symmetry must be violated in classical and quantum statistical mechanics for a number of reasons which are well known. Orthodox physicists claim that the T-symmetry is misteriously restored in certain segments of particle physics. We respect this view. However, to prevent delicate administrative implications, the use of scientific authority should be prevented as much as possible, extensive investments on experiments based on the assumption of the exact symmetry should be avoided for the time being, and the issue should be resolved experimentally one way or the other in due time.

Several additional developments have occurred during this single month by mathematicians and physicists from the U.S.A., France, Switzerland, Israel, Italy, Sweden, West Germany, and other Countries. They are simply too numerous and too complex, to be indicated in a letter.

UNIVERSITÉ DE LAUSANNE
INSTITUT DE PHYSIQUE NUCLÉAIRE
Bâtiment des sciences physiques
Dorigny
CH 1015 LAUSANNE
Tél. (021) 24 00 46

Lausanne, le July 15th 1981

Professor R. M. SANTILLI
The Institute for Basic Research
Harvard Grounds
96, Prescott Street

CAMBRIDGE - Massachusetts 02138

U S A

Dear Professor Santilli,

Thank you very much for sending me the poster for the Orleans Conference and your preprint TP-DE-81-3. Here after are the addresses of the physicists which you requested :

Prof. E. Leader, Westfield College, Kidderpore Av.
London NW3 7ST, UK.

Prof. J. Bienlein, DESY, Notkestr. 85, 2000 Hamburg 52, Germany

Prof. L. Madansky, John Hopkins University, Baltimore MD 21218

Prof. B. Montagne, CERN, 1211 Geneva, Switzerland

Prof. Ch. Prescott, SLAC, P.O. Box 4349, Stanford CA 94305

I am entirely at your disposal for further information on the 1980 Lausanne Conference on spin physics and remain

Sincerely yours

Trân Minh-Tâm



1980 International Symposium on High-Energy Physics with Polarized Beams and Polarized Targets

Lausanne (Switzerland)
25 September – 1 October 1980

IS SPIN PHYSICS WORTHWHILE ?

Round table discussion

held during the

1980 INTERNATIONAL SYMPOSIUM ON HIGH-ENERGY PHYSICS
WITH POLARIZED BEAMS AND POLARIZED TARGETS

Lausanne 25 September - 1 October

Summary
prepared by

M. Jacob, CERN, Geneva,

Moderator of the discussion

Scientific secretary: M. T. Tran, Lausanne

Organizing committee:
C. Joseph, Lausanne, R. Hess, Genève, J. Soffer, CERN, E. Heer, Genève, L. Dick, CERN, B. Montague, CERN, D. Möhl, CERN, T. Nilnikoski, CERN, S. Mango, SIN

Organized jointly by the Universities of Geneva and Lausanne

Contact: C. Joseph Institut de Physique Nucléaire CH-1015 Lausanne/Switzerland Phone 021/46 23 65 Telex 25 199
R. Hess Département de Physique Nucléaire et Corpusculaire 32, Bd d'Yvoy CH-1211 Genève 4 022/21 93 55

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

October 21, 1981 Office of the President

Professor Ch. PRESCOTT,
Stanford Linear Accelerator Center
STANFORD, California 94305

with copy of this letter to:

Professor L. Madansky, John Hopkins University, Baltimore, Md
Professor E. Leader, Westfield College, London, England
Professor J. Bienlein, DESY, Hamburg, West Germany
Professor G. Montagna, CERN, Geneva, Switzerland

Dear Professor Prescott,

I have read with interest your paper "IS SPIN PHYSICS WORTHWILE?", from the Round Table Discussion held during the 1980 International Symposium on High Energy Physics with Polarized Beams and Polarized Targets (Lausanne, Sept. 25-Oct.1, 1980). Copy of the report, jointly with your addresses were kindly mailed to me by Dr. TRAN MINH-TAM of the Institut de Physique Théorique de l'Université de Lausanne, who is here gratefully acknowledged.

I have found the various sessions of your Round Table intriguing and sound. Nevertheless, permit me to bring to your attention a rather important aspect of spin physics of which you are, apparently, not aware of. It consists of the problem whether the conventional atomic notion of spin is exact or only cr dely approximative in the transition from the electromagnetic interactions (for which the notion was conceived and it still is experimentally testes today), to the different physical arena of the strong interactions. The problem is posed by the following layers of physical knowledge.

(1) For macroscopic bodies, it is well established since the past century that charge distributions and their magnetic moments exterience an alteraction under intense fields which generally implies a change of the intrinsic angular momentum, if any.

(2) In the transition to quantum mechanics, it has been equally well established since the early part of this century, that the charge distribution of atoms experiences an alteration under strong fields, which is essentially a form of "quantized version" of the classical one.

(3) In the transition to the deeper layer of particle physics, the situation is not equally well established at this moment. To my best knowledge, it appears that the Schmidt limits in nuclear physics suggest in a rather forceful way that, say, a proton experiences an alteraction of its magnetic moment in the transition from the condition of being the nucleus of an Hydrogen atom, to the different conditions of being a member of a nuclear structure, say, Pa. This possibility is self-evident also from chain (1)-(2) of physical laws, and it is indicated in well written treateses in nuclear physics. Permit me to stress, again, that the knowledge is not experimentally established in a final form. Nevertheless, we can say with confidence that the alteration of the magnetic moment is definitely the most probable and plausible, not only on intuitional grounds, but also on grounds of available data. If orthodox physicists prefer the preservation of the magnetic moment, to reach credibility by the scientific

community at large, they should provide convincing experimental evidence as well as theoretical argumentations proving that the chain of laws (1)-(2) suddenly breaks down at level (3).

The open character of the notion of spin under strong interactions is a direct consequence of the situation above of the magnetic moment. In particular, the following two schools of thought can be identified in the contemporary community of basic research.

SCHOOL 1: This first school assumes the exact validity of the conventional SU(2)-spin Lie symmetry under strong interactions, and constructs thereafter the theory in such a way to comply with the assumption. In particular, this implies the assumption that the spin stays the same even under alterations of the magnetic moment. More fundamentally, the assumption enters in the data elaboration of contemporary experiments under strong interactions in a general tacit way. The experiments, therefore, simply cannot test the assumption.

SCHOOL 2: This second school (of which I am a member) makes no aprioristic assumptions of experimentally unverified knoweldge, particularly of fundamental character such as this one. The research attitude instead is that of identifying all most probable possibilities and letting the experiments solve the issue. When hadrons are accepted as they actually are (extended charge distributions), an alteration of their magnetic moment is expected to imply, most likely, that of spin with a possible, consequential breaking of the SU(2)-spin symmetry under strong interactions. At any rate, the same conclusion can be reached via a number of equivalent arguments. First, the construction of a theoretical model whereby the magnetic moment of the hadrons changes under strong interactions, but the spin stays the same is extremely difficult to realize owing to a host of consistency problems (the assumption is, again, that the hadron is not conceived as a point, and, also, that the issue is not left at the level of words, but actual calculations are conducted). Second, spin was conceived by the Founders of atomic mechanics for isolated particles in vacuum under long range electromagnetic interactions, while we have here a fundamentally different physical arena (wave packets in conditions of mutual penetration and overlapping). The most probable case is therefore that deep wave overlappings create interferences in the intrinsic angular momentum, resulting in the need of a more general theory. Third, it has been recently shown in the literature that, if the strong interactions have a longly claimed nonpotential nonlocal term, the SU(2)-spin symmetry breaks down at its central part, the enveloping algebra; similarly, it has been proved that, even when the third component and the magnitude of a spin are the conventional ones, the SU(2)-spin symmetry can be grossly broken because of deformations/distorsions resulting in the other components as well as in the higher-order Casimirs; etc.

But, perhaps, the most intriguing information is that of experimental nature. Apparently, the only available experiment capable of directly measuring spin-related quantities under joint strong and electromagnetic interactions is the fundamental experiment by Rauch and his associates of the Atominstitut of Wien on the spinor symmetry of neutron wave functions via neutron interferometers. The expected measurement for the 4π symmetry (two complete spin flips) was, of course, 720 deg. The best measure (conducted in 1978) was 716 ± 3.8 deg, while recent updatings due to new physical data give the value of 715.8 ± 3.8 deg. As you can see, the value 720 deg IS NOT part of the best available measures. The problem of the spin under strong interactions is therefore open at this time.

Needless to say, a problem of this nature must be solved with the participation of the scientific community at large. In the hope of contributing toward this goal, we have organized the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS which will be held at the Université d'Orléans, France, from January 5 to 9, 1981, as per enclosed announcement. A number of experimentalists, theoreticians, and mathematicians will present aspects directly or indirectly related to the problem, beginning with talks at the Newtonian level, then passing to the statistical level, and finally entering into the realm of particle physics.

I am here formally inviting you, and/or your colleagues Professors MADANSKY, LEADER, BIENLEIN and MONTAGNA to participate to this Conference, by presenting your view, whether in favor or against the preservation of the atomic notion of spin, and whether of theoretical or experimental orientation.

In particular, you can participate by either
- in person, in which case please contact the organizational office at Orléans as soon as possible, as the closing date for registration is near. In this case, please also let us know whether you are interested in delivering a talk either on the subject, or in a related topic.
  Or, in case you cannot attend,
- by mailing a contributed paper for the Proceedings of the Conference as a corresponding participant.

A registration form is enclosed for your convenience. To provide assistance in reaching your personal view on the topic, I have separately mailed to each of you one copy of a collection of seven articles prepared by our Institute entitled
"Primary bibliography on the problem of the exact or approximate validity of the SU(2)-spin symmetry under strong interactions".
while I remain at your disposal for any additional assistance you might need.

Needless to say, whenever any of you will be in Cambridge, you would be sincerely welcome to visit our new Institute.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics and
President

RMS-pm
encls.
cc.: Professors J. FRONTEAU and A. TELLEZ-ARENAS, Univ. of Orléans, France and Dr.TRAN MINH-TAM, Univ. of Lausanne, France.

HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

October 28, 1982

Professor CHRIS QUIGG
Head, Theoretical Physics
FERMILAB
P. O. Box 500
BATAVIA, Illinois 60510

Dear Professor Quigg,

FERMILAB is renowed for the completeness of its libraries, with particular reference to its subscriptions to technical Journals in physics and mathematics. Yet, your laboratories do not subscribe to the HADRONIC JOURNAL, despite the fact that our Journal has now entered the sixth year of regular and successful publication, and that it is now an established vehicle of research with a fast growing number of subscribers all over the world. It is evident that your physics library IS NOT COMPLETE without the Hadronic Journal.

Every year since 1978, we have mailed to your department, as well as to the general libraries at your laboratories, information about our Journal. As you know, our Journal is the forerunner in the promotion of experimental, theoretical, and mathematical knowledge on the rather fundamental physical problem whether the [extended] charge distribution of hadrons is perfectly rigid under strong interactions, or it experiences small deformations. In this latter case, we would have departures from the exact character of the rotational symmetry, with far reaching implications, not only for basic research at large, but also for important aspects of National interests, such as the impact on controlled fusion. In turn, implications of this nature, once matched with the plausibility of the deformations, render the study of the problem simply mandatory, particularly when the use of public funds is involved, with consequential ethical needs for scientific accountability.

It is public knowledge that your physicists are continuing the conduction of research and the publication of articles with the tacit assumption of the perfectly rigid charge distribution of hadrons [i.e., of the exact rotational symmetry], and are continuing to use public funds along these lines, despite the now established conjectural character of the basic assumptions.

It has been brought to our attention that your laboratories have not subscribed to the HADRONIC JOURNAL until now apparently because of the opposition by individual members of your department, rather than because of financial difficulties.

If this is the case, permit me to bring to your attention the fact that such an occurrence:

[1] would imply the suppression of valuable scientific information at your campus in the interests of a minoritarian group;

[2] would infringe on the rights of library users at large, with particular reference to graduate students and researchers; and, last but not least,

[3] would raise the possibility of discrimination of research at FERMILAB under governmental support.

We enclose for your information a list of articles published in all volumes of the HADRONIC JOURNAL until 1978, as well as front pages and table of contents of international workshops and conferences which are part of the Journal's scientific activities. We hope you can see in this way the number of distinguished scientists who have contributed to our Journal, as well as the number of governments who are supporting nowadays studies on the experimental verification of conventional physical laws under strong interactions.

If we can be of any assistance, please do not hesitate to let us know.

Very truly yours,

C. G. Gandiglio
President
HADRONIC PRESS, INC.

cc: Professor R. M. SANTILLI, Editor in Chief, Hadronic Journal, I.B.R., Cambridge, Massachusetts

Professor L. LEDERMAN, Director, FERMILAB

CGG/mlw

Enclosures

PART XI:

TACUP

COMMITTEE

UNIVERSITY OF CALIFORNIA, LOS ANGELES

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SANTA BARBARA · SANTA CRUZ

DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

December 8, 1982

Dear Colleagues:

As you probably know, the High Energy Physics Division of DOE has appointed a Technical Assessment Committee for University Programs (TACUP) and requested that TACUP undertake a review of DOE's University-based high energy physics program.

The charge to TACUP has two major parts. The first of these is an evaluation of the individual Tasks and Contracts. This review is being carried by eight Technical Panels, three in theory and five for experimental work, whose activities have already commenced. A list of the Panel members of TACUP is attached.

The second part of the TACUP charge is a broad analysis of the University-based high energy physics program with emphasis on questions of balance between various research components, on current problems and on future opportunities. The Panels will also participate in this second task after the review of individual Contracts is completed. The charge to TACUP dealing with the overall evaluation of the program reads as follows:

(2) Review the DOE university research program in the context of the entire HEP program including the following:
   (a) The appropriate role and scope of the program of university research.
   (b) The appropriate balance of research efforts such as: experiment and theory, accelerator and non-accelerator experiments; experiments using U. S. accelerators and foreign accelerators; large and small efforts; speculative and precision experiments; etc.
   (c) Present and foreseen university research problems and opportunities.
   (d) Outlook for the future.

The response of TACUP to this part of the charge should reflect not only the views of the TACUP members but, preferably, of the high energy physics community at large. Accordingly, I am writing to you as a member of DPF to solicit your views and comments on any topic relevant to this charge. Principal Investigators and Task leaders have already been invited to comment in the context of the Questionnaires which they were asked to complete. This letter is intended to elicit a broader range of views.

TACUP will greatly appreciate your suggestions and comments. If you care to respond please send your letter to me before December 30, 1982.

Best wishes,

Harold K. Ticho
TACUP Chair

HKT:chs
Enclosure

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

December 19, 1982

Professor HAROLD K. TICHO
Department of Physics
University of California
LOS ANGELES, California 90024

URGENT — FEDERAL EXPRESS MAIL

Dear Professor Ticho,

We would like to prepare for your TACUP Committee a comprehensive report on certain basic aspects of theoretical physics (gravitation, particle, and nuclear physics), and of experimental physics (high energy and nuclear physics). In particular, we would like to indicate to your Committee for consideration, certain beautiful, potential, fundamental advances that, in our view, are within reach of the DOE with current financial and human resources. But, for us to do so, we need the following information.

[1] Is TACUP only an advisory committee? or is it invested with decision-making authority?

[2] Which are the guidelines for evaluation? Did you devise an evaluation instrument? and, if so, is the instrument available to us?

[3] Will TACUP report only to DOE? or will TACUP report also to other governmental bodies?

[4] Which are the deadlines for the completion of [4a] existing contracts and [4b] of the entire HEP program?

[5] Will the final TACUP report be available to the public? and, if so, when, where, and how can it be obtained?

[6] Is the review limited to currently executed contracts? or does it include proposals under consideration?

[7] To achieve sufficient maturity, including reviews prior to your submission, we have difficulties to complete our report prior to the second half of January 1983 (I believe that several other colleagues will have similar difficulties owing to shortness of the notice and its occurrence within the holyday period). Kindly indicate whether the delivery of our report on general HEP aspects in the second half of January 1983 is acceptable.

A written replay to the above questions would be gratefully appreciated, possibly be Federal Express (to avoid its possible arrival after your deadline because of the current state of mail).

Wishing you the best Season Greetings, I remain

Yours, Sincerely

Ruggero M. Santilli
President
RMS-mlw cc: Drs. WALLENMEYER, HILDEBRAND, and THEWS, DOE

P.S. I am the principal investigator of DOE Contract No. DE-AC02-80ER10651.A002. However, I did not receive the questionnaire indicated in your letter. Kindly let me have a copy, in case I am entitled to. Thank you.

I enclose as a gesture of courtesy general information on our recently organized institute.

UNIVERSITY OF CALIFORNIA, LOS ANGELES UCLA

BERKELEY · DAVIS · IRVINE · LOS ANGELES · RIVERSIDE · SAN DIEGO · SAN FRANCISCO SANTA BARBARA · SANTA CRUZ

DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

December 27, 1982

Dr. Ruggero M. Santilli, President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Thank you for your letter of December 19, 1982, and the attachments.

When TACUP was set up, DOE provided us with a list of programs to be reviewed. Your Contract was not among those so flagged and for this reason it was not included in our reviewing plans. I would suggest that you contact the DOE program monitors for clarification.

Sincerely,

Harold K. Ticho, Chair
TACUP

HKT:chs

cc: Dr. B. Hildebrand

DOE F 1325.8
(7-79)

U.S. DEPARTMENT OF ENERGY

memorandum

DATE January 17, 1983

REPLY TO
ATTN OF

SUBJECT Next Meeting of HEPAP, February 7-8, 1983

TO Distribution

The next meeting of the High Energy Physics Advisory Panel is scheduled for February 7-8, 1983, at DOE Headquarters in Germantown, MD, Room A-410. The meeting will run from 9am to 6pm on February 7, and from 9am to 4pm on February 8. Tentative agenda items include: Discussion of the FY 1983 DOE/HEP Continuing Resolution and the NSF/EPP Budget; Discussion of the FY 1984 Presidential Request Budgets (if available) for DOE/HEP and NSF/EPP; Discussion of the planned 1983 Subpanel on Future HEP Facilities; Discussion of progress of the technical assessment panels for university programs and non-accelerator experiments; and Status of international bilateral agreements in HEP.

John Wallenmeyer

for P.K. Williams
Executive Secretary
High Energy Physics Advisory Panel

Tentative Agenda
High Energy Physics Advisory Panel
U.S. Department of Energy
Germantown, MD
February 7-8, 1983

Monday, February 7, 1983

9:00am	Administrative
9:15am	Discussion of NSF/EPP FY 1983 Budget and FY 1984 Presidential Request Budget*, D. Berley
10:00am	Discussion of DOE/HEP FY 1983 Continuing Resolution and FY 1984 Presidential Request Budget*, W. Wallenmeyer
11:00am	Break
11:15am	Brief Discussion of U.S. Program Abroad, B. Hildebrand
11:45am	Discussion of Budget Matters Relating to FY 1983 and FY 1984
1:15pm	Lunch
2:00pm	Discussion of the 1983 Subpanel on Future HEP Facilities
4:00pm	Break
4:15pm	Further Discussion of FY 1983 and FY 1984 Budget Matters
6:00pm	Adjourn

* Subject to availability of the FY 1984 Presidential Request Budget

Tuesday, February 8, 1983

9:00am	Administrative
9:05am	Status of International Bilateral Agreements in High Energy Physics, B. Hildebrand, E. Fowler
9:45am	Small Business Innovation Research Program, W. Wallenmeyer
10:00am	Report of the Experimental Technical Assessment Panel (Proton Decay Experiments), R. Adair
11:00am	Break
11:15am	Status of the Technical Assessment Panel on University Programs, R. Ticho
12:15pm	Further Discussion on 1983 Facilities Subpanel
1:15pm	Lunch
2:00pm	Further Discussion on Budget Matters and 1983 Facilities Subpanel
4:00pm	Adjourn

I. B. R.
THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

January 22, 1983

Dear Professor Ticho,

I am contacting you on a personal and confidential basis in the hope that you can provide us with advice and council on a problem of contemporary high energy physics we believe of particular fundamental character:

> The experimental resolution at National Laboratories of the exact or only approximate validity of Einstein's special relativity for the interior of systems with strong forces

I enclose a general presentation which, upon suitable modification, was intended for your TACUP committee. Since our contract has not been flagged, and following consultation with Bernie Hildebrand, we have absteined from submitting any formal report.

Nevertheless, we believe that the problem considered persists. I hope that the enclosed presentation, even though non-technical, possesses sufficiently diversified information to show that the problem, besides being manifestly intriguing, is indeed within experimental, theoretical, and mathematical reach.

Our old idea on how to proceed is as collegial as possible, and consists of the setting up of a Review Panel with the tasks, among others, of:

[1] Identifying the pitfalls of the current arguments intended in the hope of nullifying the need of the tests (see Section 5.3 of the enclosed report for an outline);

[2] Identifying all available proposals of direct tests of the special relativity under strong interactions, and assessing their feasibility (such as Kim's proposal to measure the meanlife of unstable hadrons -not leptons- in flight at different energies, as briefly touched in Part 3);

[3] Identifying the equipments available at National Laboratories which appear to be most suited for the tests, by keeping in mind that we are referring here to the new experimental challange of reaching actual measures under external strong interactions;

[4] Identifying additional equipments that might be suitable for recommendation particularly for low energy, high sensitivity experiments as apparently needed for some of the tests;

[5] Pointing out some of the theoretical aspects that deserve further developments as a pre-requisite for true advances in the tests (such as Mignani's studies on the nonpotential generalization of the potential scattering theory currently used in the data elaboration at National Laboratories).

I have attempted to recommend the setting up of such a Review Panel to Panofski, Lederman, and Vineyard (when director of Brookhaven) for a number of years without any success. Their formal position is that they have no internal rules for the setting up of panels of study (sic!).

The true reason, of course, is that the experimental verification of the special relativity under strong interactions is vigorously (at times histerically) opposed by well known, vested, academic interests.

With the passing of time, however, I believe that the situation is getting more and more serious for evident reasons of scientific accountability vis -a-vis the taxpayer. In fact, we are all spending large amounts of public funds in strong interactions. Most of these funds are spent on the mere belief of the exact validity for the strong interactions of basic physical laws clearly and directly established only for the electromagnetic inte-

Outline of the

THEORETICAL, EXPERIMENTAL, AND MATHEMATICAL STUDIES CONDUCTED AT

THE INSTITUTE FOR BASIC RESEARCH
Cambridge, Massachusetts

TOWARD A GENERALIZATION OF GALILEI'S AND EINSTEIN'S RELATIVITIES IN CLASSICAL AND QUANTUM MECHANICS

January 1983

ractions. Scientific accountability demands that we de-emphasize all our personal theoretical views, whether in favor of old laws or in favor of suitable generalizations, and resolve the issue in the only scientifically possible way: via direct experiments. The suppression of such resolution which has been successfully achieved by vested interests until now has grossly aggravated the situation and rendered . the initiation of preliminary collegial studies simply mandatory, in my view.

My problem is to promote such scientific process in a way as smooth as possible, and by avoiding public confrontations as much as possible (without such a committment to an orderly process you would have likely read the issue in national newsmedia). The understanding is that vested interests are capable of moderation.

To succeed in such orderly scientific objective I need council and advice. This is the reason I am contacting you. Some of the points in which any suggestion would be gratefully appreciated (even by phone) are the following.

A. Do you foresee possibilities of an informal and confidential consideration of the possibility of setting up the above Review Panel (or some other alternative) at the forthcoming H.E.P. Advisory Panel meeting in Washington on Feb. 7-8?

B. Do you believe that my presence at such a meeting might be recommendable?

C. Willy Wallenmeyer and Bernie Hildebrand at DOE are fully informed of the studies. In fact, they have been conducted under their support since the initiation at Harvard back in 1978. Nevertheless, I have never disturbed Willy and Bernie for assistance in the initiation of studies at National Laboratories, owing to the mostly human (at times questionably human) aspect of the issue (I thought that they have already sufficient administrative duties and responsibilities to keep our community together in difficult financial times to warrant my additional burden). Nevertheless, do you think that the proposal of a Review Panel or other alternatives should be submitted to Willy and Bernie?

D. Do you know any physicist at National Laboratories sufficiently independent from existing vested interests and with sufficient committment to fundamental issues to consider some help in a collegial approach to the problem?

E. Do you think that a formal presentation of the need for the verification of the special relativity under strong interactions because of manifest need of scientific accountability vis-a-vis our society, is recommendable for submission to the current H.E.P. Review Panel organized by the White House and/or other receptive governmental members and/or committees?

Thanking for your courtesy and time, and hoping to have the pleasure of meeting you in the near future, I remain

Sincerely Yours

R.M. Santilli

THIS LETTER WAS NOT ACKNOWLEDGED

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I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

March 1, 1983

Professor W.K.H.PANOFSKY
SLAC
Stanford, California

Dear Professor Panofsky,

It would be a pleasure to see you during your forthcoming visit at Harvard next week.

In fact, a friendly meeting (possibly outside Harvard) would be the ideal oppurtunity to exchange ideas on the orderly approach to the problem of the experimental test at national laboratory of the Lorentz symmetry under strong interactions.

In case you are interested in the issue and have the time, simply call me at any time.

Sincerely,

Ruggero Santilli

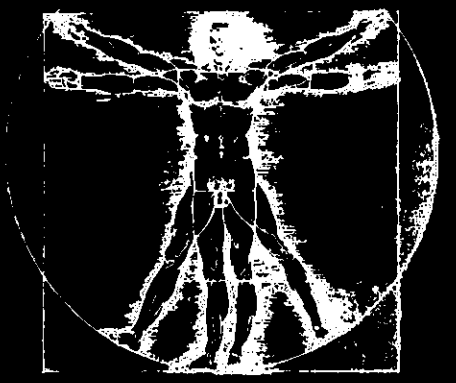
VOLUME

II

DOCUMENTATION

OF

IL GRANDE GRIDO





Ruggero Maria Santilli

DOCUMENTATION

OF

IL GRANDE GRIDO

Volume II

Ruggero Maria Santilli

— 1984 —
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and/or to individual scholars, for the continuation of the research described in Chapter 1.

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OF
IL GRANDE GRIDO
VOLUME II
by
Ruggero Maria Santilli
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EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH, GENEVA, SWITZERLAND, AND DEUTSCHES ELEKTRONEN–SYNCHROTRON, HAMNURG, WEST GERMANY

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

SIÈGE: GENÈVE, SUISSE

Adresse postale/Postal address

CERN
CH 1211 GENÈVE 23
SUISSE/SWITZERLAND

TÉLEX: 23698 CH
TÉLÉGRAMMES: CERNLAB-GENÈVE

TÉLÉPHONE: GENÈVE (022)
Direct: 834473 /834471 /834472
Central/Exchange: 83 61 11

Geneva, January 31, 1978

Dr. R. SANTILLI
Harvard University
Lyman Laboratory of Physics

CAMBRIDGE - MASS.

02138 USA

Votre/Your ref.

Notre/Our ref.
PE/ED/FA/186

Dear Dr. Santilli,

We acknowledge receipt of your application for a Scientific Associate appointment.

This will be considered at the next meeting of the Selection Committee on April 11, 1978.

Candidates will be informed of the results of their applications during the ten days following the meeting.

Yours sincerely,

W. Blair

Head, Fellows and
Associates Service

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 14, 1978

Professor W. BLAIR,
Head, Fellows and Associates Services
CERN
CH-1211 GENEVA 23 Switzerland

Dear Professor Blair,

I would like to express my appreciation for the courtesy of your letter of January 31, 1978 indicating that my application for a Scientific Associate Appointment will be considered at the meeting of April 11, 1978.

In this respect I would like to indicate that a recent grant application with Professor Shlomo Sternberg, Chairman of the Department of Mathematics here at Harvard to the U.S. Department of Energy (formerly ERDA) has been recently funded. As a result, I will have financial support for the next two academic years.

Owing to this new occurrence, I would like to confirm my application for a scientific associateship appointment, but modify my application for an appointment without salary. Whether possible some travel assistance would be welcome.

The reason for my interest in such an appointment is the following. I have been involved since some time in the study of the old idea that the strong interactions in general and the strong hadronic forces in particular are not derivable from a potential. The transition from the conventionally used forces derivable from a potential to the indicated broader form has a number of implications, particularly on mathematodological grounds.

The ultimate objective of these studies is to stress the need of subjecting to an experimental verification the validity within a hadron of those relativity and quantum mechanical laws (Pauli principle in particular) which have proved to be so effective for the atomic (as well as nuclear) structure. After all, the historical occurrence of the invalidity of previously established methods for the structure of the atoms or the more recent, equally historical discovery of parity violation, should not be ignored.

In essence, it appears that at a theoretical level the issue cannot be resolved beyond the level of personal opinions and conjectures which in any case remain far from a scientific truth. The only physically effective resolution of the issue is, in due time, via experiments.

The HADRONIC JOURNAL, of which you are eventually aware, has been organized in this spirit: to promote scientific debates on fundamental issues, in the traditional spirit of unsolved physical problems.

Clearly, the issue I am referring to goes considerably beyond my capabilities as an isolated researcher. My interest in a scientific associateship at CERN is therefore twofold: I would like first attempt to stimulate the awareness of CERN collegues on the need to conduct the indicated experimental verification, in due time. Secondly, I would like to collect the personal viewpoints of experimentalists (on the technical difficulties for a possible verification) as well as theoreticians (on the reasons for or against such an experimental verification).

Very Truly Yours

Ruggero Maria Santilli

RMS|is

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

SIÈGE: GENÈVE, SUISSE

Adresse postale/Postal address

CERN
CH 1211 GENÈVE 23
SUISSE/SWITZERLAND

TÉLEX: 23698 CH
TÉLÉGRAMMES: CERNLAB-GENÈVE

TÉLÉPHONE: GENÈVE (022)
Direct: 83 4471 /83 4472 /83 4473
Central/Exchange: 83 61 11

Geneva, April 18, 1978

Professor Ruggero SANTILLI
Lyman Laboratory of Physics
Harvard University
Cambridge, Mass. 023138
Etats-Unis

Votre/Your ref.

Notre/Our ref.
PE/PM/FA/613

Dear Professor Santilli,

Your application for an appointment as Scientific Associate at CERN was considered at a meeting of the Selection Committee held on April 11, 1978. Your letter of March 14, 1978 was brought to the attention of the Committee.

The members of the Committee asked me to give you the following information. The budget and space available were very limited, and the number of applications received was exceedingly high. In these circumstances the Committee unfortunately was unable to offer you an appointment.

Yours sincerely,

W. Blair
Head, Fellows and Associates Service

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 15, 1978

Professor GEORGES CHARPAK
Experimental High Energy Physics
CERN
CH-1211 GENEVA 23, Switzerland

Dear Professor Charpak,

I am inviting you to take an active partecipation in the efforts recently initiated at the HADRONIC JOURNAL in relation of the experimental verification of the validity or invalidity for the strong interactions of established physical laws (Pauli's principle and Einstein's special relativity, in particular).

You are familiar with the current line of studies based on(the tacit assumption of) the validity of these basic laws for the strong interactions. I am here referring to quark oriented studies, including QCD. You are perhaps also familiar with the increasing concern by an increasing segment of our community in relation to the fact that, despite truly large investments over a rather long period of time, the fundamental problematic aspects of these studies have not been resolved and, according to the view of a group of physicists, are actually increasing in time.

I do not know whether your are aware of the fact that there exist a number of physicists in USA, Europe, Japan and other Countries who are actively working on the violation of basic physical laws for the strong interactions and the search for conceivable generalizations. This is, first of all, a clear expression of the fact that the laws considered simply do not have at this time an experimental backing of any relevance for the case of the strong interactions. Secondly, this occurrence, appears to be an expression of a rather profound dissatisfaction with respect to the actual physical effectiveness of these laws for the interactions considered, as compared to the fascinating physical effectiveness of the same laws when applied to the electromagnetic interactions. As editor of the HADRONIC JOURNAL, I have been particularly exposed to this scientific current and I believe you might be interested in its existence.

In essence, I have no words to express my personal concern on the current status of hadron physics. It appears that the situation is not only at the stage of mere opinions, but actually in limbo and will likely remain in limbo until the problem of the basic physical laws is seriously confronted by the experimentalists and, in due time, resolved.

I enclose copy of a paper by Professor D.Y.KIM (now at Cambridge, England) on a review-comment of the problem. This paper also contains the most relevant references which are apparently available at this time. In case you need additional copies and/or other material, please let me know.

I would appreciate your inspection of Kim's review and your assessement of the current state of the art by theoreticians on the identification of currently feasable proposals of specific experiments.

Since I am not an experimentalist, I am unable to achieve such an assessement. I am, however, fully aware that we are at the very first steps of an expected long and laborious scientific process. I am also aware that the current state of the art is indeed rudimentary. But for the problem considered I believe in the traditional scientific process of trial and error, presentation of ideas and critical inspection by independent researchers.

In case you are interested, I would be happy to provide a more detailed presentation of my view, with a differentiation with respect to hadron and nuclear physics and with respect to relativity and quantum mechanical laws. In the final analysis, all these aspects appear to be related.

I did enjoy reading your article in the recent issue of the PHYSICS TODAY and I sincerely hope that "multiwire and drift proportional chambers" can some day also be used for truly fundamental experimental verifications, in addition to the valuable applications currently under way.

Very Truly Yours

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

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HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

November 15, 1978

Professor WILLIAM J. WILLIS, Head
Isabelle Detector Division
Brookhaven National Laboratory
UPTON, Long Island, New York 11973

Dear Professor Willis,

I am inviting you to take an active partecipation in the efforts recently initiated at the HADRONIC JOURNAL for the promotion of the experimental verification of the validity or invalidity for the strong interactions of the basic physical laws experimentally established for the electromagnetic interactions, with particular refrence to Einstein's special relativity and Pauli's exclusion principle.

You are aware of the current line of theoretical studies based on the (tacit) assumption of the validity of these laws for the strong interactions. I am here referring to quark-oriented studies, including QCD.

Perhaps, you are also aware of the increasing concern by an increasing segment of our community of the fact that, despite truly large investments over a rather long period of time, the fundamental problematic aspects of these studies have not been resolved and, according to a group of physicists, are actually increasing in time.

I do not know whether you are aware of the existence of a significant number of qualified physicists in the USA, Europe, Japan and other Countries who are actively working on the violation of the laws considered in the arena considered, and on the search for possible generalized laws.

As editor of the HADRONIC JOURNAL I have been particularly exposed to this new scientific current and I believe you might be interested in knowing its existence.

The overall picture of theoretical hadron physics which emerges from this situation is rather distressing and such to call for genuine concern by physicists genuinely interested in the pursuit of fundamental human knowledge. In candid language, we are not only at the level of mere opinions by individual or group of researchers either in favor or against basic physical aspects, but actually the entire theoretical efforts of this sector are IN LIMBO and WILL REMAIN IN LIMBO UNTIL THE PROBLEM OF THE BASIC PHYSICAL LAWS IS SERIOUSLY CONSIDERED BY EXPERIMENTALISTS AND, IN DUE TIME, RESOLVED IN UNEQUIVOCAL TERMS.

I enclose copy of a paper by Professor D.Y.KIM (now at Cambridge-England) on a review-comment of the subject. This paper also contains the pertinent references generally known at this time. In case you need additional copies and/or other material, please let me know.

I would appreciate the inspection and assessement of KIM's analysis by you and/or some of your associates. I am particularly interested in knowing whether the specific experiments which have already been proposed (via measurement of the mean life of unstable particles) are actually feasable at this time and, if so, whether they are actually valuable for the problem of Einstein's special relativity under strong interactions. Since I am not an experimentalist, I am unable to reach this assessement.

The problem of Pauli's principle (and other quantum mechanical laws) under the same interactions appears to be complementary and, in the final analysis, deeply related to that of the relativity profile.

In its simplest possible form, the intriguing scientific controversy under way is the following. If the hadronic constituents are assumed as point-like, established laws are expected to apply in full. Quark-oriented studies are then consequential to a considerable extent. On the contrary, if the hadronic constituents are interpreted as charged, massive and NON-point-like particles, they result in a state of penetration of their charge volumes while within a hadron. Studies of this rather peculiar occurrence (absent in the atomic and most of the nuclear setting) indicate the necessary presence of strong forces more general than those derivable from a potential (variationally nonselfadjoint strong hadronic forces). In turn, these broader forces appear such to produce a breaking of the SU(2)-SPIN. Still in turn, such a breaking has such a fundamental character, to JOINTLY render inapplicable established quantum mechanical and relativity laws. In conclusion, experiments on the relativity profile are expected to have an "image" or counterpart of dynamical character as far as the quantum mechanical laws are concerned.

I did read with sincere pleasure and interest your excellent article in the recent issue of PHYSICS TODAY. Permit me the liberty of expressing the hope that "the large spectrometers" may some day be used also for the experimental verification of fundamental physical laws.

Very Truly Yours

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

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HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 15, 1978

Professor JACK SANDWEISS, Chairman
Department of Physics
Yale University
NEW HAVEN, Connecticut 06520

Dear Professor Sandweiss,

I am inviting you to take an active partecipation in the promotional efforts recently initiated at the HADRONIC JOURNAL in relation to the experimental verification of the validity or invalidity for the strong interactions of the fundamental physical laws experimentally established for the electromagnetic interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

You are aware of the current line of theoretical studies on strong interactions and hadron structure which are based on the often TACIT ASSUMPTION of the validity of the laws considered in the arena considered. I am here referring to quark-oriented studies, including QCD.

Perhaps, you are also aware of the increasing concern by an increasing segment of our community on the fact that, despite truly large financial investments over a rather long period of time, the fundamental problematic aspects of the quark models have not been resolved and, as a matter of fact, are increasing in time according to the view of a group of physicists.

I do not know whether you are aware of the fact that there exist nowadays a significant group of qualified physicists in the USA, Europe, Japan and other Countries who are actively working on the VIOLATION of the laws considered in the arena considered, and are searching for conceivable covering laws.

As Editor of the HADRONIC JOURNAL I have been particularly exposed to this scientific current and I believe you might be interested in knowing its existence.

The overall picture of theoretical hadron physics which emerges from this situation is rather distressing and such to call for genuine concern by physicists with a genuine interest in the pursuit of fundamental human knowledge. In candid language, we are not only at the level of mere OPINIONS by individual or group of physicists, but actually, in my view, THE ENTIRE THEORETICAL EFFORTS ON STRONG INTERACTIONS AND HADRON STRUCTURE ARE CURRENTLY IN LIMBO AND WILL REMAIN IN LIMBO UNTIL THE PROBLEM OF THE BASIC PHYSICAL LAWS IS SERIOUSLY CONFRONTED BY EXPERIMENTALISTS AND, IN DUE TIME, RESOLVED IN AN INCONTROVERTIBLE FORM.

Almost needless to say, I have encountered numerous oppositions (even in my own campus) against the very consideration of the issue. You can however rest assured that I intend to pursuit it until the experimental verifications under considerations become unavoidable.

I enclose copy of a recent paper by Professor D.Y.KIM (now in Cambridge-England) recently appeared in the October issue of the HADRONIC JOURNAL on a review-comment of the issue. This paper also contains the known references on the subject. In case you need additional information and or material, please do not hesitate to call me.

Trusting in your scientific vision and interest, I would be grateful for your inspection and assessement of KIM's analysis. I am particularly interested in knowing
- whether the proposed experiments (via measurements of mean lifes) are actually feasible with currently available technology; and, if so,
- whether they can actually contribute to the problem of the validity or invalidity of Einstein s special relativity at small distances; and, if not -whether alternative experiments are conceivable.

Since I am not an experimentalist, I am unable to reach such an assessment.

The problem of Pauli's principle and other quantum mechanical physics laws is expected to be complementary to that of Einstein's relativity, and viceversa (see Hadronic J. 1, 223 (1978), 1, 574 (1978) and 1, 1279 (1978)).

In its simplest possible form, the following intriguing scientific debate is under way. If the hadronic constituents (and all hadrons in general) are assumed as being point-like, the established relativity and quantum mechanical laws are expected to apply in full to the hadronic structure (and strong interactions in general). However, if the hadronic constituents are interpreted as being charged, massive and physical particles, i.e., non-point-like, they result to be in a state of penetration of their charge volumes while within a hadron. Studies of this rather peculiar situation (absent in the atomic and most of the nuclear settings) indicate the need of realizations of the strong interactions in terms of forces more general than those derivable from a potential (as in QCD), called variationally nonselfadjoint strong forces. In turn, these broader forces result to have such dynamical effects to imply the breaking of the SU(2)-SPIN (in the sense that the conventional notion of spin would be inapplicable, say, for a particle producted in the core of a neutron star). Still in turn, the breaking of the SU(2)-spin has such fundamental character to imply the JOINT INAPPLICABILITY of Einstein's special relativity and Pauli's principle . In conclusion, experiments on the relativity profile are expected to have a "dynamical image" as far as basic quantum mechanical laws are concerned.

I did read with sincere interest your excellent article on the recent issue of PHYSICS TODAY. In closing, permit me the liberty of expressing my personal hope that "the high-resolution streamer chamber" will some day be used for truly fundamental experimental applications.

Very Truly Yours

Ruggero Maria Santilli

RMS/cgg
encls.

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 15, 1978

Professor DAVID R. NYGREN
Lawrence Berkeley Laboratory
Berkeley, California 94720

Dear Professor Nygren,

I am inviting you to take an active partecipation in the recently initiated efforts at the HADRONIC JOURNAL for the experimental verification of the validity or invalidity for the strong interactions of the basic physical laws experimentally established for the electromagnetic interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle,

You are aware of the current line of theoretical studies on hadron structure which are based on the often tacit ASSUMPTION of the validity of the laws considered in the arena considered. I am here referring to quark-oriented studies, including QCD.

Perhaps, you are also aware of the increasing concern by an increasing segment of our community in relation to the fact that, despite truly large financial investments over a rather long period of time, the fundamental problematic aspects of the quark models have not been resolved and, according to some, are actually increasing in time.

I do not know whether you are aware of the existence of a significant number of qualified physicists in the USA, Europe, Japan and other Countries who are nowadays devoted to the study of the INVALIDITY of the laws considered for the strong interactions and to the search for possible covering laws.

As Editor of the HADRONIC JOURNAL I have been particularly exposed to this new scientific current and I believe you might be interested in knowing its existence.

The overall picture of theoretical hadron physics which emerges from this situation is rather distressing and such to call for genuine concern by physicists with genuine interest in the pursuit of fundamental human knowledge. In candid language, we are not only at the level of mere OPINIONS by individual or groups of researchers of this or that other inspiration, but actually, in my view, THE ENTIRE THEORETICAL EFFORTS ON HADRON STRUCTURE AND STRONG INTERACTIONS IN GENERAL ARE CURRENTLY IN LIMBO AND WILL REMAIN IN LIMBO UNTIL THE PROBLEM OF THE BASIC PHYSICAL LAWS IS SERIOUSLY CONSIDERED BY EXPERIMENTALISTS AND , IN DUE TIME, RESOLVED IN THE NEEDED INCONTROVERTIBLE FORM.

I enclose copy of a recent paper by Professor D.Y.KIM (now at Cambridge-England) appeared in the October issue of the HADRONIC JOURNAL on a review-comment of the issue, with a valuable reference list. In case you need additional copies and/or other information, please let me know.

Trusting in your scientific vision and interest, I would appreciate your assessement of this paper. I am particularly interested in knowing
- whether the proposed experiments (via measurements of mean life) are actually feasable with current technology or not (see the original proposals, refs. 14, 15 and 16); and, if yes,
- whether they are actually valuable for the resolution of the problem of Einstein's special relativity; and, in any case;
- whether alternative experiments are also conceivable at this time.

Since I am not an experimentalist, I am unable to reach this assessement.

The problem of the experimental verification of Pauli's principle and other quantum mechanical laws is expected to be complementary to that of Einstein's relativity, and viceversa.

I did read with sincere pleasure and interest your recent article in PHYSICS TODAY. Permit me to express my hope that, some day, "the time projection chamber" can be used for truly fundamental experiments.

Very Truly Yours

Ruggero Maria Santilli

RMS/cgg
encls.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

May 7, 1979

Dear Drs. GEORGE CHARPAK, WILLIAM J. WILLIS, JACK SANDWEISS and DAVID R. NYGREN,

As you will recall, on November 15, 1978 I wrote an individual letter to each of you asking for advice and council on a rather crucial problem, the identification of the state of the art on the currently available proposals for the experimental verification of the expected invalidity (according to some) or possible validity (according to others) of the basic physical laws used in current trends in strong interactions. I was referring in particular to Einstein's special relativity and Pauli's exclusion principle.

I stressed in my letter to you that I was in need for such an assessement not only as an individual researcher, but also in my function as Editor in Chief of the HADRONIC JOURNAL. I also stressed that I am not an experimentalist. As such, I am not in a position to reach such an assessement, apart the selfevident expectation of a long way to reach maturity. The question was,however, how long? Is the proposal by Kim (Lett. Nuovo Cimento 12, 591 (1975))to test Einstein's special relativity via a measurement of the time-life of unstable hadrons truly lacking a germ of promise? is the proposal by Santilli (Hadronic J. 1, 574 (1978)) to test expected small deviations from Pauli's principle in nuclear physics (via low energy nuclear experiments for nuclei obeying certain criteria of selection) truly unrealizable via available technology and without a germ of promise?

I also stressed in my letter that the validity of the laws considered in the arena considered is a mere belief at this time, irrespective of the autority of its source. This creates a condition of question on the effectiveness of theoretical studies in the sector. At the extreme, it may even invite a process to our scientific accountability. After all, we are spending truly large amounts of money in strong interactions, all based on the assumption of the validity of the basic laws. How long can we continue this situation? How long can we wait before hadron physics is brought back to the traditional approach of physics in fundamental issues, that via experiments rather than beliefs?

I feel obliged to express my disappointment that none of you has even acknowledged reception of my letter.

In the meantime, the situation in theoretical hadron physics has predictably deteriorated. The enclosed paper is a manifestation of this situation.

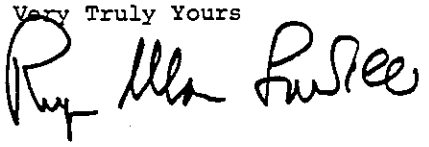
page 2.

It has been released for wide distribution (15,000 copies via the Hadronic Press)*to indicate to quark-committed colleagues that a critical inspection of quark conjectures is in motion (jointly with the study of fundamentally different conjectures for hadron structure). If they have technical arguments to disprove these criticisms, they must express them via scientific papers. The corridor-type of talks sometimes used by quark-committed physicists on quark-non-oriented studies is no longer effective or scientifically valuable. Nowadays, there are outstanding physicists in various Nations who not only question the quark models, but question the basic physical laws used in these models and are working at conceivable covering laws.

It is an easy prediction that the situation at the theoretical level will further deteriorate until the experimentalists assume their responsibilities, in this case, to initiate a predictably laborious, but essential study of the resolution of the basic controversies at the experimental level.

I sincerely hope you will reconsider your apparent negative attitude on these fundamental physical problems, despite potential, conceivable conflicts of the study considered with your current academic committments.

Very Truly Yours



Ruggero Maria Santilli

RMS/ml
encl.

* this paper will be soon distributed to your institutions.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 14, 1980

Dr. L. VAN HOVE
Director
CERN
CH-1211 GENEVA 23, SWITZERLAND

Dear Dr. Van Hove,

As a gesture of courtesy, I am enclosing copy of a draft of my paper "Remarks on the theorems of inconsistency of Heisenberg/Lie/symplectic formulations" quoting your contribution on the topic of 1951.

Any critical advice would be gratefully appreciated.

As director of CERN you should be informed that at the HADRONIC JOURNAL and, to my understanding, also at other Journals, a moratorium on the publication of papers on nonrelativistic quark models has been recently implemented.

My personal editorial experience is rather significative. I submitted for referee a paper on the topic in 1979 to a physicist expert in quarks, and a mathematician expert in quantization. The quark expert recommended the paper for publication. The pure mathematician, expert in quantization, rejected the paper as fundamentally inconsistent, because of the activation of the no-go theorem on (full)quantization, inconsistencies in the(pre-) quantization, intrinsic inconsistencies in the activation of the breakdown of the equivalence of Heisenberg's and Lagrange's equations, etc.

Regrettably, we had to dismiss the judgment by the quark expert, and rely on that by the independent mathematician.I should add that we have implemented a "moratorium", that is, a temporary suspension of judgment either in favor or against, until the issue is resolved. Also, QCD and other field theoretical settings are not included (at least at this moment, pending studies by mathematicians in the subject, to my knowledge).

It is a question for us of scientific ethics to avoid any preconceived restriction in the conduction of research, and actually solicite the view of colleagues of different orientation. We hope in this way to achieve a more mature judgment.

I do not know your personal view on the problem of the consistency or inconsistency of nonrelativistic quark conjectures. Nevertheless, you can rest assured that the expression of your view would be appreciated and welcomed irrespective of its orientation. Also, you can count on our best possible confidentiality.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls

P.S. I will be in Europe for a tour of invited lectures from February 24 to approximately March 12. I will be occasionally in phone touch with my parents in Rome, Italy (Dr. Ermanno Santilli, Via Virgilio Ramperti 19, 00159 ROME, Italy- Tel 06 43 81 507), and you can reach me there in case you so desire.

HADRONIC JOURNAL 3, 854-914 (1980)

Remarks on the problematic aspects of Heisenberg/Lie/symplectic formulations

Ruggero Maria Santilli*
Department of Mathematics
Harvard University
Cambridge, Massachusetts 02138

Received February 14, 1980

Abstract

A number of problematic aspects of conventional quantum mechanical formulations have been recently focused. A few rudimentary remarks are presented in the hope of contributing toward a more adequate identification of the open problems, as a prerequisite for their future resolution.

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THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

July 14, 1981

Professor F. JAMES,
Data Handling Division
C.E.R.N.
Geneva, Switzerland

Dear Professor James,

Thank you for the courtesy of sending me copy of your paper "Determining the statistical significance of experimental results", which I have found one of the most brilliant articles in the field.

Here at our Institute we are currently attempting the setting up of a committee of experimentalists, theoreticians, and mathematicians to initiate experimental studies on the exact or only approximate validity for the strong interactions of the conventional physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity (the Poincare symmetry), Pauli's exclusion principle and other basic laws. The committee is expected to conduct a feasibility study for possible new experiments on this fundamental open problem. Jpintly, the committee is expected to assess the possible re-elaboration of old data, as well as to evaluate existing experimental information. Additional information is available on request. Your participation would be, in my view, invaluable. Perhaps, it would be the most challenging and physically significant applications of your studies in statistical significance.

For your information, we have made some progress in the topic in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists working at the problem. In particular, we have identified the following experimental information:

- the apparent, quite large, deviation from conventional values of the magnetic moments of hadrons under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large, deviation from the prediction of conventional theories in the angle of precession of polarized neutron beams within matter, according to the experiment by Forte et al;
- the apparent, also substantial, violation of the T-symmetry under strong nuclear interactions, according to the experiment by Conzett et al;
- the apparent, also substantial, deviations from the predictions of the exact SU(2)-spin symmetry via 4 spinor symmetry experiments by Rauch et al (which DO NOT recover 720°); and other data.

Admittedly, the experimental information is still preliminary; all data can be suitably manipulated (theoretically) to force compatibility with orthodox doctrines (and interests...); and all experiments could be, in principle disproved by future, more accurate measures. However, the information is such to establish the fact that the validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information, when taken together, points toward an alteration of the intrinsic, space-time characteristics of particles under strong interactions which is quite plausible theoretically (see below), and which, if confirmed by future tests, would imply the irreconciliable invalidation of the entire Poincare symmetry, as well as the trust toward the pursue of fundamental advancement.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincaré symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

I presume you are familiar with the basic theoretical alternatives. If the familiar point-like abstractions of hadrons are truly effective for the strong interactions, there is no ground to expect deviation from conventional laws. In fact, points can only interact at a distance; the forces are then necessarily of potential type; and the familiar, local, Poincaré covariant, Lagrangian theories are consequential. BUT, all hadrons have a dimension of the order of the range of the strong interactions, and they are constituted by wave packets (rather than points). As a result, strong interactions demand the mutual penetration of wave packets for their activation. This, in turn, is a typical contact interaction in an extended region of space for which local/differential models are excessively approximative, and the notion of potential has no physical basis. Still in turn, nonlocal nonpotential interactions demand a nonunitary time evolution under which the electromagnetic characteristics of particles are not conserved, with consequential, irreconciliable invalidation of the entire (connected and discrete) Poincaré symmetry, and the need for broader physical laws.

A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

When seen from the outside, the time evolution is canonical, and the total energy is conserved. However, the motion of internal systems (such as a satellite during re-entry in atmosphere) occurs according to a noncanonical law, as a necessary condition to prevent perpetual-motion-type of approximations (in fact,nonconservative forces are non-Hamiltonian by conception). In the final analysis, our Earth has resulted to be a truly complex system beyond simplistic, Lagrangian/Hamiltonian models, and can be conceived as a Newtonian image of the structure of hadrons and nuclei in exactly the same measure as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to emphasize the complexity of the problem I am inviting you to participate. In fact, the acquisition of true scientific knowledge in the problem calls for direct measures under strong interactions, which is not an easy task. The problem also calls for an assessement of the impact of unverified theoretical assumptions in the data elaboration. A most important question is exactly in your field, and consists of the identification of the "scientific credibility" of existing experimental information in high energy physics in regard to the validity of basic laws under strong interactions.

However, permit me to confess candidly that we do not see the complexity of the problem as a reason to justify inaction, nor we accept supinely predictable attempts to prevent the acquisition of fundamental new knowledge. After all, the open character of the basic laws under strong interactions is too well known (after several conferences and countless articles) to justify the continued ignorance of the problem without risking questions of scientific ethics; the human and financial resources we spent in the development of the theory of strong interactions are too huge to justify ignorance of the basic aspects without risking dangerous administrative unbalances; and the implications (e.g., for controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and unpredictable consequences.

Please do not feel obliged to reach a final decision in any direction following this letter. Perhaps, you can follow our efforts, and decide the initiation of active involvement at some later time. On our part, we would simply need the indication of a sincere interest, for us to keep you informed. Our group will gather at the forthcoming
-FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS to be hald here in Cambridge from August 3 to 7 under partial support by the U.S. Government via DOE;and
-FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, to be held in France from Juanuary 5 to 7, 1982 under partial support by the French Government via local Institutions.
In case you can attend these meetings either as an observers or as an active participant, you would be sincerely welcome.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml
encls.

PR - Öffentlichkeitsarbeit

DEUTSCHES ELEKTRONEN—SYNCHROTRON DESY
NOTKESTR. 85 · 2000 HAMBURG 52 · TEL. 040—89 980 · TELEX 2 15 124 desy d · TELEGR.—ADR. DESY HAMBURG

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
attn. M. Mary Lou Wright
Cambridge, Massachusetts 02138
U.S.A.

August 17, 1982

Dear Madam:

The name of our Director is

Volker Soergel.

In German we call him Prof. Dr.

DESY is lead by a Directorate of five members of which Prof. Soergel is the head.

The other members are:

Richard Laude (Administration)
Prof. Paul Söding (Research)
Dr. Wolfram Schött (Services)
Prof. Gustav-Adolf Voss (Accelerators).

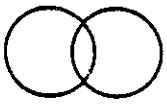
Yours sincerely,

P. Waloschek

(P. Waloschek)
DESY-PR

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859



September 7, 1982

Ms. BETTINA KLOPRIES, Librarian
DESY
Notkestr. 85
2000 HAMBURG 52 W. Germany

Dear Ms. Klopries,

I am writing you in regard to my letter of August 27, 1982, requesting information containing names of the Director General or DESY and its primary officers.

Please be informed that I received this information per a letter dated August 17, 1982, from DESY—PR, P. Waloschek, several days ago.

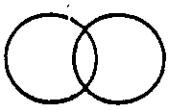
Thank you again for your cooperation.

Sincerely,

Mary Lou Wright

(Mrs.) Mary Lou Wright
Secretary

mlw



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

December 22, 1982

Professor H. SCHOPPER
Director General
CERN
1211 GENEVA 23, Switzerland

Dear Professor Schopper,

Our Board of Governors is preparing a report on the current status of High Energy Physics for submission to President Ronald Reagan, and to appropriate U. S. Governmental Agencies.

The outcome of the experimental search for the $W^{\pm}$ and Z^{o} bosons currently going on at your Laboratories, whether positive or negative, is important for the finalization of our presentation.

We would therefore appreciate the courtesy of forwarding to us an indication of the current status of the search for the heavy bosons, even a preliminary and tentative one, for our own information, as well as for inclusion in our report.

We believe that our report may be of value also for your Laboratories, inasmuch as it touches on certain fundamental aspects of contemporary trends in strong interactions. It would be therefore a pleasure for us to send you a copy of the report.

I would like to take this opportunity to wish you and all at CERN our best for a happy and prosperous 1983.

Best Personal Regards,

Ruggero Maria Santilli
President and
Professor of Theoretical Physics

RMS/mlw

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

January 20, 1983

Ruggero Maria Santilli, Professor of Theoretical Physics and President

Professor H. SCHOPPER, Director
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH
1211 GENEVA 23, SWITZERLAND

Dear Professor Schopper,

I am taking the liberty of recommending, most respectfully, to you and to your associates:

The consideration of the experimental resolution at CERN of the exact or only approximate validity of Einstein's special relativity for the interior of systems with strong interactions.

I enclose a general description of the studies conducted until now which, even though non—technical, contains sufficiently diversified information indicating that quantitative studies of the problem are within experimental, theoretical, and mathematical reach.

A collegial way to proceed would be the setting up of a Committee of Study for the purpose of:

(a) Evaluating the pitfalls of the arguments conceived in the hope to nullify the need of the tests (see pages 78-81 of the enclosed report for a review);

(b) Identifying and assessing all existing proposals (such as Kim's proposal to measure the mean life of unstable hadrons in flight at different speeds);

(c) Pointing out theoretical topics deserving further study as a necessary pre-requisite for effective tests (such as Mignani's nonpotential generalization of the potential scattering theory currently used at your Laboratories for the data elaboration of experiments in strong interactions);

(d) Identifying the equipments at your Laboratories which appear most promising for the tests (by keeping in mind that we are referring here to the new challenge of actual measures under strong external interactions); and

(e) Identifying new equipments that appear needed for low-energy, highsentitivity and moderate costs, (such as the neutron interferometers used in the main available test described in Section 3.2).

In case you are interested in additional information, you can count in my best possible assistance, including my availability to visit your Laboratories at some mutually convenient time. The same holds for all other members of our team.

But, most importantly, please keep in mind the ultimate motivation underlying our research efforts and this recommendation: the need for scientific accountability vis-a-vis our societies. In fact, we are all spending large public sums in strong interactions. Most of these public sums are spent on the basis of a mere belief of the validity for the strong interactions of physical laws clearly established only for the electromagnetic interactions. Scientific accountability then suggests that we de—emphasize all perso— nal theoretical views, whether in favor of old basic laws or in favor of suitable more general laws, and establish the physical foundations of the current theories of strong interactions in the only scientifically possible way: via direct experiments.

Very Truly Yours

Ruggero Maria Santilli
President
cc: Drs.E.GABATHULER,R.KLAPISH,E.PICASSO, J. PRENTKI, A.M.WETHERELL, P.ZANELLA, et al.
CERN
RMS-mlw
encl.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

January 20, 1983

Professor VOLKER SOERGEL
Director
Deutsches Elektronen–Synchrotron
Notkestrasse 85, 2000 Hamburg 52, West Germany

Dear Professor Soergel,

I am taking the liberty of recommending most respectfully to you and to your associates:

The consideration of the test at DESY of the exact or only approximate validity of Einstein's special relativity for the interior of system with strong interactions.

I enclose a general description of the studies conducted until now which, even though non–technical, should contain a diversification of elements and ideas confirming that quantitative studies of the problem are within reach.

A collegial way to proceed would be the setting up of a Committee of Study for the purpose of:

(a) evaluating the pitfalls of the arguments conceived in the hope to nullify the need of the tests (see the last section of the enclosed report–pp. 78-81–for an informal review);

(b) identifying all existing proposals(such as Kim's proposal on the measure of the mean life of unstable hadrons in flight);

(c) pointing out theoretical topics deserving further study as an essential pre-requisite for tests (such as Mignani's studies on the nonpotential generalization of the conventional potential scattering theory currently used at your Laboratories for the data elaboration);

(d) identifying the equipments at your laboratories which appear most promising for the tests (by keeping in mind that we are referring to actual measures under external strong interactions);

(e) identifying new equipments that appear recommendable at some future time (e.g., of the type of the neutron interferometry used in the main test described in Section 3.2 of the enclosed presentation).

In case you are interested in additional information, you can count on my best possible assistance, including my availability to visit DESY at some mutually agreable time. The same holds for all other members of our group.

But, most importantly, please keep in mind the ultimate motivation underlying our research efforts and this recommendation: the need for scientific accountablity vis-a-vis our societies. In fact, we are all spending large public sums in strong interactions. Most of these public funds are spent on the basis of the mere belief of the validity for the strong interactions of basic physical laws established only for the electromagnetic interactions. Scientific accountability clearly suggests that we de-emphasize all personal theoretical views, whether in favor of established laws or in favor of more general laws, and establish the physical foundations of the current theories of strong interactions in the only scientifically possible way: via direct experiments.

Very Truly Yours

Ruggero Maria Santilli
President
cc:ProfessorsP.SODING, andG-A. VOSS, DESY
RMS-mlw
encl.

EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH
European Laboratory for Particle Physics

DIRECTOR-GENERAL

CERN
CH-1211 GENEVA 23
SWITZERLAND

DG/1024-83

Professor Ruggero Maria SANTILLI
President
The Institute for Basic Research
96 Prescott Street

CAMBRIDGE, Massachusetts 02138
USA

Geneva, 1st February 1983

Dear Professor Santilli,

Thank you very much for your letter in which you inform me that your Board of Governors is preparing a report on the current status of high energy physics.

With great pleasure I am prepared to give you the information on our boson search, in particular as you certainly have heard the W has been discovered here recently. Enclosed you will find a copy of the paper of the UA1 experiment which describes this discovery. The second experiment, UA2, has similar results and a paper will be available very soon. I shall send you a copy as soon as I receive it.

Since the production of the Z^0 is about a factor of 10 lower than the production of the W the chances to have seen Z particles so far were very small. However, we are starting a new proton-antiproton run in our SPS in April, which will last until July. We hope very much that during that run sufficient luminosity can be accumulated in order to see also the Z^0.

If you need any more detailed information please let me know.

I certainly would be very much interested to receive a copy of your report.

With best personal regards,

Sincerely Yours,

H. Schopper

Herwig Schopper

Encl.

EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH
European Laboratory for Particle Physics

DIRECTOR-GENERAL

CERN
CH-1211 GENEVA 23
SWITZERLAND

DG/1092-83

Professor Ruggero Maria SANTILLI
President
The Institute for Basic Research
96 Prescott Street

CAMBRIDGE, Massachusetts 02138
USA

Geneva, 22 February 1983

Dear Professor Santilli,

Thank you for your letter of 20 January 1983 and for the copy of your report outlining the work carried out at the Institute for Basic Research.

It is clear that tests of both restricted and general relativity are of fundamental importance. It is equally true that experiments to test these theories need to be of considerable sophistication and carried out with a very high degree of accuracy. It is not possible to judge whether or not the CERN Laboratory is a suitable place to carry out such experiments until detailed proposals have been put forward as for other experiments in high energy physics. Presumably such proposals would only be elaborated after your suggested Committee of Study has reached some conclusions on the five topics (a) to (e) listed in your letter.

Yours sincerely,

Herwig Schopper

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

March 2, 1983

Professor HERWIG SCHOPPER
Director General
CERN
CH–1211 GENEVA 23, Switzerland

Dear Professor Schopper,

On behalf of all I.B.R. members, I would like to express our sincere appreciation for your kind letter of February 1 (arrived during my absence) as well as our congratulations for the outstanding discovery of the W's at CERN. We shall treasure the paper you kindly mailed to us among the memorabilia of our Institute.

You will be pleased to know that, as you can see in the enclosed personal correspondence with The New York Time, we are considering to join others in the recommendation of Dr. C. Rubbia to the Nobel Committee.

We are working on our report on experimental high energy in the U.S.A., and it will be a pleasure to mail you a copy whenever completed. In the meantime, you might be interested to know the main ideas of the report in case of any value to CERN.

Scientific scene created by the discovery of the W's. In our view, the discovery of the W's signals the beginning of the end of an era in particle physics. In fact, we have now a new scientific scene in the sense that, besides the predictable discovery of the Z^0 (and the confirmation of the W's), *there are no more truly fundamental new particles to discover.* The issue created by this novel situation is therefore the following: which is a truly fundamental experimental program for future pursuit? The answer we submit is: to achieve the experimental resolution of the exact or only approximate validity of Einstein's special relativity under strong interactions, including the discrete and continuous components of the Lorentz symmetry. I mailed to you on January 20, a report on this proposal (ref. [1]). Besides being nontechnical and preliminary, this report is highly insufficient on numerous aspects. Permit me to add here a few comments for whatever their value.

The need for the test of the Lorentz symmetry under strong interactions. Stated as simply as possible, the need is created by the fact that all available direct tests, even though highly tentative and inconclusive, point rather clearly toward deviations from the Lorentz symmetry. The need for the experimental resolution of this truly fundamental problem one way or another is then consequential. The arguments often heard in academic corridors in the hope to nullify the need of the tests via the citation of indirect experiments (where the Lorentz symmetry is assumed) should be treated with care owing to their possible manipulative intent (see Section 5.3 of ref. [1]).

Regrettably, the topic is plagued with prejudices, misconceptions, and even religious beliefs. For instance, it is often heard that isolated systems of particles "must" obey the Lorentz symmetry. The violation of the Galilei symmetry for classical, nonrelativistic, isolated systems is unequivocably established in nature by

closed non—Hamiltonian systems (think of our Earth when seen from the outside as isolated: the total conservation laws hold, but the internal forces are strictly non—Hamiltonian, by therefore preventing the applicability of the analytic—algebraic—geometric foundations of Galilei's relativity).

It is evident that the classical physical reality does not constitute grounds for the necessary existence of a counterpart at the particle level. Nevertheless, available indications are sufficiently serious to warrant the experimental resolution considered.

In essence, when particles can be effectively approximated as being point—like, the Lorentz symmetry CANNOT be broken, no matter what the interactions are. This includes the virtual totality of the electromagnetic interactions, as well as several aspects of weak interactions (e.g., semileptonic decays).

However, when particles cannot be effectively approximated as being point—like, we have the opposite situation, that is, we have difficulties in preserving the Lorentz symmetry as exact. In fact, once we acknowledge that perfectly rigid objects do not exist in nature, we see the possibility of deformations of the extended charge distributions of hadrons under strong interactions, in which case the rotational symmetry CANNOT be preserved as exact. Even ignoring all other arguments, the breaking of the remaining components of the Lorentz transformations follows.

To put it differently, the exact validity of the Lorentz symmetry for a proton in a particle accelerator constitutes no final indication on the problem of the validity or invalidity of the same symmetry in the interior of the proton.

In fact, the trajectory of the center—of—mass of Earth in the solar system strictly obeys Galilei's relativity as well known, while, as equally well known, the same relativity is broken in interior open problems.

A prejudice lingering in current academic circles is therefore the dream that available experimental information on high energy particle scatterings constitutes sufficient ground to claim the validity of the special relativity under strong interactions. Equally prejudicial in our view is the hope of reaching deviations from the Lorentz symmetry via such experiments. Indeed, all these experiments are conceived for exterior, closed, conservative, center—of—mass scatterings. To look at deviations under these conditions would be the same as looking at deviations from the Galilean character of Earth's center—of—mass trajectory in the Newtonian treatment of the solar system!

For these reasons we consider fundamental that, to be meaningful, the tests of the Lorentz symmetry must be conducted under actual OPEN NONCONSERVATIVE CONDITIONS DUE TO EXTERNAL STRONG INTERACTIONS. Once this crucial aspect has been resolved, then the formulation of the complementary problem for the exterior closed treatment can be consistently achieved.

To put it differently, validity of the Lorentz symmetry under electromagnetic interactions is established not only for exterior closed systems, but also for the open interior ones. In fact, Dirac conceived his equation for an electron under the exterior electromagnetic field of a proton. What we are advocating is essentially the test of the equivalent of Dirac's conception for external strong interactions.

Numerous additional prejudices exist in the current view of the problem. Regrettably, their treatment here would render the length of this letter prohibitive.

Status of currently available direct tests. To my best knowledge, the most salient, direct tests currently available, are the following [with the understanding that this letter is being written because of their lack of conclusive character].

(A) *Test of the rotational symmetry of nucleons under low energy nuclear interactions.* It has been conducted for a number of years by Rauch et al [2] via neutron interfermetry. The most recent results indicated about 1% deformation of the spherical symmetry of nucleons within the fields of Mu–metal nuclei, exactly as predicted by Eder [3] jointly with other predictions (also apparently verified, such as the joint anomalous behaviour of the magnetic moment, and the slow–down of the angle of spin precession). There is nothing mysterious here. We merely have the deformation of the sphere $xx + yy + zz = 1$ into the ellipsoids $xax + yby + zcz = 1$ $(a,b,c > 0)$ caused by intense fields, with the consequential manifest loss of the rotational symmetry.

(B) *Test of the Lorentz boosts.* The best ones available are those reviewed in ref. [4] regarding the mean life of unstable hadrons in flight (mesons and kaons). I hope you can see in this independent work by H. B. Nielson of NORDITA the plausibility of deviaitons from the Lorentz symmetry. Again, we have nothing mysterious here. In fact, you certainly remember the old idea of nonlocal (integral) dynamics in the interior of hadrons (e.g., E. Fermi), in which case you cannot apply the analytic–algebraic–geometric foundations of Lorentz transformations, let alone the transformations themselves. Once you have internal departures, they manifest themselves via departures from the mean life [4].

My personal view on the problem is the following. I believe in the invariant $xx + yy + zz - tct$ within the physical conditions originally proposed by Lorentz, Poincaré, and Einstein, that is, for motion in vacuum. It is well known that, for motion within physical media, the speed of light ceases to be constant, to acquire a dependence on local physical quantities (time, coordinates, density, wavelength, temperature, etc.). Also, physical media are manifestly inhomogeneous and unisotropic. The preservation of the old invariant $xx + yy + zz - tct$ in classical material media is therefore deprived of scientific value. The minimum we can do is to represent the speed of light as it is, i.e., as a function $c = d(t,r, \ldots)$, and admit the inhomogenuity and unisotropy of the media, by therefore resulting in the generalized local invariant $xax + yby + zcz - tdt$. The local loss of the Lorentz symmetry as conventionally known is then unavoidable, in my view.

The plausibility of a generalized invariant in particle physics is self–evident. In fact, the moment you accept the extended character of hadrons under strong interactions, you have motion of particles within a medium of other particles. Alternatively, the belief that the invariant $xx + yy + zz - tct$ is exact in the interior of a proton may well result to be of mere religious–non-scientific character. At any rate, the issue is too fundamental to be left at the level of personal views, and must be resolved via experiments in due time.

The distinction we are alluding here is the following. The homogenuity and isotropy of the empty space is so evident to prevent sufficient motivation for their additional experimental verification at this time.

However, when extended particles (such as hadrons) move within a sea of other particles (called the "hadronic medium"), the idea that they keep moving in vacuum does not seem to have scientific value. The most logical approach is therefore that of admitting the existence of new media composed of space filled up with wave packets of particles and radiation. The inhomogenuity and unisotropy of such medium is then as evident as the deformation of a perfectly spherical object. Thos loss of the Lorentz invariant and symmetry under these conditions is then as evident as the loss of the rotational symmetry under the deformation of a sphere into an ellipsoid.

Experiments on the Lorentz symmetry under exterior strong interactions should therefore test the nature of the actual medium in which motion occur, and NOT the homogenuity and isotropy of empty space, which is out of the question for us.

(C) *Test of the time–reversal symmetry in open nuclear reactions.* In its best available form, it has been measured numerous times by H. E. Conzett (Berkeley Labs.), R. J. Slobodrian (Quebec) and others [5]. The deviations from the exact symmetry are quite impressive. Again, there is absolutely nothing mysterious here. In fact, all dissipative (open) nuclear reactions are time–asymmetric. You can trivially see it as follows. Consider the Hamiltonians H of these reactions which, as you know, are non–Hermitean. Decompose them into the product of a Heritean term E = energy, and a "dissipative" term C. Then, $H = EC$ and $H^+ = C^+E$, and the time evolution assumes the manifestly time–asymmetric (Lie–admissible) form $iA = A \lhd E - E \rhd A$, $\lhd = C$ $\rhd = C^+$.

Note that, by construction, the time–asymmetry ceases to exist when you implement the system into a closed form, i.e., a form for which the total Hamiltonian is Hermitean and conserved.

Note that $i\dot{E} = E \lhd E - E \rhd E = ECE - EC^+E \neq 0$ as a necessary condition of consistency (the reaction being open by assumption). Thus, if you impose conservation, you recover automatically the antisymmetry of the product, i.e., $i\dot{H} = HCH - HCH$, $C = C^+$. Mathematically, you pass from the Lie–admissible algebras, to the simpler Lie–isotopic algebra with product $ACB - BCA$. The trivial, simplest possible Lie product of current use, $AB - BA$, is ignored here because excessively dependent on the point–like approximation of particles.

Regrettably, the measures by Conzett, Slobodrian, et al [5] appear to be disproved by re–runs at Los Alamos; the situation is now in somewhat scientific disarray; and the need for a resolution by a third, independent party is essential.

The lack of scientific disaster in case of confirmation of departures from the Lorentz symmetry. A number of colleagues have the impression that the experimental confirmation of departures from the exact character of the Lorentz symmetry would constitute a sort of scientific vacuum. Nothing is more removed from the truth. In fact, the mere possibility of departures is stimulating an enthusiastical thrust toward the generalization of old ideas. For instance:

(A') Theories leaving invariant the "deformed charge distribution" $xax + yby + zaz = 1$ have been constructed via a step–by–step Lie–isotopic generalization of the conventional theory of rotations;

(B') Theories capable of leaving invariant the "deformed charge distribution in space–time" $xax + yby + zaz - tdt$ are well under way. Their construction is made possible by the Lie–isotopic lifting of the Lorentz group in which the original group is deformed into a form admitting the inverse of the new metric as the identity, that is, as the Casimir element of order zero. Its invariance is then trivial for all functional dependences of the speed of light.

(C') The possibility of a time–asymmetry is promoting a virtual explosion of novel studies in fields even outside particle physics, such as statistical mechanics or biophysics. The mathematical theory is, this time, the Lie–admissible generalization of the Lie–isotopic theory as indicated early.

The apparent beautiful compatibility with quark theories and the W's. Another rather frequent misconception is that a departure from the Lorentz symmetry is in conflict with quarks. Again, nothing

can be more removed from the truth. In fact, the lack of exact character of the Lorentz symmetry would merely imply that quarks cannot be considered, strictly speaking, as elementary. In such a case, quarks would merely be COMPOSITES OF MORE ELEMENTARY ENTITIES. As a matter of fact, the approach appears to offer genuine possibilities of achieving a strict form of quark confinement (identically null probability of tunnel effects for free quarks), trivially, because of the profound technical differences between the mechanics for the outside (conventional QM) and the generalized one for the interior dynamics.

Numerous other possibilities for advances in quarks, which are now prevented by the current assumption of a rigidly exact Lorentz symmetry, would be permitted by deviations. In fact, we are organizing a summer workshop on these problems where there is a specific session devoted to "applications to quarks, QCD and gauge theories" (see enclosures).

The regrettable politics at U. S. National Laboratories and the opportunity at CERN. Very unfortunately, U. S. National Laboratories are currently controlled by vested academic interests opposing most vigorously the tests of Einstein's special relativity under strong interactions. This is well known in the States and, by no means, it is a confidential disclosure. This momentary weakness of the U. S. can be the advantage of a laboratory such as CERN. In fact, it seems to me that the minds of CERN physicists are more independent, when compared to the monolitically controlled minds of their colleagues in U. S. National Laboratories, thus exhibiting the elements for independence of scientific thought.

In the final analysis, I am contacting you precisely because I have faith in CERN, particularly after your taking over the Directorship.

I.B.R. possible assistance. In case you are interested in considering the possibilities in more details, and without any unnecessary formal commitment, our Institute can provide all possible support.

Note that I have studiously abstained from recommending any specific experiment, and I shall continue to do so, even though I have several in mind. In fact, the selection of experiments should be a collegial effort taking into consideration numerous factors, as well known.

Our Institute can assist you toward such a collegial study in a number of ways, e.g.,

(1) *By preparing a collection of papers in the field which are essential for the acquisition of mathematical, theoretical, and experimental knowledge needed for judgment.* I am referring to:

- a few mathematical papers on the Lie–isotopic and the Lie–admissible generalization of Lie's theory;
- a few theoretical papers on the current efforts to achieve the generalization of the Lorentz transformations along lines A', B', and C'; and
- copies of all important experimental papers along lines A, B, C.

(2) *By coordinating a presentation at CERN of members of our team.* The I.B.R. is coordinating all scientists interested in the problem on a world–wide basis. It would be a pleasure to identify a team composed, say, of

- one or two mathematicians in Lie–isotopy or Lie–admissible genotopy; such as M. L. Tomber (Michigan); H. C. Myung (Iowa); et al.
- two or three theoreticians working at the generalizations such as: G. Eder (Atominstitut, Wien), working at the generalization of spin; R. Mignani (Univ. of

Rome, Italy), working on the generalization of the potential scattering theory for data elaboration; and myself, currently working on the generalization of rotations and Lorentz transformations;

- three or four experimentalists who have worked at the problem, such as: H. Rauch (Wien); H. Conzett (Berkeley); R. J. Slobodrian (Quebec); G. Matone (Frascati), et al.

(3) *By arranging possible stays of I.B.R. members at CERN to assist the experimentalists.*

Kindly review these various options and feel free to communicate your comments. You can count on my best cooperation. More particularly, please feel free to indicate whether a possible interest should be kept conficential at this moment. I am full aware of the multiple difficulties of your post, and you can count on my honoring your requests in their entirety.

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

References:

[1] *Theoretical, experimental, and mathematical studies conducted at the I.B.R. toward a generalization of Galilei's and Einstein's relativities in classical and quantum medhanics,* I.B.R. nontechnical report dated January, 1983, not intended for publication;

[2] H. Rauch, Hadronic J. 5, 729 (1983)

[3] G. Eder, Hadronic J. 4, 2018 (1983)

[4] H. B. Nielsen, Nuclear Physics B211, 269 (1983)

[5] R. J. Slobodrian et al, Phys. Rev. Letters 47, 1803 (1981)

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

June 6, 1983

Professor H. SCHOPPER, Director
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARH
1211 GENEVA 23, Switzerland

Dear Professor Schopper,

We would gratefully appreciate the courtesy of indicating to us the procedure for the submission of experiments to CERN.

The organization of our forthcoming First Workshop on Hadronic Mechanics is proceeding on schedule. A number of participants intend to submit a group proposal to CERN at the conclusion of the meeting.

To provide you with a tentative and preliminary idea, one of the proposals is expected to deal with new measurements of the mean life of unstable hadrons at different energies (pions and kaons, in particular). In fact, available experimental data appear to show a deviation from the exact Lorentz symmetry, as conceivable since several decades because of possible, internal, nonlocal effects.

Thanking you in advance for your courtesy, I remain,

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

June 6, 1983

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Thanking you in advance for your courtesy, I remain,

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

PART XIII:

PHYSICAL
REVIEW
LETTERS

AND

PHYSICAL
REVIEW
D & C

PART XIII–A:

CORRESPONDENCE

WITH

R. K. ADAIR,

EDITOR OF

PHYS. REV. LETTERS,

IN 1979–1980

Division of Particles and Fields

American Physical Society

TO: Membership of the Division

FROM: L. Pondrom, Secretary-Treasurer

SUBJECT: Election Results and Other News

1.) New Members of the DPF Executive Committee

Vice Chairperson: J. Sandweiss, Yale University

Executive Committee: H. Frisch, University of Chicago
R. Jaffe, Machusetts Institute of Technology
R. Lanou, Brown University

The other officers of the Executive committee for 1979 are: M. Perl, Chairperson, L. Pondrom, Secretary-Treasurer. The other Executive Committee members are: D. Caldwell, S. Gasiorwicz, P. Rosen, and H. Quinn. The next meeting of the committee will probably be during the APS meeting in Washington, D. C., 23 -26 April 1979.

2.) Announcement of Conferences

The International Conference on Electromagnetic and Lepton Interactions will be held at Fermi National Accelerator Laboratory, Batavia, Illinois from 23 August to 29 August 1979. These dates are earlier than those listed in the LBL Pocket Diary. Please note the change.

Los Alamos Scientific Laboratory will host a LAMPF Program Options Workshop which will address critical questions in nuclear and particle physics and how they can best be investigated through the use of intermediate energy accelerators. The meeting will be held in Los Alamos, August 20-31, 1979. Panel membership is by invitation; plenary sessions are open to all interested persons. Further information may be obtained from John C. Allred, Mail Stop 830, Los Alamos NM 87545 USA.

3.) PPF Subscription Drive

A subscription form for SLAC - PPF is included in this mailing for the convenience of those members for the Division who wish to subscribe to this weekly listing of preprints.

4.) Letter from the Editors of Physical Review Letters

A letter to the membership from the editors of PRL is also included in this mailing.

THE PHYSICAL REVIEW

AND

PHYSICAL REVIEW LETTERS

BROOKHAVEN NATIONAL LABORATORY, UPTON, NEW YORK 11973
Telephone (516) 924-5533 (FTS) 664-2540
Telex: c/o BNL, 96-7703 Cable Address: BROOKLAB

January 26, 1979

To the membership of the Division of Particles and Fields:

The Editors of Physical Review Letters are most anxious to work towards a situation such that we publish the best short papers in theoretical particle physics. At the present time, only about 6% of the Letters are concerned with theoretical particle physics while, for example, about 20% of the pages in the Physical Review (A,B,C and D) are devoted to theoretical particle physics. While such numerology is surely not an absolute guide to an ideal distribution of subject matter in the journal, we do believe that this indicates that we have a serious deficit in the theory of particles and fields--and we can hardly conclude that this deficit follows from a lack of progress in the subject! Aside from the fact that we are publishing very few theoretical particle physics papers, we have a strong feeling that we are missing many of the better papers and that the papers we do publish are not really representative of the best work on particles. We hope that we can find some way to change this: we would like to publish more theoretical particle physics papers, perhaps 10 or 15 a month on the average (which is at least twice what we are publishing now) and we would like to feel that the papers we publish are representative of the most interesting work in the field. We hope that we can achieve a position such that the Phys. Rev. Letters would be the first journal to be considered when an American particle physicist plans to publish a short report on work which he considers outstanding. We recognize that this will only be the case when he is confident that his paper will be considered in a responsible manner. It is clear that this confidence is now wanting.

Our general system of identifying appropriate papers through the counsel of referees who work in the area of inquiry considered by the paper, works well in most fields. It does not seem to work nearly as well, probably not well enough, in theoretical particle physics. There are probably a number of reasons for this state of affairs but we do not think that we really need to understand the difficulties with any precision in order to conclude that there is a problem and to consider remedies for the problem.

Aside from specifics, we believe that we can revive Phys. Rev. Letters as a primary journal for theoretical physics only through

some action taken through cooperation of the community and the Editors of the journal. Inevitably, this will require some commitment and increased effort on the part of that community. Equally, the design and implementation of the new procedures which seem to be required will test the ingenuity and flexibility of the Editors and we are prepared to do our best to effect necessary changes.

These changes are constrained by certain practical considerations. At the present time the Editors consider over 2000 papers a year and approve the publication of 1000 papers. We are considering a situation where we hope to handle, perhaps, 300 theoretical particle physics papers a year and to publish about 150. An administratively efficient organization has been developed over the years which, we believe, does a very good job of handling this flow of material to Phys. Rev. Letters and one should consider procedures which make use of this organization. We suggest, then, the following procedures for the handling of theoretical particle physics papers.

The Division of Particles and Fields would recommend to the Editors the appointment of 4 Associate Editors for theoretical particle physics. Papers in theoretical particle physics would be submitted to the journal as they are now. The Editors would select two referees and send a copy of the abstract and title page of the paper and the names of the referees to an Associate Editor. If both of the referees approved of the paper, the paper would be approved for publication with a copy of that approval sent to the Associate Editor. If both referees advised rejection of the paper, the paper would not be accepted but sent back to the author with the referees' comments. A copy of the paper together with the referees' comments would be sent to the Associate Editor. If the two referees disagreed, the comments of the referee who rejected the paper would be sent to the author while the paper and referees' comments would be immediately sent to an Associate Editor for his advice. The authors reply to the referees would be forwarded to the Associate Editor as it is received.

We hope to get 300 papers a year and, with the scenario presented here, we would expect that about 50% of the papers would come before an Associate Editor. This would give each Associate Editor about 40 papers a year which is, we believe, an appreciable but not too onerous a work load. The Associate Editors would be chosen to cover somewhat different areas but there would be no effort to define areas too precisely.

We hope that the changes in procedure listed here will improve the probability that a paper is considered responsibly and then make the journal more attractive to authors. We believe that the journal has a great deal to offer to prospective authors: Phys. Rev. Letters is probably the most widely read journal in physics. We have 6,000 individual (non-library) subscribers and competitive journals have less than one-fifth as many. Our refereeing system will continue to be somewhat more abrasive than the more authoritative system of receiving editors (though we hope that our referees will be a little more tactful in their criticisms of the work of their friends and colleagues) but we hope that our authors will tolerate this abrasion

as a part of our democratic procedures. We have a democratic way of handling papers in the American Physical Society Journals and, on some levels, as with so many democratic procedures, we act less efficiently than autocracies. With our journals, the referees which represent the community in a rather representative manner, take over some of the duties which the editor exercises in a more authoritative journal. If the community is responsible, we believe that democratic procedures are, on balance, better. We hope that we can find a way to use the community in a manner such the inherent responsibility of the community can be exercised in a contribution to a better journal.

Sincerely,

R. K. Adair, G. L. Trigg, G. L .Wells
Editors, Physical Review Letters

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

April 16, 1979

Dr. R. K. ADAIR, G.L. TRIGG and G.L.WELLS
Editors, PHYSICAL REVIEW LETTERS
BROOKHAVEN NATIONAL LABORATORY
UPTON, LONG ISLAND, N.Y. 11973

Dear Drs. Adair, Trigg and Wells,

I have read with interest your communication to the members of the Division of Particles and Fields of the AMP of January 26, 1979. I would like to express my support for your action. In particular, I have admired your clear statements of facts related to theoretical papers in your Journal, as well as your clear expression of determination to improve the situation.

I would like to take the liberty here to express my personal view, mostly originating from my indipendent research interests in theoretical physics, as well as my experience as editor in chief of the HADRONIC JOURNAL.

I believe that the conditions indicated in your communication are a reflection of the current, delicate moment of our community of basic research. Permit me to candidly confess that, in my view, the current conduction of research is mainly an expression of personal opinions, or beliefs by individual or group of researchers, and not the manifestation of an experimentally established physical veritas. I am here referring only to the conduction of research in the theory of strong interactions.
I would like to stress that such an occurrence is the necessary condition for advancements in human knowledge. That is, without opinions, beliefs and conjectures, subsequently proved or disproved, there would be no advancement.

Yet, the situation in our community is different, in my view. Permit me to candidly confess that, by and large, the opinions by autoritative groups of researchers are generally considered the physical veritas, and any non-aligned study is generally considered wrong, or without physical value.

This situation is created by the nowaday vexing state of affais of the quark models, quantum chromodynamics and related schools. A series of (rather courageous) articles in the 1978 volume of the Hadronic Journal has stressed the simply unequivocal validity of these studies for the Mendeleev-type, exterior, "chemical", classification of hadrons. Yet, the same articles have expressed doubts on the joint validity of the same models, also for the structure, and have suggested the search of

fundamentally different models of structure capable of reaching full compatibility with the established models of classification, while capable of resolving some of the problematic aspects inherent in the quark conjectures. This is much along the conceptual structure which produced the solution of the problem of the atomic phenomenology: one model of classification (Mendeleev) and a fundamentally different, yet compatible model of structure (Bohr). Almost needless to say, this line of study was advocated as a complement, and not as a substitute for the current studies on quark conjectures. Specifically, the attitude was that studies on quark conjectures for hadron structure should continue, while, jointly, fundamentally different models should be investigated.

On the surface, this appears as a reasonable attitude. In practice, however, it is faced with rather considerable difficulties, most of which, in my view, are of purely emotional character. The issue which is at stake is not whether quarks exists or not. More fundamentally, the issue is whether the basic physical laws used in quark models (Einstein's special relativity, Pauli's principle, the spin-statistics theorem, etc.), which are experimentally established until now only for the electromagnetic interactions, are valid or invalid for the strong interactions in general, and the strong hadronic forces, in particular. See the enclosed leaflet on reprint volumes edited by H. C. MYUNG, S. OKUBO and myself.
It is understood that, if these laws need a generalization for the strong hadronic forces (as suggested by rather numerous arguments, and as nowaday believed by a number of qualified physicists), the quark conjecture is ruled out in the final form. Indeed, there would be the lack of the basic ingredients (e.g., the notion of spinor) to even vaguely define a quark.

Still in my view, this situation has created a clear division of the physics community into "quark-believers" and "quark-non-believers" with divergencies, not of minute technical character, but rather of fundamental nature. In turn, this situation, still in my view, directly appears at the editorial level of specialized journals in the field.

Perhaps, a most representative case is my recent paper jointly with C.N.KTORIDES and H.C.MYUNG, submitted to Phys. Rev. D and entitled "Lie-admissible approach to broken SU(2)-spin under strong non-self-adjoint interactions". The very title tells you the non-aligned nature of the study. The divergences between myself and the Phys. Rev. referee are simply irreconciliable. The inspection of the correspondence would be (amusing, as well as) instructive, in the sense that we might acquire consciousness of the current, deep, disagreements in strong interactions. Please feel free to ask copy of the correspondence to Dr. D. NORDSTROM. On my part, I have no objection for you inspecting it, with the understanding that should not be released outside the circle of the editorial organization of the Phys. Rev. and Phys. Rev. Letters.

page 3.

By returning to your communication, I believe that the organization of your refereeing process is simply impeccable, and so is, of cour e, that of Phys. Rev.

My only suggestion is related to the actual selection of the two referees. Permit me to be candid in this crucial point. If you receive a paper on strong interactions of non-aligned nature with respect to quarks, and you select for the referees two outstanding experts on quark conjectures, this is virtually equivalent, in my view, to the rejection of the paper at the arrival.

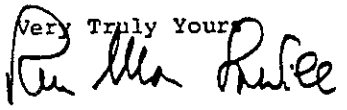
I beg you not to consider this as a criticisms of the past. The situation in strong interactions I am referring to has actually materialized in 1978, even though has been lingering for years. I am making these remarks only in the hope that may be of some value for the future.

The way I handle this situation in my Journal is the following. Whenever I receive a paper on quarks,I send it to two referees, carefully selected as being of opposite views, that is, one quark believer and one quark-non-believer. As you can see from the enclosed Table of Contents of Volume 1, our Journal does indeed publish numerous articles on quarks. This means that I accept papers even though one referee states that it is not only wrong, but fundamentally wrong. Exactly the same approach is followed, without any prejudice, for papers by quark-non-believers, that is, I send them to one quark expert and one of fundamentally different orientation. I feel obliged to this type of refereeing because, the problem of the structure (not the classification) of hadrons is still fundamentally unsolved, and any different attitude would create in me questions of scientific ethics.

The implementation of this type of selection of the referees implies, however, a change in the editorial function. Indeed, as an editor, I have to make a judgment of scientific value, despite opposing reports. But, this was, after all, the historical function of editors. It is only brought to light again by the current disagreements in the physics community.

In closing, permit me to express my sincere esteem in all of you. If I can be of any assistance as a referee (of the quark-non-believers type) or for any other function, please do not hesitate to contact me.

Very Truly Yours,



RMS/ml

Ruggero Maria Santilli

c.c.: Dr. D. NORDSTROM, Editor, PHYSICAL REVIEW D.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

May 7, 1979

Drs. R.K.ADAIR, G. L. TRIGG and G. L. WELLS
Editors
Physical Review Letters
Brookhaven National Laboratory
UPTON, N.Y. 11973

Dear Drs. Adair, Trigg and Wells,

Perhaps, the enclosed paper may assist you in clarifying the contents of my letter to you of April 16. Judging from your lack of acknoledgment of this letter, I am under the impression that my letter was not sufficiently exhaustive.

As you can see, the enclosed paper presents a review of the rather numerous and substantial criticisms on quark conjectures which are moved by rather numerous and outstanding physicists all over the world.

I would like to add here that the contents of this paper is only partial, that is, I have absteined from presenting additional technical criticisms on quarks because of the need, in this case, to refer to specific papers by specific authors.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encl

c.c: Dr. D. NORDSTROM

P.S. The enclosed paper is not intended for submission to Phys. Rev.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
BROOKHAVEN NATIONAL LABORATORY, UPTON, NEW YORK 11973
Telephone (516) 924-5533 (FTS) 664-2540
Telex: c/o BNL, 96-7703 Cable Address: BROOKLAB

PHYSICAL REVIEW LETTERS
Editor
ROBERT K. ADAIR
Department of Physics
Yale University
New Haven, Conn. 06520
Tel. 203-436-1582
HOME: 50 Deepwood Dr.
Hamden, Conn. 06517
Tel. 203-777-2955

May 25, 1979

Prof. Ruggero Maria Santilli
Science Center, Room 331
Harvard University
One Oxford Street
Cambridge, Mass. 02138

Dear Prof. Santilli:

Thank you for your letters of April 16, and May 7. We apologize for not answering you sooner but I suspect that it is possible to prove that a letter addressed to three people has a much better chance of being overlooked that a letter to one. The human condition is such that each of the three assume that one of the others will answer the letter. I have to assume the most guilt, however, as our division of labor rather clearly assigns to me a major responsibility for communication of our policies with our communicants.

As any responsible editor must be concerned with biases of his advisors, we are concerned over the possibility of the formation of schools where the members of one school reject out-of-hand the work of another school. At Physical Review Letters, we do not consider such problems with schools or sets of views as important as for the broader journals of record such as Physical Review D. We do attempt to avoid sending papers which directly attack a narrowly held position to the authors who have established that position but broader questions, such as the question of the character of quarks and the correct place of quantum chromodynamics in physics, we leave to the general community. We take this position (of largely ignoring the possibility of such biases) for a number of reasons, some of which are peculiar to our journal, a journal of selected short communications.

First, while we recognize that physics and physicists follow trends and styles which are not neccessarily founded impeccably on logically sound foundations, we feel that this is not as damaging as you do because we believe that the bias against views counter to the currents of the time is not so great as you intimate. I know that Chew, Mandelstam, Venieziano and others are deeply interested in a description of the strong interactions which may not, and probably cannot, accommodate the simple (or simplistic?) view of quarks which is prevalent but I am confident that the carefully reasoned papers which come from this group are accepted by the publications of the American Physical Society. We are also less concerned over such possible biases than we might be because we do not consider our journal as a complete journal of record. We reject 55% of the papers submitted to us for reasons which do not relate to the correctness of the paper but to the specific fit of the paper to our journal. If we reject a radical paper, which turns out to be an important and seminal paper in physics, we do not feel that we are suppressing the ideas in the paper; there are other journals which can, and should, publish the paper. In the long run, the market place of ideas should act to select the gold from the dross. We do not feel that our selective journal is the proper market, however.

Sincerely yours,

R. K. Adair/glt

R.K. Adair
Editor

RKA/jw

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 30, 1979

Dr. R. K. ADAIR, Editor
PHYSICAL REVIEW LETTERS
UPTON, N.Y. 11973

Dear Dr. Adair,

I appreciated your letter of May 25, 1979. I personally agree with most, if not all your comments. Nevertheless, the voice of concern which I candidly communicated to you, expressed at this time by a minority of our community, in my humble view, deserves a serious consideration by all editors, including myself.

Besides the comments of my preceding letter, the issue touches the question: when is a paper on strong interactions well written? A partial answer is:when the assumptions are carefully and specifically identified, the implications of these assumptions worked out to the necessary rigour, and the results confronted with physical veritas.

The concern I am referring to here is that current papers of quarks or QCD orientation are, in general, grossly deficient when inspected from this profile. The point is that simply none of these papers identifies even partially which are the assumptions and which are the established facts. Even though I could not inspect all these papers (there are too many), in all the papers I personally inspected this was indeed the case.

On more specific grounds, the question that Einstein's special relativity is a mere conjecture at this time for the strong interactions, has been indicated by a number of authors, beginning from the very founders of contemporary physics, and lately presented in numerous papers and even monographs (of course, of non-quark inspiration). In 1978 the HADRONIC JOURNAL lanched, via a series of articles, a moment of reflection on the basic physical laws currently used in quark-QCD-type of studies. This effort, in particular, complemented previous aspects with the identification of the fact that Pauli's exclusion principle, the spin-statistics theorem and numerous other quantum mechanical laws are a mere belief, when referred to the hadronic constituents. These papers, not only have received a rather wide distribution, but they have been even reprinted.

The concern is that all (to my knowledge) papers on quarks-QCD simply ignore the totality of these contributions, and assume in a tacit form the validity of the fundamental physical laws. This concern, in my humble view, deserves a serious consideration for a number of reasons. On scientific grounds, we have here all the ingredients for considering the possible existence of a scientific misrepresentation. Responsible physicists are

understandably concerned of the potential negative implications for the pursuit of knowledge implied by this situation.

In any case, the recommendation is rather specific: authors of quarks-QCD orientation, irrespective of their scientific authority and their status, should clearly identify in their papers each and every assumption of the study without an experimental backing at this time, and then present their studies. Alternatively and equivalently, the recommendation is that these papers should clearly separate what is experimentally established and what is not, what is a conjecture and what is a physical veritas. If they do not desire to enter into this task, they should at least quote the papers by now specialized in the topic.

If this recommendation unrealistic?

A profile which, quite candidly, we cannot ignore (for our own sake) is the financial aspect of funding research in strong interactions. Of course our Journals do not have a direct connection with this financial aspect . Yet, an indirect connection exists, trivially, because the entire refereeing process, as well as that of presentation of proposals, is based on existing literature. A potential insufficiency at the level of papers then clearly propdgates itself at the funding level. This profile should be seriously considered too because a number of valuable physicists have seen their proposals rejected, their tenure refused and are unemployed with a family to support. The thinking by these colleagues is different than ours.

In closing, permit me to stress that I am in the same situation as yours. Indeed, the articles published by my Journal have been written until now in the traditional style (called in the marked the "Phys. Rev. style"), as far as papers of quark-QCD orientation are concerned. Rather than a form of criticism to you, you should interpret this letter as a call to join forces, reach a mature assessement of the situation, subsequently take the necessary steps for an improvement, and help each other.

Sincerely

Ruggero Maria Santilli

RMS/ml
c.c.: Dr. D. NORDSTROM.

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Telephone (516) 924-5533 (FTS) 664-2540
Telex: c/o BNL, 96-7703 Cable Address: BROOKLAB

PHYSICAL REVIEW LETTERS
Editor
ROBERT K. ADAIR
Department of Physics
Yale University
New Haven, Conn. 06520
Tel. 203-436-1582
HOME: 50 Deepwood Dr.
Hamden, Conn. 06517
Tel. 203-777-2955

August 17, 1979

Prof. Ruggero Maria Santilli
Science Center, Room 331
Harvard University
One Oxford Street
Cambridge, Mass. 02138

Dear Prof. Santilli:

I apologize for responding to your letter of May 30 as late as this. I could not answer, responsibly, quickly because I wished to discuss your ideas with others. We editors of Physical Review Letters must not presume to act as arbiters ourselves on scientific matters but to act as arbiters on the communities perception of these matters. I have now discussed the problems which you bring up (as I understand you) and I believe that my correspondents (and I) are not in complete agreement with you. In particular, for almost every scientific paper, the work is based on certain assumptions of the period and I do not believe that it is either practical or desirable that all of these assumptions should be reviewed for each paper. I certainly agree that present QCD theories assume the validity of many concepts which have not really been tested on that scale. I also believe that this is the correct way to procede in physics. But I also believe that it is wise, necessary and altogether a good thing that, occasionally, able people question the bases of present ideas. I believe in quantum mechanics very much as Bohr did, and with comparitively minor caveats, almost every physicist today accepts quantum mechanics in that form. Nevertheless, I have always been pleased that such able people as David Bohm (for example) have continued to question quantum mechanics. But, if I were to act as referee to a paper which used conventional quantum mechanics, I would object to a reference to Bohm as one who questioned QM unless some very

special point of Bohm had beem addressed by the theory or experiment described in the paper. I cannot, then, agree with you that QCD papers should refer to others who have questioned the applicability of special relativity, etc.. I do not see that such a set of references would be useful.

As for those who swim against the stream, I am pleased that some do. Perhaps that is really the right direction. But to swim against the stream is not, in itself, enough; you must get somewhere. Some, like Geoff Chew, are getting interesting results of course and, no doubt there are others, going in different directions, who are finding positive results of value. But this is outside of my competance.

Sincerely yours,

Bob /jw

R.K. Adair
Editor

RKA/jw

September 10, 1979

Dr. R. K. ADAIR, Editor
The Physical Review Letters
Brookhaven National Laboratory
UPTON, N.Y. 11973

Dear Dr. Adair,

I would like to express my appreciation for your letter of August 17, 1979 and for your consideration of my comments.

Nevertheless, I feel obliged to express my disagreement with your views.

The aspect under consideration was, what is called in the trade, the "Phys. Rev. style" of presentation of papers on quark conjectures, QCD, and related topics. In particular, the profile under consideration was the total silence on all papers published by the APS Journals, to my best knowledge, of the fact that the validity of conventional physical laws for the strong interactions (Einstein's special relativity, Pauli's exclusion principle, the spin-statistics theorem, etc.) is a mere belief at this time, deprived of any clear, direct, or otherwise final experimental backing.

Upon consultation with your scientific advisers, you have reached the decision of leaving the editorial status quo unchanged, that is, of continuing the current practice of complete silence on this truly fundamental issue.

I believe that this editorial practice can serve the academic (as well as financial) interests of your advisers but, under no circumstance, this practice can serve the pursue of physical knowledge. If you have convincing counterarguments, I would be glad to reconsider my view.

Also, I believe that this practice is one of the most effective ways of opposing or otherwise delaying the experimental verification of the validity of invalidity of the basic laws considered for the strong interactions, trivially, by avoiding the creation of the awareness in the scientific community of the existence of the problem. Again, if you have convincing counterarguments, I will be glad to reconsider my view.

Whether your scientific advisers agree or not, the conjectural character of the basic physical laws used in quark conjectures on hadronic structure and related studies is a scientific reality. It was lingering in our communities for decades. In 1978 it become technically identified and explicitly stated in a number of articles of the Hadronic Journal. Lately, this situation has been verified in all details in the recent Second Workshop on Lie-admissible Formulations, held at Harvard University from August 1 to 7, by a number of mathematicians and physicists from the USA, France, Israel, Switzerland (plus corresponding participants from the USSR and Australia who could not physically attend the meeting because of lack of travel funds).

It is inappropriate here to quote technical arguments. I would like simply to report historical facts identified by some participants of this workshop. For instance, Wolfgang Pauli made it quite clear in his historical papers and lectures that his exclusion principle was conceived and must be considered as applicable only under conditions of lack of overlapping of the wave packets. The validity of the same conditions for the spin-statistics theorem is then consequential. Simply calculations show that, whether quarks, partons, eletons, or other, the hadronic constituents must be in a state of overlapping of their wave packets. The current, easy, application of Pauli's principle and the spin-statistics theorem in hadron physics is therefore in strict violation of Pauli's teaching.

Similarly, Einstein made it quite clear in his papers, correspondence and teaching that his special relativity was conceived for point-like particles under action-at-a-distance interactions (electromagnetic). It cannot be otherwise because this relativity is a relativistic generalization of Galilei's relativity which, in turn, is fundamentally dependend on the Newtonian concept of point-like particle and action-at-a-distance forces only (variationally selfadjoint forces). Par contre, the point-like approximation of particles under strong interactions (whether hadrons or their constituents) is strictly against the experimental evidence (all strongly interacting particles have a charge radius which coincides with the range of the strong interactions). The current, easy, application of Einstein's special relativity is, therefore, in direct conflict with Einstein's teaching as well as experimental data.*

Enrico Fermi expressed explicitly and quite clearly his doubts on the validity of conventional geometries, relativities and laws for the region of space within strongly interacting particles (you may consult his lectures in Nuclear Physics).

The list of historical reasons of doubts could continue.

What we have done in the literature on the Lie-admissible coverings of the Lie algebras and related formulations is the identification of a number of technical reasons indicating the expected invalidity of conventional laws for the strong interactions under the conditions of overlapping of the wave packets, because of the necessary emergence of forces more general than $f = -\partial V / \partial r$ (variationally nonselfadjoint forces, consequentlial lack of existence of a Hamiltonian, consequential inability to introduce <u>all</u> Lie

* Please, do not quote in this respect the so-called "experimental result" in certain, recent, deep inelastic scatterings of leptons on hadrons indicating a point-like structure of the costituents of the proton. These "experimental results" are nothing more than a <u>theoretical</u> elaboration of experimental data fundamentally dependent on the (primary) <u>assumption</u> of the validity of the special relativity in the conditions considered. Quoting these "experiments" would therefore only serve the purpose of propagating the current controversies from the theoretical setting to the experimental profile.

algebras-let alone those for the SU(2)-spin and of the Poincare group-, consequential applicability of the covering Lie-admissible algebras for the time evolution law under these broader forces, consequential, possible existence of Lie-admissible coverings for the strong interactions of conventional laws of the elm interactions, etc.).

There is no doubt that studies on hadron structure based on the validity of conventional laws must continue, and I have explicitly stated it in my own papers, but, under the condition that the conjectural character of these laws is clearly stated or otherwise formally acknowledged by the "orthodoxy" that is, by your advisers.

The current policy of complete ignorance of this situation by this orthodoxy can at best be identified as a scientific misrepresentation. I would like to be on record by indicating that the potential implications of this situation , not only for the pursuit of physical knowledge, but for the supporters themselves, could be conspicuous if excessively protracted.

One of the primary duties of our profession is to separate beliefs from facts, and to promote the experimental resolution of divergencies. When treating truly fundamental issues, such as that of the basic physical laws for the strong interactions, the fulfillment of this duty becomes mandatory.

I disagree with virtually all passages of your letter. For instance, you indicate your view that "for almost every scientific paper, the work is based on certain assumptions of the period and I do not beleive that it is either practical or desirable that all of these assumptions should be reviewed for each paper".

My comments are the following. Suppose that AT LEAST ONE PAPER ON QUARKS OR RELATED TOPICS BY AN AUTHORITATIVE SUPPORTER (a list of names could be easily formulated at this point) would clearly state and identify the conjectural character of the basic physical laws in his studies on the hadronic structure. Then, I would have accepted your view in its entirety. Indeed, once this first paper of this character appears in the literature, there is no need to repeat the passage in each and every paper along the same lines. The point remains that I do not know even one single paper, by even a less authoritative quark supporter providing this crucial function. How can I then accept your statement without questioning it?

Similarly, you state that "I cannot agree with you that QCD papers should refer to others who have questioned the applicability of special relativity, etc.. I do not see that such a set of references would be useful."

As indicated in my preceding correspondence, there is indeed no need to quote papers questioning the validity of the fundamental physical tool of QCD, the special relativity. This however, under the assumption that the literature in QCD has <u>at least once</u> and in one single paper clearly performed the duty indicated early: the separation of beliefs from facts. When

the totality of the literature in the topic is completely silent on this truly crucial aspect, the perspective of a possible scientific misrepresentation is unavoidable. Again, if you have counterarguments of even a minimum of convincing character, I would be glad to reconsider my view.

I am also under the impression that your advisers are substantially non-informed of the "positive results of value" (in your language) achieved by researchers currently involved in the formulation of experiments for the future resolution of the issue considered (in the series of reprint volumes "Applications of Lie-admissible algebras in physics" we have already published two volumes and are working on two additional volumes). These studies, however, are written for colleagues with scientific humility and vision and they will be likely dismissed by your advisers as exercises of curiosity (the balance is then restored because of a growing number of qualified physicists considering quark oriented studies as exercises of curiosity).

I am also sincerely concerned of your personal condition. I am fully aware that the Editors at Physical Review Letters must act as arbiters of the scientific community. However, you have selected to act as arbiter of only part of the scientific community, by and large, that committed to quark conjectures. But the moment of reflection on the validity of the basic laws for these conjectures has been launched on a world wide basis (e.g., my recent draft "An intriguing legacy by Albert Einstein: the expected invalidation of quark conjectures" has been mailed world wide in 15,000 samples; the announcement of the Second Workshop on Lie-admissibility- centered on the study of the problems considered- has been mailed to all institutions of basic research). This has activated the brains of valuable mathematicians and physicists. I doubt that this scientific drive to resolve experimentally basic issues will be stopped by quark committed physicists. Their opposition, either direct or in the form of ignorance we are referring here, can only promote a process to our scientific accountability. If this moment will indeed arrive, I have no doubt that your current advisers will turn their back to you, in the sense that they will release the totality of the responsibility on your current decision to you.

At the risk of being pedantic, I am recommending here that you and your associates in the Editorial conduction of Physical Review and Physical Review Letters reconsider the situation and your decisions. In particular, I am recommending that you

(1) consider the suggestions by your current advisers for what they are: personal viewpoints of one part of the scientific community completely unsubstantiated at this moment by experiments;
(2) consider my suggestion as a representation of the opposite viewpoint by a minority (at this time) of the scientific community; and
(3) have the literature on Lie-admissibility inspected by scientists with a genuine scientific vision and humility (for your information, the Proceedings of the Second Workshop on Lie-admissibility are scheduled for publication in the December issue of the Hadronic Journal, Volume

2, number 6, 1979).

I discourage the attempt of having the literature in Lie-admissibility seriously inspected by your current advisers. They represent the orthodoxy and, as by now historically established, they will likely die in the belief of being the recipients of the final physical veritas. You will recall, for instance, the opposition by the Academy of France against the idea that meteorites are bodies from our galaxy..... You will recall the opposition by Boltzmann against this strange idea by Planck, so contrary to established classical knowledge..... The list of episodes qualifying the behaviour of the orthodoxy in the pursuit of physical knowledge could be endeless.

What you are facing, however, is not a minute aspect. Instead, it is related to truly fundamental topics, with either a direct or an indirect primary function for energy related issues (think at the controlled fusion as a laboratory construction of bound states of hadrons). We simply cannot afford the luxury of following beliefs by individual physicists on issues of this type. Of course, I expect that your advisers will dismiss as nonsense this energy-related connection. But, such a possible dismissal may later on result to be a further reason to invite a process to our scientific accountability....

A final point which your should bring to the attention of your advisers is the damage, in my view, that they are producing to Physical Review and Physical Review Letters. I am referring here to the fact that your Journals are completely out of the following efforts (at least at this time)
- to achieve a critical inspection of the validity of conventional laws for the strong interactions;
- to achieve covering laws specifically conceived for the strong, under the rejection of point-like abstractions and conditions of overlapping of the wave packets; and, last but not least,
- to achieve maturity of formulation on the only way to effectively conduct physics: the experimental resolution of these issues.

For instance, a number of months ago I submitted to Phys. Rev. D a joint paper with a mathematician and a physicist entitled "Lie-admissible approach to broken SU(2) spin symmetry under strong nonselfadjoint interactions". The paper was specifically intended to promote the experimental verification of Pauli's principle under strong interactions, beginning at the level of nuclear physics where very small deviations might have escaped currently available studies. This paper has been strongly rejected by your advisers or your entourage because "much out of the mainstream of physics". The understanding is that this paper is out of the mainstream of THEIR physics: that made up of personal beliefs for which experimental verifications are strictly excluded.

Similarly, I have tried to recommend to other colleagues the submission of papers along these lines to your Journals, but with complete failure until now. As one colleague put it to me, he does not intend to submit

page 6

any paper to your Journals other than of minute incremental character on established trends, if nothing else, in order "not to be offended by the language of the referees".

Judging from your letter, I have serious doubts whether you are truly aware of the gravity of these occurrences and their implications.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli

367 Linwood Avenue
NEWTONVILLE, Ma 02160

Tel (617 969 3465)

RMS/ml

c.c. Dr. D. Nordstrom
96 North Country Rd
Shoreham, N.Y. 11786

Drs. Adair and Nordstrom,
perhaps, one of you should call me at home (or we should see each other). I have serious reasons of concern. Until I can help you, I am sincerely glad to do so.

Best Personal Regards

Rolly

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

BROOKHAVEN NATIONAL LABORATORY, UPTON, NEW YORK 11973
Telephone (516) 924-5533 (FTS) 664-2540
Telex: c/o BNL, 96-7703 Cable Address: BROOKLAB

PHYSICAL REVIEW LETTERS
Editor
ROBERT K. ADAIR
Department of Physics
Yale University
New Haven, Conn. 06520
Tel. 203-436-1582
HOME: 50 Deepwood Dr.
Hamden, Conn. 06517
Tel. 203-777-2955

September 24, 1979

Dr. Ruggero Maria Santilli
367 Linwood Avenue
Newtonville, Massachusetts 02160

Dear Dr. Santilli;

Thank you for your letter of September 10. I will answer you with the special hope that I can clarify my position. On page 4 of your letter, you write; "I am fully aware that the Editors ··· must act as arbiters of the scientific community ···." But we are not arbiters of science; we certainly do not have, nor do we foolishly claim, that competence. I suppose that we are arbiters of certain minor questions of style but even here we serve as best we can as representatives of the community and we are constantly (and correctly) reviewed even in such matters by the community through the Publications Committee of the American Physical Society. The community acts as arbiters through the referee systems and while I recognize that the community, acting as a kind of committee of the whole, is subject to enthusiasms which are not always well founded, I have great confidence that the general open-mindedness and common sense of the community defines a consensus which is wiser and more fair than any substitute which I can imagine. Of course, I can only sample the community through some choice of advisors and you may well consider that my sampling is deficient but I believe that is is most unlikely that the position I have taken is not approved by a considerable majority of physicists (and I would be disingenuous not to state that that position is in accord with my own beliefs also).

I should not present the technical side of my conclusions with the view of opening a discussion with you -- you can certainly find much wiser men than I for such discussions -- but only as a point of information. My advisors (and I, myself) do not believe that there is any particular blindness in the community towards the fact that the basic laws which you discuss have not been firmly established in the regions of space-time and momentum transfer which are important in elementary particle physics. Though I have been a reasonably active physicist for more than 30 years, I do not know when the applicability of the spin-statistics theorem in particle physics was not questioned! While I have not the time, nor the competence, to penetrate your detailed (and, my advisors say, elegant) discussions, your broader, general statements contain little that I did

not believe that I knew. Both my advisors and myself believe that the present direction of the main flow of particle theory which, tentatively and conservatively, assumes the validity of basic concepts, unproven as they may be, is in the best tradition of physics. As you know well, most theoriests do not believe that it is yet necessary to give up on the basic assumptions which you question and, I believe, that most theoriests consider that these assumptions should not be given up until it is necessary. It will be a long time before we will know who was right and how we should have proceeded. In the mean time, I believe that the journals are appropriately open to substantial contributions which assume the validity of these assumptions or question the assumptions.

All of us, theorists and experimentalists, are quite interested in the possibility of proving -- or disproving -- the fundamental theoretical concepts, such as the spin-statistics theorem, in particle physics. I would be very interested in making such measurements myself if I could be convinced that the measurements would bear strongly on the relevant questions. Needless to say, I must be very careful about committing many man-years of effort and very large sums of money to measurements (and that is what is involved for even simpler particle physics experiments) unless I am strongly convinced that the efforts will be very useful. At the present time, I know of no such possibilities and I do not promise that it will be easy to convince me to attempt such measurements.

Sincerely,

R. K. Adair

RKA/ja

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 23, 1979

Dr. ROBERT K. ADAIR
50 Deepwood Dr.
HAMDEN, Connecticut 06517

Dear Dr. Adair,

I would like to express my appreciation not only for your letter of September 24, 1979 and for your time, but also for its contents and for its style of presentation.

I believe that we are having a valuable scientific interaction, which may be mutually beneficial. Permit me the liberty of expressing candidly my comments The candor of my language is solely intended to communicate with you in the sole language that may be effective for expressing physical issues.

I am in COMPLETE AGREEMENT when you state that
"Both my advisors and myself believe that the present direction of the main flow of particle theory which, tentatively and conservatively, assumes the validity of basic concepts, unproven as they may be, is in the best tradition of physics."
Actually I have rarely seen (these days) a deeper maturity of presentation of the essence of physics : a sequential chain of approximations, which therefore calls for doubts and critical examination of each and every step.

I am in SUBSTANTIAL DISAGREEMENT with the way this style is implemented via the current editorial practices at Physical Review D and Physical Review Letters. These well worded doubts are simply absent in the style of presentation of quark-oriented papers. All I was indicating in my preceding letters is that the style of presentation of quark-oriented papers has received, lately, a negative reaction by an apparently increasing numbers of physicists. Most of them are silent with you. I have selected to express this point to you in the sole intent that it may be of some value to you.

I am in IRRECONCILIABLE DISAGREEMENT when, in regards to the experimental verification of basic laws for the strong interactions, you express the view that
"I would be very interested in making such measurements myself if I could be convinced that the measurements would bear strongly on the relevant questions. "..."I must be very careful about committing many man-years of efforts and very large sums of money ...unless I am strongly convinced that the efforts will be very useful.At the present time, I know of no such possibility."

This view simply establish that YOU HAVE ZERO TECHNICAL KNOWLEDGE OF THE STUDIES OF LIE-ADMISSIBILITY, ZERO KNOWLEDGE OF THE STATUS OF FORMULATION OF EXPERIMENTS, AND ZERO KNOWLEDGE ON THEIR TECHNICAL AND HISTORICAL IMPLICATIONS.

There is little I can do to improve this situation. What it calls for is time, considerable time, to read the literature, which is already quite large, and expanding rapidily. Please feel free to express this views to myself (because you can count on my confidentiality), but I urge you to abstein from expressing views of this type to others, before achieving a necessary knowledge of the literature.

First of all, we have an experiment on the verification of Pauli's principle in nuclear physics that is feasible with current technology, according to the view of a number of experimenters (NOT COMMITTED TO QUARKS), with the understanding that the experiment is predictably delicate and will predictably call for a further joint effort by experimentalists and theoreticians.

Secondly, this experiment is in nuclear physics and, as such, it will cost expectedly less money and time than a corresponding experiment in particle physics. As a matter of fact, this is the reason why we have suggested the initiation of experiments at the nuclear level. Recent studies re-elaborated at the Second Workshop on Lie-admissibility (you may study the Proceedings) have indicated the conceivable existence of very small deviations from the totally antisymmetric character of identical nucleons in nuclei whose charge volume is below that predicted by the proportionality rule with the total number of nucleons. For these nuclei, the nucleons are in an experimentally established, statistically small state of penetration of their wave packets. This is sufficient to activate the Lie-admissible formulations via a very small departure from the conventional Lie's formulations, as representative of small forces nonderivable from a potential. In turn, this implies a small breaking of the SU(2) spin symmetry and, thus, a statistically small departure from the exact fermionic character of the nucleons, under the conditions considered.

Thirdly, your view implies a gross disrespect to the Founding Fathers of contemporary physics. What we are doing IS NOT NEW, as you have stated yourself. We are simply trying to bring the physics community to its senses. The forces we use were suggested by Fermi. The proposed experiment is intended to test FERMI'S LEGACY which you ignore. Furthermore, the forces considered imply a nonunitary time evolution law and, thus, the invalidity of the conventional uncertainty in a small amount. This is exactly Einstein's view on the lack of terminal character of the conventional indeterminacy. The proposed experiment is intended also to test EINSTEIN's LEGACY, which you also ignore when you express doubts on the advisability whether to spend the money. Furthermore, the mechanics of the departures expected from Pauli's principle is necessarily realized at the level of the enveloping algebra (to accomodate broader forces). This is exactly

the view ex pressed by Jordan, von Neumann and Wigner (the enlargement of the envelope of Heisenberg's representations, from the associative to a nonassociative form). As a matter of fact this view by these Masters IS AT THE FOUNDATION OF LIE-ADMISSIBILITY. The experiment proposed is intended to test also this legacy by JORDAN, VON NEUMANN, AND WIGNER, which you also disregard with your attitude on the experimental profile. Yet more, the experiment is also intended to test a RATHER INCONTROVERTIBLE, CLEAR, AND WELL STATED LEGACY BY PAULI: he made it clear that his exclusion principle was conceived under the conditions of LACK of overlap of the wave packets (the atomic structure), trivially, because under conditions of overlap he was expecting "stronger" forces (FERMI'S LEGACY) which would prohibit him to even SEPARATE THE WAVE FUNCTION, LET ALONE TO ESTABLISH ITS TOTALLY ANTISYMMETRIC CHARACTER. Our proposed experiment is intended to TEST THIS TEACHING BY PAULI SO GROSSLY IGNORED, NEGLECTED, AND ABANDONED BY HIS FOLLOWERS.

What shall we do to bring the physics community to its senses? What do you need more than that? Which language shall I use?

Fourthly, we are currently spending billions of dollars of taxpayers money in experiments on strong interactions, ALL based on the assumptiom of the validity of the basic laws, and NONE intended to test the basic laws themselves. In particular, most of these experiments, and most of the most expensive experiments, are devoted to aspects, certainly valuable, but of purely minute incremental character which may, on a long term basis, eventually attract only the attention of curious historians. Your view implies that it is better to continue this status quo, rather than entering into the experimental verification of the basic laws, that is, INITIATE ACTIVE EFFORTS OF TRIAL AND ERRORS, RATHER THAN SITTING PASSIVELY IN AN ATTITUDE OF WAIT AND SEE. This is the reason why I have recocomended you to abstein from expressing views of this type to others. Owing to the large amounts of money spent in conventional stuff, and the comparatively minute amount needed to initiate the test of the basic laws, your attitude might trigger, at the extreme, a process to our scientific accountability.

Fiftly, the most paradoxical aspect, in my view, is the fact that the opponents to these crucial experiments (generally quark committed physicists are simply not aware of the fact that the possible invalidity of basic quantum mechanical laws would leave unaffected the validity of unitary models as well as QCD. This is again due to their total ignorance on the technical treatments of Lie-admissibility. Their minds are simply obfuscated by the unequivocal physical results of these models, in the sense that they are unable to separate what is unequivocally established by these experiments and what is left fundamentally open.

In the Lie-admissible literature we have repeatedly expressed the view that the rather large volume of physical results of unitary models and QCD establish the validity of these models for the Mendeleev-type classification of hadrons only (or, you may say, their "exterior" treatment, or

"chemistry"). The essential character of the CLASSIFICATION has been established by the Nobel assignements for the Ω^- prediction and discovery, and, more lately, by the prediction and discovery of the J/ψ particle and related states. These are results that are, and will remain in the history of physics. No further, potential or actual, advancement of our knowledge can invalidate these results.

Nevertheless, these results DO NOT ESTABLISH THAT QUARKS ARE REAL PARTICLES that is, they do not establish that the same models provide a joint classification of hadrons into unitary multiplets and a structure of each individual member of a given multiplet, all at the same time, all via the same model. This occurrence did not make sense for the atomic phenomenology and there are reason, serious reasons, in our view, that a similar separation classification/structure may eventually result to be necessary at the hadronic level. After all, our efforts on Lie-admissibility are centered in achieving a fundamentally different model of structure, but under the condition that it achieves strict compatibility with the established models of classification. This is exactly along the efforts by Bohr, Thomas, and Fermi to achieve compatibility with Mendeleev. But these Founders of contemporary physics did not search, as the quark physicists do, for one single model capable of representing the totality of the phenomenology considered.

In particular, if you read deeper in the quark literature, YOU DO NOT NEED TO ASSUME THAT QUARKS ARE REAL PARTICLES TO ACHIEVE THE SAME RESULTS. Technically, quarks are representation of a unitary group (apart phenomenological jargon). Thus, the idea of quarks is deeply linked to that of a unitary multiplet. This is, in our view, PURE CLASSIFICATION.

If you read the Lie-admissible literature, you may see that a possible invalidation of conventional laws within a hadron would merely establish this dichotomy classification/structure; leave the physical validity of the unitary models unaffected for the classification profile; and identify their arena of physical relevance: a good, but first-approximation of the hadronic world, under the point-like abstraction of particles (or lack of overlap of the wave packets) as necessary under the validity of the special relativity (in Einstein's own view).
In conclusion, if the legacies by Fermi, Einstein, Jordan, von Neumann, Wigner, Pauli and other will eventually be proved to be true (if physicists stop being passive on the matter and start working on them), this would mean no disaster for the unitary models and QCD, but only the identificatic of the next logical step: a first, but genuine treatment of particles as extended objects under conditions of overlapping of their wave packets and forces beyond the trivial $f = -\partial V/\partial r$.

The true problem for a possible genuine advancement, along the teaching of the Founding Fathers of contemporary physics, is of HUMAN AND NOT OF MERELY TECHNICAL CHARACTER: the desire by the orthodoxy in physics to remain attached to old views as much as possible.

This situation can be best expressed via Heisenberg's words (see his touching memoires "Physics and Beyond",pp.70-71).

> "In science, it is impossible to open up new territory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hadardous leap forward."

To which, he adds, soon thereafter:

> "However, when it comes to enter new territory, the very structure of scientific thought may have to be changed and that is far more than most men are prepared to do."

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

P.S. You might be interested to know that my recent paper "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" has been accepted for publication by a leading Journal other that the Physical Revied D or the Hadronic Journal.

I enclose "Chart 4.9" of my Volume II with Springer-Verlag of "Foundations of Theoretical Mechanics" now in press. This chart (intended in its nautical meaning) may provide you with a quite readable account of the issues here considered, and it is written in a form understandable to graduate students. I would like to stress, however, that the technical treatment is elsewhere, and it is re-elaborated in the Proceedings of our recent Workshop. I would like to bring your attention, in particular, on Part 9, pp. 343-349 of this chart on the historical, authoritative, voices of doubts, so forgotten by our community, so misrepresented, so mistreated, and, lately, so opposed in their experimental verification, or even treatment (see the case of my paper submitted on January 4 at the Physical Review D).

Oct. 30, 1979

Dear Dr. Santilli:

I have received your insulting letter of Oct. 23 and write this note as a termination of our correspondence.

R. K. Adair RKA

PART XIII–B: CORRESPONDENCE ON THE MORATORIUM ON NONRELATIVISTIC QUARK THEORIES AT THE HADRONIC JOURNAL OF 1980

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

BROOKHAVEN NATIONAL LABORATORY, UPTON, NEW YORK 11973
Telephone (516) 924-5533 (FTS) 666-2540, 2544
Telex: c/o BNL, 96-7703 Cable Address: BROOKLAB

February 13, 1980

Dr. R.M. Santilli
Department of Mathematics
Harvard University
Cambridge, Mass. 02138

Dear Dr. Santilli:

I have read with interest your general letter of 8 January to editorial and advisory boards of journals in theoretical physics. As I trust you recognize, the nature of our journal is such that I can take no explicit action regarding the journal in response. However, I am personally interested in the fundamentals of quantum theory. Accordingly, I would greatly appreciate it if you could send me a reprint of your review paper, Hadronic J. 2, 1460-2018 (1979) (your Ref. 3). I infer that it would be a good place to start to learn more about the problem.

Sincerely yours,

George L. Trigg

George L. Trigg
Editor

GLT/jaw

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 14, 1980

TO: The Editorial and Advisory Boards of Journals in Theoretical Physics

FROM: R.M.Santilli, Editor of the HADRONIC JOURNAL

SUBJECT: Follow up of my letter of January 8, 1980

Dear Colleagues,

I would like to express my appreciation and gratitude for your interest in regard to the topic of my letter to you of January 8, 1980 and for the request of more specific information I received from a number of colleagues (I understand that there are difficulties in locating in research libraries the "Foundations of Mechanics" 1979-edition by Professors Abraham and Marsden, and the Proceedings of the Second Workshop on Lie-admissible Formulations, Hadronic J. Volumes 2, number 6 and 3, number 1, 1979).

I have prepared a preliminary, hand written note on my (limited knowledge) on the so-called "theorems of inconsistency of Heisenberg/Lie/symplectic formulations". A copy is enclosed in the hope that can be useful in reaching a first idea of the technical aspects, problems, and issues. Any critical remark, comment, or advice would be appreciated.

You will be pleased to know that a systematic, coordinated study of the issue has been initiated, with particular reference to the editorial profile of papers activating the inconsistency theorems in the various branches of physics (quantum mechanics, quantum field theory, quatum statistics and plasma physics, and quantum gravity). Particularly gratifying has been the answer to our call for help by a number of mathematicians, experts in the field.

At the HADRONIC JOURNAL we have initiated a special file on references (and copies) of past and expected, future, contributions in this (rather intriguing) issue. This information is at the disposal of all of you, as well as of your Referees.

The study of the problem at the THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (August 4 to 9, 1980) has been confirmed, and, again, you are welcome to join us.

We hope that these efforts will result in a precise identification of the problem, as well as the achievement of a mature editorial decision on all quantum mechanical papers with generalized Hamiltonian structures activating the no-go theorems.

Your participation to this scientific effort is appreciated.

Sincerely

Ruggero Maria Santilli
Editor
HADRONIC JOURNAL

RMS/ml

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 16, 1980

Dr. GEORGE L. TRIGG, Editor
Physical Review Letters
Brookhaven National Laboratory
Upton, Long Island, New York 11973

Dear Dr. Trigg,

Your kind letter of February 13, 1980 reached me just while I am leaving for Europe for a few weeks (to deliver invited lectures on the so-called inconsistency theorems). Regrettably, I do not have complimentary copies of my memoir-review (I have zero research funds). I do not have also complimentary copies of the Proceedings (the few available were committed months before their appearance owing to the considerable demand).

Nevertheless, I would like to do my best to assist you. I have therefore instructed Ms. Lyons, my secretary, to mail you my own personal copy of the Proceedings (two volumes). I would be truly grateful whether, after keeping them for, say, one or two weeks, you return them to me. I would need them on my way back from Europe during the second week of March . In case of lack of reception, please feel free to contact Ms. Lyons at this address (tel (617) 495 3352/mornings) during my absence.

Permit me add a few comments, in case of any value to you. The following three different inconsistencies of Heisenberg/Lie/symplectic formulations have come to light.

INCONSISTENCIES IN THE QUANTIZATION OF HAMILTON'S INTO HEISENBERG'S EQUATIONS. These inconsistencies (read, no-go theorems) have been studied in great details by Abraham and Marsden in their recent edition of "Foundations of Mechanics". I would like to encourage you most warmly to look directly at this source because the excellent technical presentation of this volume is reduced in my review to only a few lines (p. 1781). In particular, I recommend the inspection of pages 434-439 (from the definition of quantization in the language of the symplectic geometry to the proof of the lack of its existence).

Incidentally, these initial inconsistencies could be disposed off, from an editorial viepoint, by saying that a new theory should not necessarily admit rules of construction from an old one. In different terms, if these no-go theorems are taken alone, they might not constitute yet reason of concern on editorial grounds .

INTRINSIC INCONSISTENCIES OF HEISENBERG EQUATIONS . Two different types have come to light, and additional ones are forthcoming (judging from possible papers in our Journal). The first is an intrinsic inconsistency of the time evolution law $A|\rangle = (AH-HA)|\rangle$ for all polynomial operators A and H in r and p of at least order three. The best prove of this inconsistency is given in Abraham-Marsden book, page 439. A vulgarized proof is in my memoir. In essence, the value of the commutator depends on the selected use of the differential rule. The inconsistency explodes in the face of sceptics when one shows that $[H,H] \neq 0$ or $= 0$, depending on the computational channel.

Independently from that, Lagrange's and Heisenberg's equations become inequivalent for all Hamiltonians of the same type (polynomial order higher than two, e.g. p^3, p^2r, rp^2, etc). Intriguingly, this inconsistency is completely absen for Hamiltonians of electromagnetic type. This means that the inconsistency is absent also for unified gauge theories of weak and electromagnetic interactions, QCD, and all models with exactly the same structure of the electromagnetic interactions (free term plus an interaction term at most linear in the momentum or derivative coupling). Nevertheless . the inconsistency is activated rather clearly by a number of topics, e.g., nonrelativistic chromodynamics, dissipative nuclear processes, gravitation, plasma physics, etc.

page 2.

This (second) inconsistency has been studied in great detail in Hood's thesis (while I was at Boston University). See also the article by Hellmann and Hood, Phys. Rev. D5, 1552 (1972). A rudimentary summary is presented in a few lines of my memoir (p. 1779).

The relationship between these two inconsistencies is also intriguing, and so is that with others under study (e.g., in Feynman path approach), under the same conditions.

These inconsistencies are an editorial problem, in the view of a number of editors (me included) deserving a serious attention. In essence, we lack at this moment sufficient technical information to reach a mature decision whether to accept, or reject, or hold papers activating these inconsistencies. The reasons are rather clear. Suppose you reject by fiat the use of the differential rule hoping to salvage old stuff, but then you cannot escape from inconsistencies at the Heisnberg-Lagrange level, as well as at the level of the presumed equivalence Heisenberg-Schrödinger equations. Similarly, suppose you assume as "true" Heisenberg equations (to try to salvage QM) and claim as "untrue" Lagrange's equations. But then QCD is at stake because based on "untrue" equations. Similarly, suppose you claim as "true" Lagrange's equations (to salvage QCD) and as"untrue" Heisenberg's equations. But then QM is at stake (these are some of the "suggestions" I received to salvage as much as possible old knowledge).

INCONSISTENCIES IN THE DIRAC'S LIMIT OF HEISENBERG'S INTO HAMILTON'S EQUATIONS. These inconsistencies were studies at our Workshop and are reported in p. 1780 of my memoir. They can be interpreted as an "inverse" formulation of Abraham-Marsden no-go theorem of quantization, but the implications are different, particularly in regard to the presumed equivalence Heisenberg-Schrödinger representations.

The idea of our Third Workshop (scheduled for August 4 to 9, 1980) is to gather mathematicians, physicists and editors in a selected and restricted number (maximum 20-23, to avoid dispersal of energies), and conduct a study of the problem. The hope is to achieve some valuable information for us on how to handle papers activating the inconsistencies (and they are quite numerous, in my view). The understanding is that academic dances of mumbo-jumbo hand waving, (such as "Heisenberg's equations are true and Lagrange's equations are false') are ignored, and the advice by specialists, experts in the field is taken in due account.

As of this moment, it appears that the response is promising for rendering this meeting a reality (despite the predictable existence of questionable opposition).

We would be sincerely pleased to have you with us. In case you can attend, please let me know in advance, so that I can secure for you the best possible accomodation.

Also, It would be a pleasure for me to meet you before the Third Workshop, and have a friendly, relaxed, informal exhange of views in this intriguing situation. Beginning from the third week of March, 1980, you would be most welcome here in Cambridge, or at your discretion, I would be glad to drive to Yale.

As a final comment, you might be interested to know that this situation was triggered by a paper on nonrelativistic chromodynamics. A leading expert on quarks recommended the paper for publication as excellent, but a mathematician expert in quantization indicated that the paper was fundamentally inconsistent. I therefore recognized that my scientific accountability was at stake here. My letter to editors-colleagues of January 8, 1980 was motivated by the desire to share this experience with all interested physicists, even though I was fully aware that the letter is strictly anti-career-oriented, as far as my future is concerned. This is a fact of contemporary academic life.

Sincerely

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL
RMS/ml

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

March 19, 1980

TO: Mathematicians interested in quantum mechanics
FROM: R.M.Santilli, Editor of the Hadronic Journal
SUBJECT: call for help for an intriguing editorial impasse

You might be interested to have some information about an editorial impasse which occurred recently at the Hadronic Journal. It concerns all physics articles in nonrelativistic quantum mechanics based on Heisenberg's equations (and related physical laws) with generalized Hamiltonians of the type

$$H_{gen}(q,p)=T_{gen}(q,p)+V(q,p)\,;\ \text{Polyn.Order}\ T_{gen}(q,p) \gtrsim 3;\ \ V(q,p) = \text{linear in } p, \qquad (1)$$

e.g., $H_{gen} = \frac{1}{2}pF(q)p + V(q)$ (Nota Bene: the impasse excludes conventional Hamiltonians $H = T(p) + V(q,p)$ with Polyn. Order $T = 2$, as occurring for electromagnetic interactions).

A significant number of papers in different fields are involved in this intriguing case, with particular reference to: nonrelativistic quark dynamics; nuclear physics; quantum statistical mechanics; plasma physics; controlled fusion; and quantum gravity.

The impasse originated with the submission to the Hadronic Journal of a comprehensive paper in nonrelativistic quark dynamics (for which the use of generalized Hamiltonians is necessary to achieve meaningful mass spectra). The paper was recommended for publication by qualified referees. But other, equally qualified, referees recommended the rejection quite firmly. The inability to resolve the technical differences between these equally qualified, opposing views, resulted in the impasse. The fact that the problems originate in the generalized structure of the Hamiltonian, and the joint use of conventional laws, suggested the extension of the impasse to other fields.

To the best of my understanding, the problematic aspects underlying the impasse are the following.
Problematic aspects in the quantization. As known in mathematical circles, a theorem by Abraham and Marsden (following notes by Chernoff, as well as preceding contributions) (ref.1) establishes the lack of existence of the full quantization for the models considered. A first group sees no problem in this, on the basis that two different disciplines should not necessarily admit a map. A second group disagrees on the basis that, to prevent possible intrinsic inconsistencies of quantum mechanical models, the problematic aspects of quantization should equivalently occur for all quantum representations (e.g., those via Heisenberg's equations, via Schrödinger's equation, via Lagrange's equations, etc.). The issue is therefore whether or not the various representations of quantum mechanics are consistent (that is, mutually compatible) from the viewpoint of quantization, e.g., whether or not the Abraham-Chernoff-Marsden theorem admits a form of image for the quantization of the Hamilton Jacobi into Schrödinger's equation. To my knowledge, no contribution by mathematicians exists on this topic at this time.
Intrinsic problematic aspects. Generalized Hamiltonians (1) activate a lemma by Hellman and Hood (ref.2) according to which, for the Hamiltonians considered, Heisenberg's equations are not necessarily equivalent to the (operator) Lagrange's equations (for conventional Hamiltonians this problem does not exist). A first group dismisses this occurrence, e.g., on grounds that there exist transformations $(q,p) \to (q',p')$ mapping $H_{gen}(q,p)$ into $H'_{conv.}(q',p')$. The equivalence between Heisenberg's and Lagrange's equations is then

regained (under boundedness and other conditions inessential here) for the transformed Hamiltonian, as often used, e.g., in path integral approaches. A second group disagrees quite vigorously on a number of counts, e.g.,

(a) Generalized Hamiltonians violate the imprimitivity theorem (ref.3, p.204) for a genuine validity of Galilei's relativity. Thus, the transition from conventional to generalized Hamiltonians may imply the loss of Galilei's relativity, and, thus, of the notion of Galilean quantum particle.

(b) When the equations of motion are computed explicitly, generalized Hamiltonians imply nonconservative, nonlinear, velocity-dependent forces. In this case, the systems are open, that is, they violate the conservation of total physical (rather than canonical) quantities, such as, total angular momentum, energy, etc. (hint: for Hamiltonians (1) the symbol "p" does not represent the physical linear momentum $m\dot{q}$). This appears to confirm problematic aspects (a).

(c) The time evolution of open systems in the vector field form with local variables q and p = physical linear momentum is noncanonical at the classical level, and nonunitary at the quantum level for coherence of the theory under the classical limit. Under a nonunitary time evolution, most of the conventional laws and principles of quantum mechanics (e.g., Pauli's exclusion principle; Heisenberg's indeterminacy principle; etc.) are not preserved, as shown in ref. 4, pp. 1865-1888. Similarly, the transformations mapping $H_{gen}(q,p)$ into $H'_{conv}(q',p')$ are generally noncanonical at the classical level, and nonunitary at the quantum level. The equivalence of Heisenberg's and Lagrange's eqs. would be then regained at the loss of the basic physical laws. This confirms the problematic aspects for the conventional notion of Galilean quantum particle.

The implications of these occurrences are nontrivial. For example, for models of plasma physics with Hamiltonians (1) the validity of Pauli's exclusion principle is open (theoretically and experimentally, to my best knowledge); for models of dissipative nuclear processes with Hamiltonians (1) the validity of Heisenberg's indeterminacy principle is unresolved at this moment (also theoretically and experimentally, to my knowledge); for nonrelativistic quark models, the problematic aspects prevent at this time a consistent, quantitative, formulation of the hypothesis that quarks are physical Galilean particles, without affecting the physical content of these models as far as the Mendeleev-type classification of hadrons is concerned (the classification can be conducted via spectrum generating, Schrödinger-type equations for which no problematic aspect is know at this time).

Problematic aspects in the classical limit. Even though not universally accepted, classical mechanics is expected to be admitted by quantum mechanics under "a" suitable limit, for the logical coherence of the theory. The open problems are here numerous. For instance, we do not apparently know at this time whether the Abraham-Chernoff-Marsden theorem admits a form of "inverse". Also, we do not know whether problematic aspects in the limit of Heisenberg's into Hamilton's equations equivalently exist for the limit of Schrödinger's into Hamilton-Jacobi equations. The background issue is whether the various representations of quantum mechanics are mutually compatible under the classical limit (ref.5).

Any critical comment, remark, or advice would be gratefully appreciated. To assume full responsibility, I enclose copy of my ref.5 providing an outline of the problematic aspects, while I remain at the disposal of interested colleagues for more specific information.

REFERENCES

(1) R.Abraham and J. E. Marsden, Foundations of Mechanics, Benjamin/Cummings (1979 edition)

(2) W.S.Hellman and C.G.Hood, Phys. Rev. D5, 1552 (1972)

(3) G.W.Mackey, Unitary Group Representations, Benjamin/Cummings (1978 edition)

(4) R.M.Santilli, Hadronic J. 2, 1460 (1979)

(5) R.M.Santilli, Hadronic J. 3, 854 (1980)

P.S. Some of these open problems are contemplated to be studied at the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS scheduled in Cambridge, Ma, from August 4 to 9, 1980.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE NEW YORK 11961
Telephone (516) 924-5533

May 22, 1980

Dr. R.M. Santilli
Department of Mathematics
Harvard University
Cambridge, Mass. 02138

Dear Dr. Santilli:

Thank you for lending me the material from the workshop on Lie admissibility. I apologize for having kept it longer than the two weeks or so that you had suggested; I hope that this did not cause you any difficulties.

I find, to my regret, that my familiarity with modern abstract algebra is sufficiently sketchy that I was not really able to appreciate much of the argument. I cannot help feeling, however, that your campaign calls for much more drastic action than is really warranted. As you must be aware, this is not the first instance in which physics theory has made progress on the basis of questionable mathematics, nor is it likely to be the last. I do not mean in any sense to disparage the work that you and others are doing to try to provide a sounder basis; but I do not feel that a moratorium of any sort would be useful.

I thank you again for lending me the material, and I offer my wishes for success of the forthcoming workshop. I regret that my schedule does not permit me to attend.

Sincerely yours,

George L. Trigg

George L. Trigg
Editor

GLT/jaw

PART XIII–C:

REJECTION OF A PAPER ON THE EXPERIMENTAL VERIFICATION OF PAULI'S EXCLUSION PRINCIPLE IN STRONG INTERACTIONS

January 5, 1981

Dr. D. NORDSTROM, EBitor
The Physical Review D
Brookhaven National Laboratory
Upton, Long Island, N.Y.

Dear Dr. Nordstrom,

I have now concluded a series of consultations in regard to my paper "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions", which was submitted to your Journal on October 4, 1980. I am now ready to prepare a revised version. In particular, I would like to implement the following changes.

(1) The paper is essentially intended to solicit the experimental measurements of the intrinsic quantities of hadrons under strong interaction (spin, magnetic moments, etc.) This knowledge is clearly useful for energy issues (the controlled fusion). Clearly, if the magnetic moment of nucleons mutates (in our Lie-admissible language) under the conditions of the strong interactions in general, and those of the controlled fusion in particular, the magnetic confinement calls for suitable implementations. As a first point I would to attempt a better identification of this primary objective via a few introductory remarks.

(2) It has been brought to my attention by a number of colleagues that the mutation of the magnetic moment is an old idea in nuclear physics. In fact, conventional theories cannot interpret the magnetic moment of nuclei (see the Schmidt limits). This simple interpretation of an experimental fact was subsequently abandoned because of the predominant theoretical belief that the intrinsic characteristics of hadrons as measured under long range elm interactions remain the same under the additional presence of the strong and the conditions of wave overlappings. Also in the introductory part, I would like to point out this occurrence (e.g., Blatt-Weiskopf, Theor. Nucl. Phys., p. 31 clearly state in p. 31 the expectation that the magnetuc moment of nucleons change under nuclear conditions). The relevance with the paper is selfevident. In particular, it is quite difficult to construct a quantitative model whereby the magmetic monent mutates and the dpin remains the same.

(3) As directly recommended to me by Professor Rauch of the Atominstitut of Vienna, Austria, during a recent viist of mine at his institute, his experiments in neutron interferometry are capable of testing directly the relationship between magnetic moment and spin because the angle measured for the 4 symmetry is directly linked to the mageetic moment. After all, the precession which is measured is due to a megntic field. Thus Rauch's experiments, if properly repeated, for instance, along the alternatives suggested in my paper, could likely produce an experimental resolution of the issue. The understanding is that the achievement of this experimental knowledge is not opposed.

(4) I have several improvements of details, such as the fact that the actual improvement of the fit via Lie-admissible mutation calls for two-sided representations, and cannot be achieved via the linear one-sided mutation considered in the paper.

(5) On editorial grounds, I have also numerous improvements to implement throughout the paper. In particular, and following a kind suggestion by Professor Okubo and other colleagues, I shall remove from the paper any mention of the quark conjectures.

Since the submission of the paper on October 4, 1980, I have not received comments or referee report from you.

Please considered the revisions indicated earlier in this letter and, in case appropriate, let me have your comment and or advice. Also, any other contructively critiaal criticism -would be particularly helpful for the finalization of the paper.

I would like to take this opportunity to wish to you and to your Journal a happy and prosperous 1981.

Sincerely

Ruggero Maria Santilli
Professor of Physics
University of Massachusetts in Boston

RMS-ms

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BOX 1000 - RIDGE, NEW YORK 11961
Telephone (516) 924-5533

Physical Review D
Editor
D NORDSTROM
Associate Editor:
STANLEY G. BROWN

21 January 1981

Dr. R. M. Santilli
28 Cross Street
West Newton, MA 02165

Dear Dr. Santilli:

We have received your letter of 5 January regarding your proposed revisions in your manuscript entitled "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions". Just before receiving your letter we received the report of one of our referees on your manuscript. A copy of the report is enclosed.

The serious objections in the enclosed report should be considered before any revisions in the paper are undertaken. Of the three objections listed in the report the third one is of particular concern to us from an editorial standpoint. In your submittal letter you stated that "This paper essentially presents one of the primary results of the recent Third Workshop in Lie-admissible Formulations". According to Reference 5 of your paper the proceedings of this workshop were to be published last year. Thus the implication is, as the referee suggests, that much of the paper "appears to be a rewrite of already published ideas." There would then appear to be little new material in the paper that would warrant its publication.

The delay in obtaining a report on your paper resulted from the very severe constraints on referee selection requested in your submittal letter. We sent the paper to one referee who recommended a

second referee, the individual who returned the enclosed report.

We are returning your manuscript for your consideration of our comments.

Yours sincerely,

D. Nordstrom
Editor

DN:cp
enc.

REPORT OF THE REFEREE:

This paper is unacceptable for several reasons:

1. The claim that this theory gives a better fit to the data is invalid. The data agree perfectly with standard theory, since the experimental error limits enclose 720°. Consequently, any suggested improvement is meaningless.

2. None of the proposed experiments are substantive. Anyone can ask for better accuracy or for a thermal beam of neutral kaons. The Physical Review need not publish idle dreams. (We need constructive suggestions.)

3. Aside from the sections commented on above, the rest of the paper appears to be a rewrite of already published ideas.

Ruggero Maria Santilli

Editor in Chief
Hadronic Journal

[redacted]

February 3, 1981

Dr. D. NORDSTROM, Editor
The Physical Review D
1 Research Road
Box 1000, Ridge, New York 11961

Dear Dr. Nordstrom,

Thank you for your letter of January 21, 1981 in regard to my paper "Experimental indications for the inapplicability of Pauli's exclusion principle for strong interactions".

Permit me to reassure you that the paper was original at the time of the submission on October 4, 1980, and so is still today. The originality and novelty of content relies on the presentation, apparently for the first time, of the fit of experimental data for spinor symmetry via the SU(2)-admissible treatment of the broken SU(2)-spin symmetry. I believe that the sentence you refer to should be extended to read "the rest of the paper appears to be a rewrite of already published ideas", which is indeed correct.

In regard to timing your referee was only partially informed. In fact, the Proceedings of the THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS (which will treat the issue in all necessary detail) have not been published in December, have been delayed for several reasons, and they will appear perhaps in late spring.

The issue is therefore reduced to the capability and-or possibility by your office to process the paper as any other paper calling for refinements of existing experiments (which is a considerable percentage of your publications), and which is apparently processes in one-to-two months. Also, please keep in mind that I have funds for paying the publication charges.

On my part, I can provide you with the final revised version in a matter of days. However, quite frankly, my time is very very limited due to the multiplication of invitations to deliver speeches on the topics, as well as research activities. I will be happy to spend the necessary time, but with the understanding that the paper will receive a serious review.

My comments on the clearly political referee report are enclosed. In case you suggest more moderate comments, please let me know, and I shall rewrite them.

Sincerely

Ruggero Maria Santilli

RMS-ml

AUTHOR'S COMMENTS ON THE REFEREE REPORT OF PHYSICAL REVIEW D ON THE PAPER

Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions

DATE OF RECEPTION OF REPORT: January 30, 1981; DATE OF SUBMISSION OF PAPER: October 4, 1980

Objective of paper. To suggest the refinement of experiments on the so-called spinor symmetry via neutron interferometers and the measure of intrinsic characteristics of particles (spin, magnetic moment, etc.) under strong interactions. These characteristics have been measured countless times under long range electromagnetic interactions, but no direct or final experimental knowledge exists at this time for the same characteristics under strong interactions.

Relevance of paper. The achievement of the physical knowledge advocated by the paper is important for a number of selfevident aspects in physics, mathematics, and engineering. To reach a judgement of the referee report it is useful here to recall the importance of the advocated physical knowledge for the controlled fusion. In fact, the magnetic confinement, as an example, is rather crucially dependent on the value of the magnetic moment of nucleons under the conditions of the controlled fusion (strong interactions at very high pressures, densities and temperatures). The reader is encouraged to reflect on the financial implications of the issue.

Clear objective of referee. To prevent the achievement of this physical knowledge.

PRELIMINARY OPEN QUESTIONS.

1. The referee was aware of the date of publication of the PROCEEDINGS OF THE THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS (December 1980), judging from available material. The contents of the paper will be treated at length in these proceedings. At the same time, the referee report is the result of a few minute work (because it contains no scientific elaboration whatsoever, but mere statements of personal views). Yet, the report was delayed several months. Is this a mere coincidence, or a planned machination to achieve scientific obsolence of the paper?

2. The statement that "The data agree perfectly well with standard theory" is known to be false. First, the 720^{o} of spin precession needed for the validity of the "standard theory" are missing in a number of experiments. Second, all the times the 720^{o} are admitted by the data, they barely enter within experimental error and are far from the median value needed for the "standard theory". Third, and more importantly, the recovering of the 720^{o} of spin precession is only a part of the requirement to establish the "standard theory" under strong interactions. A number of additional insufficiencies exist, are well known, and some of them are reviewed in the paper. For instance, there are clear clusters of points outside the curve needed for the validity of the "standard theory" which have no explanation at this time other than that via the breaking of the "standard theory" and its Lie-admissible generalization (conventional interpretations are not excluded here; it is simply stressed that they are lacking). Owing to these clear occurrences, the question opened by the referee report is the following: why has the referee selected a sentence which is known to be false? Was this only an unfortunate error due to a genuine selfconfidence? Or the selection was done because of financial-academic-ethnic considerations?

3. The statement "none of the proposed experiments are substantive" is doubtful at best. The paper predicts a breaking of the SU(2)-spin symmetry under strong interactions and recommends specific experiments for its verification. If this prediction will eventually result to be correct, a fundamental part of contemporary theoretical physics must be reinspected. Is there in the current literature a proposal more substantive than that? The referee appears to be fully aware of this aspect. Yet, he states the opposite. WHY?

MORE SUBSTANTIAL OPEN QUESTIONS.

4. The report has all the ingredient of scientific discrimination in the following sense. A considerable number of papers published by Physical Review (and other Journals) refers to improvements of established knowledge of aligned character. It is an easy prediction that this referee would have supported proposed experiments of this nature, say, an improvement of the current value of the magnetic moment of the nucleons under electromagnetic interactions, or a test of QED at very small distances. Yet, this referee opposes the repetition of experiments on the spinor symmetry. WHY?

5. It is assumed that, to qualify as referee for Physical Review D, this referee has received a good physics education, including nuclear physics. At any rate we must expect that the referee has studied Blatt-Weisskopf, Theor. Nuclear Physics, and that he has read the statement by these authors

> *"It is possible that the intrinsic magnetism of a nucleon is different when it is in close proximity to another nucleon." (loc. cit., p.31).*

The paper submitted simply calls for the experimental verification of this possibility. WHY IS THE REFEREE OPPOSED TO THE ACHIEVEMENT OF THIS PHYSICAL KNOWLEDGE?

6. Experiments on the measure of the intrinsic characteristics of particles under strong interactions undermine the very foundations of the contemporary financial-ethnic interests of the academic world. In fact, possible deviations from the magnetic moment and spin, if experimentally established, would imply the invalidation of Einstein's special relativity and the need for more adequate theories. In turn, this is expected to imply the invalidation of quark conjectures (because quarks are crucially dependent on their very definition on the special relativity). Is this referee a bona fide believers of standard views? Or is this referee an exponent of these financial-ethnic-academic interests? To prove his good faith the referee should give TECHNICAL arguments establishing the validity of standard views, and, to achieve credibility by the scientific community

at large, these arguments MUST NOT be based on a plurality of experimentally unverified assumptions (for instance, the arguments must be completely independent of quark conjectures). WHY NO TECHNICAL ARGUMENT IS PROVIDED BY THE REFEREE IN SUPPORT OF HIS SINCERETY? AND, AT ANY RATE, WHERE ARE THOSE TECHNICAL ARGUMENTS? IN NUCLEAR PHYSICS THE EVIDENCE IS MUCH IN FAVOR OF A MUTATION OF THE MAGNETIC MOMENT AS CLEARLY STATED IN A NUMBER OF WELL WRITTEN SOURCES. IN HADRON PHYSICS THE ISSUE IS UNRESOLVABLE AT THIS MOMENT BECAUSE OF THE CUSTOMARY REDUCTION TO QUARK ARGUMENTS, THAT IS, TO A PLURALITY OF PERSONAL VIEWS BY INDIVIDUALS. WHERE ARE THEN THE TECHNICAL ARGUMENTS SUPPORTING THE SCIENTIFIC CREDIBILITY OF THE REPORT?

CONCLUDING COMMENTS.

This author would have accepted with gratitude a critical report by the referee, but only under the uncompromisable condition that he would have FIRST stated clearly his support for the experiments suggested, and then entered into all deficiencies of the paper for the achievement of the objective. This has not been the case. The referee has quoted as "dreams" the prediction of the paper. This is in flagrant disagreement with the expectation of nuclear physics. Also, this is in serious disagreement with the social needs to achieve the controlled fusion and, thus, on the social need to reach scientifically credible data on the intrinsic characteristics of particles under strong interactions. But, most of all, this is in disagreement with centuries of tradition whereby sound physical knowledge is achieved via direct and clear experiments. Different views can at best qualify as scientific politics, but not as the pursue of human knowledge.

This author recommend the most vigorous possible condamnation of attitudes of the type reported here. Lacking this action the risks are selfevident. For instance, by keeping in mind the size of the financial investments in the controlled fusion, a rather natural question is:

HOW LONG CAN WE DELAY THE MEASURE OF THE INTRINSIC CHARACTERISTICS OF PARTICLES UNDER STRONG INTERACTIONS WITHOUT RISKING A COMPLETELY UN-NECESSARY CRISIS, SUCH AS A SENATORIAL INVESTIGATION ON THE MATTER ?

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Physical Review D
Editor
D NORDSTROM
Associate Editor:
STANLEY G BROWN

14 April 1981

Dr. R. M. Santilli
28 Cross Street
West Newton, Massachusetts 02165

Dear Dr. Santilli:

Your manuscript entitled "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions" was returned to the referee along with a copy of your response to the referee's first report. A copy of this referee's second report is enclosed.

We also contacted a second referee on your manuscript. We enclose a copy of the report excerpted from the comments of the second referee.

In view of the enclosed reports we regret to inform you that we cannot accept your paper in its present form. We are therefore returning your manuscript.

Yours sincerely,

D Nordstrom

D. Nordstrom
Editor

DN:cp
enc.

SECOND REPORT OF THE FIRST REFEREE:

My opinion has not changed. I do not recommend publication.

REPORT OF THE SECOND REFEREE:

This paper is very poor, basically confused on physical issues, and is definitely not publishable. In this I agree fully with the report of your (experimental) reviewer. In my opinion the author's remarks on spin are totally unfounded and seriously flawed.

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

July 16, 1981

Dr.D. NORDSTROM, Editor
The Physical Review D
Brookhaven National Laboratory
UPTON, Long Island, New York

Dear Dr. Nordstrom,

As a gesture of courtesy, I enclose copy of my solicitation for the year 1981 to Dr. Vineyard to initiate active studies at Brookhaven on the open problem of the basic physical laws.

The pressing need for these studies has been elaborated in the letter, as far as the physical aspect is concerned. The editcrial aspect is transparent. In fact, the lack of initiation of these studies in national laboratories favors academic mumbo-jambo of the type of the referee report of my article submitted to Phys. Rev. D: "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions", as per your recent letter (April 14, 1981).

The terms "academic mumbo-jambo" are the gentlest I can found to qualify these referees. The second claims that my work is "totally unfounded and seriously flawed". He may be true, of course. But to prevent the suspicion of mumbo-jambo the referee should have proved rigorously the statement with all due math. Ventitations of statement of the type this referee has, withour any justification, do nothing more than confirm the view by the famed philosopher at Berkeley, Paul Feyerabend, according to which contemporary physics is conducted via "subterfuge, rethoric,and propaganda." (reference is first to Journals .....).

As I indicated earlier in our correspondence, I reject referee report of this type at the HADRONIC JOURNAL, and I recommend you again to do the same at PHYSICAL REVIEW D. It is the only way our Journal can serve the pursue of knowledge, rather than the pursue of scientific politics.

In the past I have absteined from contacting other members of the Editorial Board of the Phys. Rev. D, such as the Editor in Chief, and I shall continue to do so as a gesture of courtesy to you. Please reinspect again the issue. In case I can bring the case to the attention of the high ranks at Phys. Rev. without causing you any inconvenience, please let me know (phone (617) 964 1684).

For your information, the crucial experiment by Rauch et al on the SU(2) spin symmetry to which my paper was addressed, has been recently re-elaborated by the Authors at the Atominstitut of Wien, Austria. The new value is $\alpha = 715.87 \pm 3.8^{\circ}$ which DOES NOT INCLUDE THE 720° OF THE EXACT SU(2)-SPIN SYMMETRY!! The ultra-mumbo-jambo of the referee is now even more clear (the physical foundations and theoretical rigour of the SU(2)-spin/symmetry-breaking has been established beyond doubt in the literature via the experimentally established wave overlapping; consequential contact, nonlocal, nonpotential forces; consequential nonunitary time evolution at the level of each individual particle;

and, finally, consequential alteration of the electromagnetic spin values). You will see soon the new value published in the literature.

But, what is truly disturbing, and I still cannot accept with grace, is the opposition of the referees to experiments. The words "totally unfourded and seriously flawed" are indeed intended to prevent even the consideration of the experiments recommended. If these people are in good faith, WHY DO THEY FEAR EXPERIMENTS WHICH MAY CONFIRM THEIR VIEWS? After all, the exact SU(2)-spin symmetry may indeed be established experimentally under strong interactions. I cannot accept positions of this type to prevent the feeling of being their accomplice, in an apparent machination to prevent the achievement or otherwise the establishing of fundamental physical knowledge.

Sincerely

Ruggero Maria Santilli

RMS-ml

You are here warmly encouraged to mail copy of this letter to the anonimous referees.

D13.5

DX1317

EXPERIMENTAL INDICATIONS FOR THE INAPPLICABILITY OF PAULI'S EXCLUSION PRINCIPLE UNDER STRONG INTERACTIONS

Ruggero Maria Santilli*

Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

(RECEIVED 7 OCTOBER 1980)

Abstract

Recent experimental data on the 4π symmetry of the wave-function of neutrons, obtained via neutron interferometer experiments, are inspected in detail. It is shown that the Lie-admissible treatment of the broken SU(2)-spin symmetry under strong interactions is not only compatible with available experimental data, but actually produce a fit better than that for the exact symmetry. It is stressed that, despite these results, the available experimental information is still unable to rule out for the strong interactions the familiar notion of spin as established for the electromagnetic interactions. A number of specific experimental tests are proposed for the final resolution of the issue either in favor or against the conventional notion of spin and related physical principles, such as Pauli's exclusion principle.

*/Supported by the DEPARTMENT OF ENERGY under contract number DE-AC02-80ER10651

PART XIII–D:

REJECTION OF A THEORETICAL AND AN EXPERIMENTAL PAPER ON TIME–REFLECTION ASYMMETRY IN STRONG INTERACTIONS

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 16, 1982

Dr. GEORGE L. TRIGG
Editor
PHYSICAL REVIEW LETTERS
.1 Rsearch Rd
RIDGE, New York 11961

Dear Dr. Trigg,

I hereby submit for publication in the Physical Review Letters my note entitled "Use of the hadronic mechanics for the best fit of the time-asymmetry recently measured by Slobodrian, Conzett, et al"

For this purpose, I enclose:
(a) Three copies of the note;
(b) two copies of a few separate calculations for referee use (particularly for referees who do not know the "hadronic mechanics");
(c) a collection of the most important experimental and theoretical papers quoted in the note (the theoretical ones being mostly unavable in the Journals of the AIP);
(d) a duly signed copyright agreement; and
(e) the PACS categories: 11.30 Er and 24.70 +s.

In submitting this note, permit me to ensure my best possible collaboration for referee comments, suggestions and criticisms based on explicitly presented elaborations and calculations. I would therefore consider it a personal courtesy whether you encourage the referees to avoid the presentation of unsubstantiated personal opinions and views.

In submitting this note, I would like also to express the concern of a segment of our community for the amount of time that resulted to be needed for Physical Review Letters to publish the experimental results of the international collaboration Berkeley-Quebec (and Bonn) treated in the note (compared to the rapidity with which the Los Alamos rebuffal was passed by Phys. Rev. C). I would like therefore to ask, most respectfully, that this note be processed within the period of time internationally considered appropriate for a letter (say, two months), or that you kindly inform me of foreseable delays.

I remain at your disposal for any assistance you may need.

Sincerely,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS; mlw
encls.
PS: Publication charges will be paid by the IBR.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES
1 RESEARCH ROAD BOX 1000 RIDGE NEW YORK 11961
Telephone (516) 924-5533

20 May 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Re: Use of the hadronic mechanics for the best fit of the time-asymmetry...
By: Ruggero Maria Santilli

LR2111

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

enc.

R2L

Referee's report on LR2111, "Use of the Hadronic Mechanics..." by R. M. Santilli

This manuscript presents a great deal of formalism, the physical significance of which escapes me, which is said to be inspired by an experimental study of ^{7}Li(^{3}He,$\vec{p}$) and ^{9}Be(^{3}He,$\vec{p}$) and inverse reactions by Slobodrian, Rioux, Roy, Conzett, von Rossen and Hinterberger (ref. 1 of the manuscript). It is my understanding that the general concensus of the nuclear physics community is that the data shown by Slobodrian, et al., indicating a large difference between the polarization of the protons produced in these reactions and the analyzing power of the inverse reactions, are not correct. A repetition of the ^{9}Be(^{3}He,$\vec{p}$) and inverse reaction measurements by Hardekopf, et al., Phys. Rev. 25, 1090 (1982), yielded data in disagreement with the measurements of Slobodrian, and found agreement between the polarization and analyzing power, as one would expect from time-reversal-invariance.

Even accepting the results of Slobodrian, et al., which I do not, the purposes of the present manuscript remain obscure. After many equations of exceedingly general and elementary aspect, expressed in a bizarre notation which is said to be "hadronic mechanics," the author comes to the conclusion (p. 5) that "the ratio between the analyzing power of the forward reaction and the polarization of the backward reaction is equal to the ratio of the corresponding units of the enveloping algebras of operators." I do not pretend to understand this calculation, or even its result, but the next sentence seems to give the game away: "... The data... give for [the] ratio... a dependence on θ_{cm} which is nicely in agreement with the assumed commutativity restrictions for the hadronic units. The fit [to] the data is then reduced to a mere selection of the best function of θ_{cm} that achieves the desired fit." Some grammatical features of these remarks defeat me, but my best guess is that what the author means is that any function whatsoever which one makes up is automatically the prediction of his theory! This is indeed a remarkable theory.

I do not think the present state of the work as reported is in a condition which merits publication in Phys. Rev. Letters. As a stylistic note, the manuscript is written in broken english which adds greatly to the difficulty of understanding what the author is trying to do. Finally, I note that all references in the manuscript are dominated by a publication known as the "Hadronic Journal," which is unknown to me.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 26, 1982

To the Editors of
THE PHYSICAL REVIEW LETTERS
1 Research Road
RIDGE, New York 11961

RE: "Use of the Hadronic mechanics for the ....."
BY: R. M. Santilli
NO: LR2111

Dear Colleagues,

I acknowledge receipt of the rejection of my paper jointly with a copy of one referee report. The desired referee appears to have a rather complete lack of knowledge of the experimental, theoretical, and mathematical studies underlying the paper. I am therefore respectfully asking that you ignore this report, and select two new referees according to the following qualifications:

(a) the referee should have an in depth knowledge of the indicated studies underlying the paper, as quoted in the references, e.g., proceedings of FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE–ADMISSIBLE TREATMENT, held in France on January, 1982 (copies of all references are available on request);

(b) in case of rejection by these experts, the report should identify technical errors, while expressions of personal feelings should be avoided as much as possible; and,

(c) for reasons communicated separately to your Editor in Chief, Dr. David Lazarus, the referee SHOULD NOT be selected from Harvard University, the Massachusetts Institute of Technology, and other local colleges.

The paper is therefore returned to you enclosed. Since the report mailed to me is purely qualitative, I provide below only qualitative comments. I remain, of course, at your disposal, for additional technical comments.

AN HISTORICAL ASPECT. P. A. M. Dirac made it quite clear in his limpid writings that he expected the violation of both the space and time reflection symmetries. In fact, in his paper Rev. Mod. Phys. **21**, 392 (1949), p. 393, he states

> "I do not believe there is any need for physical laws to be invariant under these reflections".

Scholars in relatativity can see in this statement one of the best manifestations of Einstein's teaching. In fact, we learn the equivalent role of space and time coordinates beginning from undergraduate courses in special relativity.

Apparently, the referee ignores completely this historical aspect. WHY?

A STATISTICAL ASPECT. The irreversibility of the macroscopic physical reality is established by incontrovertible experimental evidence, while the reversibility of particle physics is a mere conjecture at this time. The problem of the reconciliation of these two contrasting situations has remained unresolved since the time of its identification in the early part of this century.

Any researcher or referee who has done a minimal but serious study of this problem, knows that such a reconciliation is virtually impossible on true technical grounds. For instance, to achieve credibility, the supporter of a reversible particle mechanics must prove that the experimentally established noncanonical character of the time evolution of Newtonian systems can be reduced to a large collection of unitary transformations of the particle constituents. I am, of course, not referring to academic systems of perpetual—motion type. Instead, I am referring to the systems of the real world, e.g., those that are of non—Hamiltonian type because of drag and follower forces, as dayly encountered by engineers.

The most natural resolution of this historical problem is the recognition of a small violation of the time—reversal symmetry in particle physics, beginning with short range nuclear interactions. The experiment by Slobodrian, et al, is a clear indication of the possibility of a future final resolution of the problem along its most natural lines.

Apparently, the referee opposes even the continuation of research for the future resolution of this historical problem. WHY?

AN EXPERIMENTAL ASPECT. All experimenters I have personally contacted, besides those of ref. 1, have unanimously indicated their expectation that the time—reversal symmetry is violated in strong interactions. In their view, the only open aspect is the AMOUNT of the violation. The continuation of experimental efforts is therefore vital for the resolution of the issue.

Apparently, the referee opposes the conduction of new experiments. WHY?

A SOCIOLOGICAL ASPECT. As we all know well, one of the most important sociological aspects of contemporary research in nuclear physics is the expectation of contributions valid for NEW forms of energy, particularly for the hopes to achieve controlled fusion. In this latter respect, the problem of the reversible or irreversible character of nuclear interactions acquires a rather substantial dimension, not only of scientific—technological nature, but also of administrative—financial character.

This is well known to experts in the field. For the sake of this letter, it is sufficient to note that, say, a deviation in the time—symmetry of the order of 10^{-3} [which is more than compatible with the measures by Hardekopf, et al] could imply a rather

significant effect for sufficient fluxes of nucleons. In turn, this could have sizable implications in the very design of attempts at the controlled fusion.

In short, rather immense human and financial resources are currently spent by several Countries in attempting the controlles fusion. Scientific accountability in the use of public funds demands that fundamental physical issues of the type addressed by Slobodrian, et al, be resolved in the most exhaustive possible way.

Yet, the referee says that this serious experimental study is not needed. WHY?

A FIRST THEORETICAL ASPECT. The referee essentially claims something to the effect that the special relativity should imply only one form of interacting Lagrangians. Since this is not the case, he would therefore conclude by saying that the special relativity is a "remarkable theory". In fact, he uses exactly the same reasoning, although applied to the fact that the rudimentary model of the paper does not predict an explicit dependence on θ_{cm}.

We are all aware that to achieve one given interacting Lagrangian we need considerably more ingredients than Lorentz covariance. Yet, the referee desires a different criterium for the theory of the paper. WHY?

A SECOND THEORETICAL ASPECT. We all know equally well that reflection operators depend explicitly on the rotational symmetry. In particular, the exact T–symmetry implies the exact spherical symmetry of the charge distribution of protons and neutrons in the conditions of the experiment by Slobodrian, et al.

We are all aware that the possibility of a perfectly spherical symmetry of the charge distribution of nucleons under impact with nuclei is quite remote. Yet, the referee tacitly implies the validity of this absolutely rigid charge distribution. WHY?

A THIRD THEORETICAL ASPECT. The current efforts to construct the hadronic mechanics are essentially oriented toward the representation of nucleons whose spherical symmetry admit small deformations. This is technically realized with generalizations of the enveloping associative algebra into isotopic or genotopic forms, that is, with a generalization of Lie's theory at the level of the envelope (and thus, of the Lie algenras and groups). By no means, these efforts are intended to be the only possible way of reaching a dynamics which is intrinsically irreversible, and numerous other ways are conceivable.

The promotion of theoretical studies of different orientation on the problem of particle irreversibility is clearly essential to achieve maturity of experimental finalization, even for the case of the reversibility. Yet, the referee appears to oppose these theoretical studies. WHY?

A FEW ADDITIONAL REMARKS. The following aspects of the report deserve a comment.

(1) My English is admittedly broken. In fact, I never had the time to sit in an English class. Yet, my English has been fully sufficient to communicate with colleagues willing to communicate. Besides, your Journal has some of the best staff in the English language.

(2) The following statement in the report is erroneous

"A repetition of the $^{9}Be(^{3}He,p)$ and inverse reaction measurement by Hardekopf, et al ....."

In fact, these experimentalists measured only the polarization of the direct reaction, and assumed the measures by Slobodrian, et al, for the inverse reaction, as clearly stated in their paper. In the final analysis, this is only one (out of several) reasons calling for additional experiments. Actually, errors such as this one by the referee constitute one of the motivations whereby the publication of the paper by Hardekopf, et al, was done excessively soon on a comparative basis with the long consideration process of the paper by Slobodrian, et al, as reported in detailed to Dr. Lazarus.

At any rate, the point confirms beyond any reasonable doubt that the referee does not possess sufficient technical knowledge of the topic.

(3) As a referee of your Journal, when I receive a paper listing a Journal unknown to me, it is my ethical duty to study the relevant papers of that Journal BEFORE passing judgment. If, for any reason, I do not have the time to do that, I simply disclose it to you, and ABSTAIN from passing judgment. This referee admits explicitly that he does not know the Hadronic Journal. He also admits explicitly that he does not know the studies underlying the paper (*). YET HE EQUALLY PASSES JUDGMENT. WHY? Most paradoxically, I submitted the paper with a selection of at least some of the most relevant papers in the Hadronic Journal, precisely to prevent this claim. EVEN WITH THE READY AVAILABILITY OF PAPERS, THIS REFEREE HAS CLAIMED LACK OF KNOWLEDGE JOINTLY WITH THE PASSING OF JUDGMENT. WHY?

For these and other reasons indicated separately to Dr. Lazarus, I beg you:

I : to ignore the report of this referee;

II : to avoid the use of this referee in future editorial processings at your journal; and

III: to implement an equitable scientific process via the selection of two experts in the field of the proposal, as specified above.

In particular, please keep in mind that, if my paper is rejected because of technical errors identified by the referees, not only you can count on my graceful acceptance, but you and the referees will have my sincere gratitude.

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
cc: Dr. D. Lazarus, APS;

(*) I refer here not to my papers, but instead to papers by distinguished mathematicians, theoreticians and experimentalists we can identify in the references considered.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 28, 1982

RE: "Use of the hadronic mechanics for ...."
BY: R.M.Santilli
RE: LR2111

Dr. G. TRIGG
Editor
The Physical Review Letters
Ridge, New York

Dear Dr. Trigg,

I would appreciate the courtesy of replacing the NOTE ADDED IN PROOF of my paper with the enclosed one, which is the result of rather considerable consultations with colleagues in the USA and abroad. The version in your possession may be misleading because it does not indicate explicitly that the value 0.0 must be referred to the DIFFERENCE P–A (polarization less analyzing power), and not to each individual one of these quantities, for the exact T–symmetry.

Needless to say, the paper may contain additional imperfections of this type. You can therefore count on my best possible collaboration for technical improvements of this type suggested by qualified referees.

Your assistance in this submission is appreciated.

Very Truly Yours,

Ruggero M. Santilli

RMS-mlw

encls.

NOTE ADDED IN PROOF. Upon completion of this work, R. MIGNANI (Univ. Rome, Italy) informed me of the appearance of the rapid communication by R. A. HARDEKOPF, P.W.KEATON,P.W.LISOWSKI, and L.R.VEESER, Phys. Rev. Letters C25, 1090 (1982). Contrary to the statement by these authors, their experiment is still inconclusive for several reasons. In fact, their only four measurements can be fit by several curves, including a possible central peak (not considered in the communication). Also, they measured only the polarization of the direct reaction and relied upon the measures by Slobodrian et al. on the analyzing power of the inverse reaction. These data do not appear to give the value 0.0 for the difference :(polarization less analyzing power), as needed for the exact time reversal symmetry. As a result, the only aspect that the measures by Hardekopf et al may leave open is the AMOUNT OF VIOLATION.

REVISED VERSION DATED MAY 28, 1982

of the Note Added in Proof

of the paper

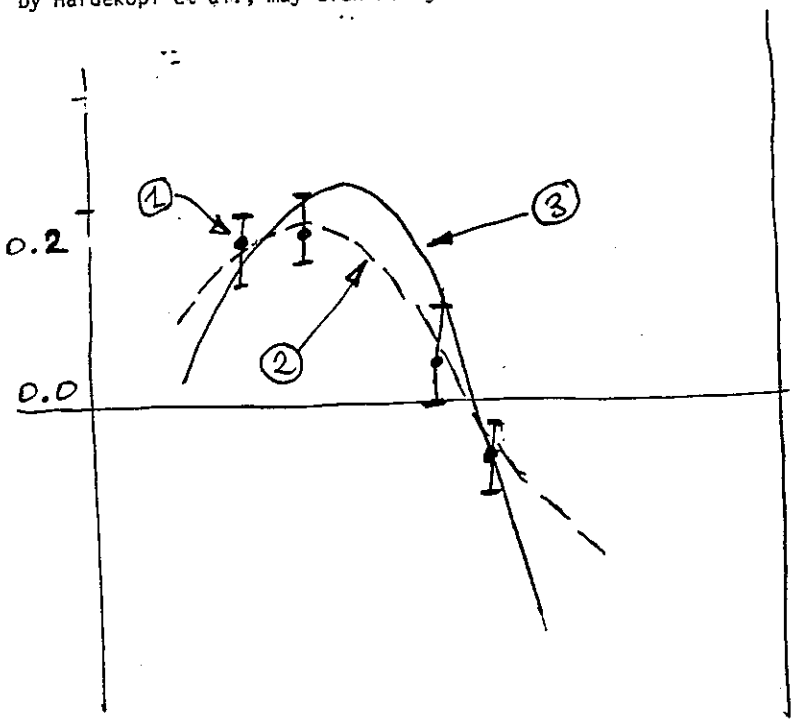
"Use of the Hadronic Mechanics for the ...."

by R.M.Santilli

ref. (Phys. Rev. Letters) LR2111

QUALITATIVE ELABORATION OF THE NOTE ADDED IN PROOF OF THE PAPER
"Use of the hadronic mechanics for ...."
by R.M.Santilli
submitted to Phys. Rev. Letters
PRL ref. no LR2111

ASSUMPTION: That the four measures by Hardekopf et al. do indeed yield a null difference (P- A) at those points.
ARGUMENT: this is not sufficient to establish an exact T-symmetry (i.e., P - A = 0) because the four measures can accomodate a family of curves, all implying a non-null difference P - A.
CALCULATIONS: The statistical probability that the four measures by Hardekopf et al imply exactly the same curve as that of the analyzing power of the inverse reaction is quite small and, depending on the (unknown) error of the four measures by Hardekopf et al., may even be ignorable



NOTES:
(1) ⊥●⊥ indicate the four measures by Hardekopf et al. and their error for the polarization of the reaction $^{9}Be(^{3}He,p)^{11}B$.
(2) — — ● indicates the curve of the (over fourteen) measures by Slobodrian, Conzett, et al. on the analyzing power of the inverse reaction $^{11}B(p,^{3}He)^{9}Be$
(3) ——— indicates one of the infinite number of curves admitted by measures as per note (1) ALL different than the curve as per note (2).

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES
1 RESEARCH ROAD BOX 1000 RIDGE, NEW YORK 11961
Telephone (516) 924-5533

2 July 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Re: Use of the hadronic mechanics for the best fit of the time-asymmetry...
By: Ruggero Maria Santilli

LR2111

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

George L. Trigg
Editor
Physical Review Letters

enc.

P.S.: The referee was chosen from your list of experts.

G.L.T.

NOTE BY RMS: THE REFEREE IS EXPECTED TO BE S. OKUBO.

R2L

MS#023550
Use of the hadronic mechanics for ...."
R. M. Santilli

1) The idea is new but quite unorthodox with many untested hypothesis. The theory contains two arbitrary time-reversal violating interactions associated with two arbitrary operators $\not{T}$ and $T^{\not{D}}$ if I understand correctly. Thus it has little quantitative predictive power.

2) A large time-reversal violation in the strong interaction will cause many problems in conjunction with the presence of weak interactions. For example, consider angular correlations between polarization axis and momentum of, say, weak decays of polarized nucleus or polarized Λ-particle in $\Lambda \to p\pi$. If a large time-reversal really exists, the effect should already have been observed. Usually, this fact is quoted to imply its absence by a ratio of 10^{-3} to one. We note that a small effect of similar nature is known to exist in $K_L^o \to \nu\mu\pi^+$ decay. A far more serious problem is the absence of the electric dipole moment of the neutron. Many theories have been simply abandoned because of this fact alone. The author should show that his theory will be consistent with these experimental facts in spite of a large violation of the time-reversal.

3) A large time-reversal violation would, I believe, contradict with the currently accepted cosmology. Although this fact should not be counted against it, it will weaken the philosophy of the paper. Note that the popularity of the grand-unified-theory is partly due to its consistency with cosmology.

4) In conclusion, I cannot say that this paper satisfies the urgency criteria for publication in Phys. Rev. Lett. However, if questions raised here are satisfactorily resolved in a future revised version, then it may be accpetable for publication in Phys. Rev.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

July 21, 1982

Dr. GEORGE L. TRIGG, Editor
The Physical Review Letters
1 Research Rd
RIDGE, New York 11961

RE: "Use of the hadronic mechanics for the best fit of the time–asymmetry recently measured by Slobodrian, Conzett, et al."
BY R.M.Santilli; PRL ref. No. LR2111/L–1

Dear Dr. Trigg,

I would like to express my sincere gratitude for the quite valuable referee's report you mailed me on July 2, 1982. I believe that all the comments by the referee are scientifically sound and critically constructive. I have therefore provided a sincere effort to comply with the referee's suggestions by rewriting the paper entirely [except the calculations and formulae which have been only controlled again].

However, some of the referee's comments call for a critical assessement of current theories (unified gauge theories and cosmology in particular) that does not seems recommendable to conduct in a paper on time–asymmetry. My asnwer is therefore consisting of:

- the enclosed revised version which, as you can see, is as smooth as scientifically possible in the sense that particular care has been provided to avoid possible conflicts with readers of potentially different views, as well as to avoid any comment on existing theories; also, the revised version closes with the indication of possible contributions of the hadronic mechanics, and underlying time–asymmetry, to quark theories which I believe can be potentially relevant; and
- this letter in which I take the liberty of indicating aspects that are not recommendable for consideration in the paper.

EXPERIMENTAL SITUATION. I was in the South when the Los Alamos experiment on time–asymmetry was conceived; I have been in touch a number of times with the Québec–Berkeley and the Los Alamos experimenters; and I have consulted with a number of experimentalists here and abroad.I believe that the Québec–Berkeley experiment is correct as published in PRL. At any rate, the experimentalists have recently repeated the measures via a carbon polarimeter, by confirming the original measures. I hope that their confirmation will soon appear in press. This is the experimental situation in nuclear physics (see below for other fields) independently from any theoretical consideration, e.g., the need to achieve a true compatibility of the particle description with the irreversibility of the classical real world.

WEAK INTERACTIONS. An important point of the referee's report is the sound and predictable need to avoid conflicts with unified gauge theories of weak and electromagnetic interactions. I hope that the revised version has answered this question, by indicating the reasons why the time–asymmetry measured by Slobodrian, Conzett et al. is expected to be fully compatible with gauge theories. In fact, the origin of the time–asymmetry can be identified theoretically and experimentally in the deformation of the charge distribution of hadrons under impact and penetration within those of other hadrons. The time–asymmetry measured by the Québec–Berkeley collaboration occurs for nuclear reactions involving the exhange of two nucleons. In the transition to the leptonic decays of hadrons, such a time–asymmetry is expected to descrease substantially, assuming that a deformation of the charge distribution makes sense for the case of point–like leptons. Also, as stressed by the experimenters, scattering amplitudes do not appear to be sufficiently sensitive to the time–asymmetry. Thus, for any comparison to have sense, the data for the

leptonic decays should be re—formulated for the polarization/analyzing power cases, assuming that it is possible for the decays considered.

It appears that a considerable segment of the physics community is under the expectation that the amount of time—asymmetry measured by the Québec—Berkeley group is a sort of new "strong constant", in the sense that should occur for all strong interactions, by therefore resulting into a direct conflict with unified theories.

The paper submitted will have achieved one of its primary objectives if it succeeds in indicating the erroneous character of this belief. In fact, a difference in time—asymmetry is already measured in the two different reactions studied by the Québec—Berkeley group.

An aspect which has been omitted from the paper is the indication of the recent problematic aspects of gauge theories in regard to their prediction of the heavy bosons. As you know, these predictions have not been confirmed at DESY, and a reshuffling is under way at CERN. The affair has been termed "embarassing" in a recent note in Science here enclosed.

ELECTRIC DIPOLE MOMENT OF NEUTRON. This is another fully sound comment by the referee. However, the null value of the moment has not been touched because the paper is not intended to present a structure theory of nucleons. At any rate, we should not forget that a structure theory of the neutron which is capable of representing the null value of the dipole moment in a form acceptable by the scientific community at large, is still lacking at this moment. In fact, quark theories do not appear to have an explicitly computed, identically null probability of tunnell effect for free quarks [besides other requirements] to provide a conclusive solution of the problem.

COSMOLOGY. Again, the referee is correct in indicating the relationship between the popularity of a theory and its alignment with contemporary views in cosmology. I am also happy to see that the lack of apparent agreement of the Québec—Berkeley time—asymmetry with cosmology is not recommended as a serious drawback by the referee. In fact, no cosmology should be taken seriously unless it is capable of representing in full [actually, it is based on] the irreversibility of the real world. This basic requirement does not appear to be satisfied by current theories in cosmology, as one can see from the fact that the PPN approximation is essentially reversible in dynamic al contents, or from other facts. But this is only one of the major problematic aspects of cosmology today. We should not forget that at time zero the universe was the biggest possible black hole. Unless the explosion of a black hole is proved to be possible, contemporary cosmology cannot explain the birth of the universe in any credible way. Also, the basic equations are incompatible with electromagnetism, as one can see in Ann. Phys. 83, 108 (1974). In fact, for a massive body with zero total electromagnetic data, the equations for the exterior problem predict zero source, i.e., are given by $G_{\mu\nu} = 0$. But, matter has a charge structure. Whether in flat or curved space, classical electromagnetism predicts a non—null electromagnetic tensor $T_{\mu\nu}$ for moving charges with null total data of charge, electric and magnetic dipole moments, and radiations , unless all the charges are at rest and at very small mutual distances. The equations should tehrefore be $G_{\mu\nu} = cT_{\mu\nu}$ for the case considered.The situation appears to be clear-cut, in the sense that, either one accepts the basic equations of contemporary theories in gravitations, in which case electromagnetism must be abandoned and reconstructed, or one accepts electromagnetism, in which case the field equations for gravitations must be reviewed from their foundations. Additional serious problems have been raised through the years by Yilmaz [who has an intriguing theory apparently capable of at least reaching compatibility with electromagnetism]. For these and other reasons, the aspect of cosmology has been completely ignored in the paper.

CONJECTURAL CHARACTER OF HADRONIC MECHANICS. This is a further point of the referee's report which is quite valuable. In the revised version I have therefore taken all the necessary precaution to stress more clearly the conjectural character of the new mechanics. Never-

theless, the agreements of the predictions of the theory with experimental data in nuclear physics should not be ignored. I am referring here to the several contributions by Eder [e.g., in representing nuclear magnetic moments]; the prediction of the deformation of the charge distribution of extended nucleons [of about 1%] and its agreement with the measures by Rauch et al; and, last but not least, the agreement with the Québec–Berkeley measures on time–asymmetry which is simply impossible via the ordinary QM, to our best knowledge at this time.

PREDICTIVE POWER OF THE THEORY. There is no doubt that the referee is correct in indicating that the rudimentary model of the paper has limited predictive power. However, we should keep in mind that the time–asymmetry [as well as the space–asymmetry and the rotational–asymmetry] vary from reaction to reaction. Thus, particular precautions have been taken in the structure of the new mechanics to AVOID single, fixed, predictions.

On more explicit terms, QM is based on the operator $H = T + V$ where T is fixed, and V is an "arbitrary" (in the language of the referee) potential needed to represent a sufficiently broad class of potential forces. The hadronic mechanics preserves H, and adds generalized forward and backward units $I^* = I + Q$, where I is the unit of QM and the operator Q is "arbitrary" to represent a sufficiently large variety of NON-potential forces. The identification of V calls for experimental informations on the nature of the action-at-a-distance. The identification of Q calls for additional experimental informations on the charge radius, density of hadronic matter, etc. As a result, the time–asymmetry is capable of varying from one reaction to the other, up to the point of being null ($Q = 0$) for point–like structures.

SUITABLE JOURNAL FOR PUBLICATION. I believe that the topic presented in the paper is best suited for a letter, and for this reason it has been submitted to you. You can count on my best possible understanding in case you recommend otherwise. However, please keep in mind my considerable uneasiness in turning the paper into a full length version. This is due to the fact that the basic ideas of the hadronic mechanics have by now appeared in print several times, and I see no reason to review them again at this time.

THE NEED TO PURSUE NOVEL ADVANCES. I am in full agreement with the general spirit of the referee report that due consideration and respect should be provide for existing theories receiving the majority of consensus. For this reason I have avoided any criticism of current views in the enclosed paper.

However, I believe that, jointly, we must pursue novel advancements via the traditional scientific process of trial and error, as I am confident the referee will agree. Lacking this process, we risk the transformation of physics into a religious preservation of old dogmas over a large financial platform.

In the particular case of the time–asymmetry, I believe that truly relevant advances along established trends are possible, such as a realistic possibility of achieving "strict confinement" of quarks and other possible contributions indicated in the concluding part of the paper. As a result, the acceptance of the experimental results on time–asymmetry, and the theoretical study of its representation, rather than being in conflict with existing trends, constitute the foundations for the possible solution of some of their problems.

Very Truly Yours

Ruggero Maria Santilli
RMS-miw ; encls:
1. Outline of possible applications of the hadronic mechanics to quark theories;
2. Diagram indicating the possible accomodation of curves with $P \neq 0$ in the Los Alamos measures;
3. Note recently appeared in Science in regard to the situation for heavy bosons.

POSSIBLE APPLICATIONS OF THE HADRONIC MECHANICS TO QUARK MODELS, QCD, AND ALL THAT.

Nontechnical lines prepared by the staff of
The Institute for Basic Research

We assume the reader is familiar with:

(1) The existence, at the mathematical level, of a Lie—isotopic and of a Lie—admissible generalization of Lie's theory;

(2) The existence of a Birkhoffian generalization of (classical) Hamiltonian mechanics as a realization of the generalized Lie theory via functions on T*M; and

(3) The current efforts to build a "hadronic mechanics" as a realization of the generalized Lie theory via operators on (a suitable formulation of) a Hilbert space. The hadronic mechanics is being constructed as a generalization of quantum mechanics for extended hadrons under joint action—at—a—distance/Hamiltonian and contact/non—Hamiltonian interactions, in such a way to admit the Birkhoffian (rather than the Hamiltonian) mechanics as classical image.

The state of the art in the studies by mathematicians, theoreticians, and experimentalists for the construction of the hadronic mechanics is reported in the *Proceedings of the First International Conference on Nonpotential Interactions and their Lie—admissible Treatment*, Hadronic J. Vol. 5, numbers 2, 3, 4, and 5.

There are growing indications that the new mechanics can provide significant contributions in a number of essentially open problems of quark theories and related fields. No active research has been conducted to date in the topic. These few lines are intended to indicate some of these possibilities on a confidential basis.

(1) **Possible alternative to spontaneous symmetry breaking.** A basic idea of the hadronic mechanics is that of representing the extended character of hadrons via an isotopic generalization of the Hilbert space. Under such isotopy, conventional symmetries (those expressed via unitary operators) are generally broken. There are indications that this approach can be a valuable alternative to spontaneous and other treatments of symmetry breakings. A novelty of the approach is the achievement of the breaking without predicting new particles, evidently, because of the realization of the breaking without "action—at—a—distance" forces. This line of study has been proposed by S. K. Yun (IBR and Saginaw Valley State College).

(2) **Possible construction of quarks as clusters of more elementary particles.** The isotopy of the Hilbert space of a conventional QM particle implies the possibility of altering its intrinsic characteristics such as charge, spin, parity, etc. Therefore, it appears that the hadronic mechanics could "build" a quark within hadronic matter, in the sense that a cluster of particles obeying the hadronic mechanics could reach all the desired intrinsic characteristics for quarks. This possibility was formulated by R. M. SANTILLI (IBR) in 1978 and has remained unexplored since that time.

(3) **Possible contribution to the open problem of quark confinement.** The available efforts to reach quark confinement are essentially based on the assumption that the same mechanics holds in the exterior and in the interior of hadrons. The hadronic mechanics recovers the conventional QM for the exterior treatment of a hadron (motion of its center of mass under long range interactions), while it postulates a generalized mechanics for the interior problem. This basic idea appears to be naturally set for a valuable contribution to confinement. In fact, particles obeying the generalized mechanics can occur only under short range, contact, non—Hamiltonian interactions. Whenever these interactions are absent, and the conventional physical conditions of contemporary detection are recovered, particles obeying the hadronic mechanics cannot exist, and must decompose into conventional particles. This idea was suggested by R. M. SANTILLI in 1979, and has also remained unexplored until now, pending the availability of more detailed formulations of the new mechanics.

(4) **Nonrelativistic equations of structure for light quarks.** As is well known, Schrödinger—type equations are currently available for quarks, provided that at least one of the quarks is heavy. For light quarks (e.g., as expected for pions), conventional nonrelativistic Schrödinger—type equations generally yield complex values of the total energy. Apparently, this difficulty can be by—passed by the isotopy of the eigenvalue equations, as it has been rudimentarily illustrated via the use of the Hulten potential by R. M. SANTILLI. As a result, it appears that the hadronic mechanics could provide new possibilities of achieving physically consistent structure equations for light mesons.

(5) **Miscellaneous applications.** If one acknowledges the possibility that the basic physical structure of contemporary quark theories is an excellent, but only approximate characterization of nature, and that a finer physical world exists within a hadron, an array of additional possibilities occur for contributions in numerous (if not all) aspects of quark theories, including possible adjustment of jet theories to experimental data, refinements of the predictions based on gluons, etc.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES
1 RESEARCH ROAD BOX 1000 RIDGE, NEW YORK 11961
Telephone (516) 924-5533

3 September 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Re: Use of the hadronic mechanics for the best fit of the time-asymmetry...
By: Ruggero Maria Santilli

LR2111

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

for George Barton

George L. Trigg
Editor
Physical Review Letters

enc.

R2L

Third Referee's Report on

R. M. Santilli: "Use of the hadronic mechanics..."

MS# LR 2111

(1) Without passing judgment on "hadronic mechanics" as developed by numerous authors and papers published primarily in the Hadronic Journal, this paper is not a great contribution: it essentially states that a theory (hadronic mechanics) which ab initio was constructed so as to violate various generally cherished conservation laws including time reversal invariance indeed can account for such a violation. He does derive the appropriate formula but the theory is much too general to allow a quantitative comparison. Nor is it the only class of theories one can construct to account for a time asymmetry. Why should one accept "hadronic mechanics" over other alternatives?

(2) It is not sufficient to argue qualitatively that the violation of time reversal invariance depends on the process. Does this class of theories permit sufficient freedom to allow quantitative (order of magnitude) compatibility between the large violation (if it exists) of reference 1 and the very small violation (if it exists) of the absence of the neutron electric dipole moment? His paper does nothing to support "hadronic mechanics" from the theoretical side. All the support (in the context of time asymmetry) stands and falls with that experiment.

(3) I don't see how the words "for the best fit" in the title are borne out by the paper. The only issue is whether or not there is an effect and the

experiment is not unequivocal.

As I see it, the only point made by this paper is that "[illegible] mechanics" can indeed account for time asymmetry. Since it is a very u[illegible]ox theory this is hardly much of a recommendation.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

September 9, 1982

Dr. GEORGE L. TRIGG, Editor
The Physical Review Letters
1 Research Road
RIDGE, New York, 11961

RE: note no. LR2111/L–1
"Use of the hadronic mechanics for ...."
by R.M.Santilli

Dear Dr. Trigg,

I acknowledge receipt of your letter of September 3 returning the paper with the referee's comments. I accept several of them as scientifically valuable. However, I have doubts of various nature on others. I have therefore revised the manuscript accordingly, and I am returning it to you enclosed. In particular, the revisions over the preceding version are the following.

[1] The term "best" in the title has been eliminated because inessential and questionable, as correctly indicated by the referee;

[2] The referee is correct in indicating that other interpretations of the time–asymmetry are possible. I have therefore added a sentence in the first concluding remarks of page 5, to the effect of indicating this expectation as well as soliciting their study. However, I felt obliged to indicate that, particularly the interpretations based on additive terms in the Hamiltonian, must prove their compatibility with the established irreversibility of the macroscopic world. This point is clearly important for physicists interested in a distinction between the pursue of knowledge and that of academic interests.

[3] The referee is also correct in indicating that a quantitative study of the compatibility of the time–asymmetry suggested by experiments[1] and gauge theory is much in order. This study is an important objective of our institute, and it is already under way. I have therefore indicated the appearance of a forthcoming paper in the topic in the fourth paragraph of page 5. I disagree firmly on the need to present the results jointly with the paper submitted. In fact, this paper deals with certain specific nuclear reactions involving the exchange of two nucleons, while the topic under consideration deals with leptonic decays of hadrons. The distinction between these two physical arenas is self–evident, and equally self–evident is the need to treat the two aspects separately.

[4] I have added a sentence to the footnote of page 6 to the effect of indicating that the four measures of the Los Alamos group are insufficient to establish the exact time–symmetry, as indicated in the enclosed diagram previously mailed to you (and not intended for publication because trivial). In the meantime I have visited Slobodrian in Quebec and personally inspected his measures and experimental setting. I have also conducted additional travel and research, all leading to doubts on the [rather fast] Los Alamos work.

[5] I have finally made three linguistic, minor changes (eliminated "back " after Dirac in page 1, and the like.

On the following technical points I disagree with the referee.

[a] The referee appears to be convinced that the variation of the time– and space–asymmetries from reaction to reaction is a mere personal belief. This is not the case. We are all in agreement on the violation of the P–symmetry, as established by experiment[5] and several others. I urge the referee to inspect again these papers and convince himself of the clear experimental evidence indicating the variation of the space–asymmetry from case to case. For the time–asymmetry we only have the two different reactions studied in ref.[1] . The amount of the deviations is clearly open at this moment, as we all agree. However, the fact that the violation changes from reaction to reaction is

incontrovertible, as established by the inversion of the convexity of the polarization curve from one reaction to the other.

[b] The referee appears to be still convinced of the need to reach a formulation of the hadronic mechanics capable of achieving one single prediction of time–asymmetry. I disagree because this would be contrary to experimental evidence. At any rate, it would be equivalent to pretending that the special relativity predicts one single interaction Lagrangian term. This is not the case [if it were, the relativity would have been rapidily abandoned half a century ago]. I still do not understand why the referee then has a double standard of scientific evaluation, that is, he accepts the special relativity even though the theory does not predict one single fixed interaction, while he rejects the hadronic mechanics because its available formulations is too broad.

[c] The referee appears to have genuine doubts on the validity of experiments[1], which are perfectly legitimate. However, he appears to have a much more permissive attitude for other aspects. For instance, has this referee rejected papers on gauge theories because the theory predicts a finite nonnull probability of production of free quarks, along the explicit statement to this effect by Nambu at the Einstein Centennian Celebrations (and as one can verify by himself via explicit calculations when a conventional space–time and a conventional mechanics is used)? –incidentally, I favor the publications of papers on quarks despite these open problems, because the opposite view would imply the halting of the scientific process of trial and error. But then the same standard must be used for the open problem of the time–asymmetry as well as that of the hadronic mechanics.

But most of all, I appeal to the referee for what is at stake here. It is true that my note does not constitute a great contribution, particularly when compared to other contributions by other authors to the construction of the hadronic mechanics [particularly those by H.C.Myung]. However, what is at stake here is whether human knowledge should be maintained at the level of the physical laws discovered long ago, or the scientific pursue of genuine advances should be permitted.

By ignoring all the aspects identified in preceding letters, and ranging from the need to achieve a true compatibility with macroscopic reality, to several others, the sole implications for controlled fusion are such to warrant a differentiated study of the issue, that is, that with hadronic mechanics and that with the atomic mechanics. Rather than preventing the appearance of some of them, the study of all the possibilities should be promoted. The future, rather than any of us, will tell which is the best way to go.

It is therefore hoped that, with the further revisions submitted, the paper will finally meet with the referee approval.

Very Truly Yours

Ruggero Maria Santilli
RMS–mlw
encls.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

September 28, 1982

Dr. GEORGE L. TRIGG
Editor
PHYSICAL REVIEW LETTERS
1 Research Rd
RIDGE, N.Y. 11961

RE: paper # LR2111/L-1, "Use of hadronic mechanics for the fit ...."

Dear Dr. Trigg,

I am rushing copy of a recent paper by the experimental group in Québec which, as you can see, CONFIRMS the existence of the time-asymmetry in the specific nuclear reactions considered. This paper has been submitted for a rapid form of publication. It shows rather clearly that my letter LR2111/1-1 is timely because dealing with a basic, open physical issue.

I would appreciate the courtesy of forwarding one copy of the paper and of this letter to each of the two referees of my paper LR2111/L-1. I hope they will acknowledge that the possibility that time-symmetry is truly violated in the cases considered is quite real, and definitely such to warrant an open scientific debate for the community at large.

A new element has emerged recently at the IBR. It concerns the possibility that the time-reversal symmetry can be restored to an exact form for the case of the experiment by Slobodrian, Conzett, et al, provided that the symmetry is expressed for extended particles. In different terms, the experiments indicate possible violation for the symmetry of contemporary use, THAT FOR POINT-LIKE APPROXIMATIONS OF PARTICLES. If the formulation of the symmetry is done instead by taking into account corrections due to the extended character of nucleons under short range interactions, then the exact character of the symmetry can apparently be restored. The proof for the model of paper LR2111/L-1 is trivial. In fact, the generalized units $I^{\triangleright}$ and ${}^{\triangleleft}I$ are (scalar) multiples of the atomic unit I. The isotopic-antiunitary time-reversal operators $\tau^{\triangleright}$ and ${}^{\triangleleft}\tau$ therefore leave invariant the equations of motion, by therefore being symmetry in the conventional sense. The P-A data are then nothing but a measure of the implications of the extended character of nucleons. To put it differently, the insufficiency does not appear to be in the notion of symmetry, but instead in the simplistic, atomic, definition of symmetries for point-like abstractions, when we have in reality extended charge distributions at distances smaller than their size.

I would appreciate advice whether the current version of paper LR2111/L-1 should be modified to include these latter results.

Very Truly Yours

Ruggero Maria Santilli
RMS-mlw
cc. Dr. Lazarus

CONFIDENTIAL

September 9, 1982

Dear Professor Okubo,

I am taking the liberty of contacting you for advice on a rather delicate situation that, unless kept under control, is risking to degenerate to the detriment of our community of basic research at large.

It concerns a rather strong opposition by referees of the Physical Review Letters to publish my note on the use of the hadronic mechanics for the fit of the time asymmetry by Slobodrian, Conzett, et al.

If considered per se, as an individual case, the rejection of the note is ignorable. In fact, I had in the past several papers rejected and all rejections were accepted with the best possible grace.

The case of my note, however, is fundamentally different this time, and may trigger events that could rapidly become outside the control of all of us. This is due to the fact that this rejection arrives following a chain of too numerous and too questionable [on ethical grounds] occurrences. Thus, it could be the typical drop which overflows the glass.

It would be impossible for me to give you an idea of the episodes I am referring to because it would take a book [perhaps one day I will write my memoirs and I will document them otherwise nobody would believe to the dimension of academic greed]. I therefore simply limit myself to send you a copy of my letter to Dr. Lazarus and of its enclosures. In this way you can see a small part of the facts I am referring to.

Another point you should be aware is that a new mechanics, the Birkhoffian mechanics, has been constructed without one single paper appearing in journals of the APS. This is due to oppositions by referees which can be only interpreted as essentially motivated by personal financial-academic-ethnic interests. Nevertheless the episode is very grave and well known to educated observers.

There is now a great fear that the episode will repeat itself again, owing to the total lack of control on the ethics of the refereeing process. I am referring here to the construction, this time, of the hadronic mechanics again without one single paper appearing in journals of the APS because of referee's problems.

Finally, you should be aware of the number of scholars interested in the field at this time. This may give you an idea of the pressure I have to take into account recommending a public action of containement of the organized academic interests against the pursue of novel physical knowledge.

I am going to Washington on September 14-15-16 for several reasons, but also to discuss this grave situation with qualified observers. One of the topics of the agenda is whether and when to pass to a public disclosure of the situation. This in turn may imply a disclosure of all the past, documented, episodes, and it would be a disaster for all, with international repercussions. A crisis of this nature must be avoided at any costs.

In case you considers it appropriate, I beg you to advice me on the appropriate action to undertake. For instance, should I continue to improve the paper and to

resubmit it until approved? or should I simply withdraw the paper and submit it to a journal other than those of the APS?

Each case is very risky. The first is strongly opposed by several observers because of the determination to prevent a repetition of the episodes underlying the publication of the paper by Slobodrian, Conzett, et al [it took 1 ½ years to publish this experimental paper which should have been published immediately, and then criticized in separate papers by other experimentalists, as the SOLE way to have a genuine freedom in the pursue of novel human knowledge].

The second alternative is favored by myself for the simple reason that I do not want to waste my time in academic dances [after all, this is the reason why the Hadronic Journal was founded in the first place]. However, it is a very risky approach because my withdrowal may trigger the crisis I indicated earlier.

Please advice me for the best course of action. You would gain additional reasons for my sincere gratitude. However, if you decide to abstein from any advice, you can equally count on my full understanding.

Sincerely

P.S. I take the opportunity to enclose copy of a general presentation of our institute which I am confident you will like.

THE UNIVERSITY OF ROCHESTER
RIVER CAMPUS STATION
ROCHESTER, NEW YORK 14627

DEPARTMENT OF PHYSICS
AND ASTRONOMY

Sept 14, 1982

Dear Prof. Santilli:

Thank you for your honest and courageous letter. I greatly sympathize with your situation. I have had often, and has still the similar referee problem, from time to time, needless to say. In general, if my paper were twice rejected by the same journal, I follow one of the following three alternatives: I may simply drop the matter and forget it, or rewrite and keep it for suitable later use, or resubmit it to other journals when I feel that the paper is still worthwhile. Although the bad reports of referees is so painful to our ego, and so often down-right incorrect, we have to keep in mind also a impartial perspective on our-own works. Although the formulation you made may be mathematically elegant and appealing to you, others may not think so. Besides, the elegance of mathematics is not enough for physics, since we have to compare our theory with experiments. We have always to ask the question by ourselves whether any theory can explain the experiments as equally as or better than other theories? You have to concede that the conventional QCD and unified electro-weak gauge

theory based upon the quark-model can account for impressingly large experimental facts. Also, many of its predictions have been, since, experimentally confirmed. If you want to contest it, you have to show that your theory can do the same. The point I am making is that your formalism, though it may ultimately turn out to be correct, is still premature for comparison of this kind. This is the same reason why I am a bit discouraged by my works on non-associative Quantum Mechanics. Personally, I believe that the future of non-associative physics is to blend and/or modify the present frame-work of Q.C.D. But it may take many years, if it will ever be successful. Meanwhile, we have to keep a low-profile and to be modest, I am afraid.

Returning to the particular question of the time-reversal violation, the conventional and orthodox approach (although it is not the only possibility) is via the so-called Kobayashi-Maskawa theory within the electro-weak gauge theory of Salam-Weinberg-Glashow. It contains only a few parameters and is compatible with almost all time-reversal-violating phenomena (except for the recent work of Sliobodrian et al) such as absence of the electric dipole moment of the neutron, and $K_L - K_S$ mass differences etc. On the whole,

(cont'd)

the theory can account for practically all electro-weak phenomena known at present, and is regarded as a beautiful theory as such. This is the reason why many experts are dubious at any unconventional new approach such as yours on the subject. They feel that it is a waste of time. Also, this is why they doubt the correctness of the experiment by Slobodrian et al. Since the recent history of high energy physics is full of many incorrect experiments, we cannot really tolerate such an attitude. Indeed, your theory ~~may~~ may explain the experiment of Slobodrian et al (assuming it to be correct) but not the very large bodies of other atomic and electro-weak experiments which can be explained satisfactorily by the conventional theory.

May I honestly suggest to you to not make any unnecessary protest on the matter which will be simply ignored at best, or to be worse is interpreted to show how hopeless your position is? The best course seems at least to me that you simply drop the matter or submit it for other journals. The unnecessary protest, I am afraid, will bring nothing but unpleasantness to you, not to mention a fact that it may also damage your reputation.

I hope that the tone of this letter does not sound to be pontifical. If it unfortunately does so, then I beg your forgiveness. It is unintentional and is due to my insufficient command of English.

Sincerely, S. Okubo

CONFIDENTIAL-CONFIDENTIAL-CONFIDENTIAL

September 18, 1982

Dear Professor Okubo,

Please accept the sentiments of my most sincere appreciation and gratitude for your letter of September 14 I have found on my way back from Washington. I am particularly grateful for the open character of the letter which I consider essential for true scientific communication.

Needless to say, I understand and respect your view most sincerely. It is therefore only with considerable regret that I see myself forced by several circumstances to be unable to accept your recommendation to withdraw the paper and submit it to other journals. This is the result not only of a serious consideration of your proposal, but also of consultations with a number of other scientists that would be affected by the decision, as well as with concerned observers. Permit me to stress that your proposal is indeeed fully sound and I have implemented it in the past on several other cases. However, the implications underlying this paper are such to prevent a withdrawal at this time. It is a sort of "Rubicon" created by questionable events over one decade.

Permit me to indicate to you the reasons for my inability to withdraw the paper and then recommend a possible compromise. I shall express the situation as honestly as I can, with the understanding that I can be fully explicit on scientific grounds, but I cannot disclose in full all the political aspects.

THE SCIENTIFIC PROFILE. Permit me to disagree most respectfully but most firmly with your views. I believe that your remarks have no relevance at all for the paper. In fact, all your remarks are related to electroweak interactions, gauge theories and QCD, while the paper treats a fundamentally different field, that of certain nuclear interactions involving the exchange of two physical nucleons. All colleagues I have contacted fail to see how considerations on electroweak interactions can be used to reject a paper in strong nuclear interactions.

Secondly, all the theories you refer to are centrally dependent on the representation of the interactions as closed. In this case, the center-of mass trajectory must necessarily be time-reversal invariant, as stressed clearly in the paper. The model presented in the paper, on the contrary, is centrally dependent on the representation of the nuclear interactions as open (the paper studies nucleons "a" in interactions with the external nuclei "A" of the fixed target). This point alone is sufficient per se, ignoring all the others, to render inapplicable all your remarks. In fact, the violation of the time-reversal invariance CANNOT exist in your setting. This point is stressed in the paper beginning with the example of the center-of-mass trajectory of our Earth, which is strictly time-reversal invariant, and the need to reach an open interior treatment to see the irreversibility of trajectories.

The mention of the Kobayashi-Maskave theory is a confirmation of the complete inapplicability of your remarks. In fact, there is nothing wrong with this theory, even assuming that the time-asymmetry measured by Slobodrian, Conzett, et al is correct in the quantitative amount indicated by these experiments.

This compatibility is total and two-fold. First, you must pass from the open treatment of the paper via Lie-admissible birepresentations, to the corresponding isotopic Hilbert space treatment of the exterior, closed, strong problem. You will see the transition from two units, one per each direction of time (Lie-admissible algebras) to one single unit for both directions of time.

$$\underset{\text{OPEN INTERIOR}}{\left(A\,{}^{\triangleleft}T\,B - B\,T\,{}^{\triangleright}A\right)} \Longrightarrow \underset{\text{CLOSED INTERIOR}}{\left(A\,T\,B - B\,T\,A\right)}, \quad T = {}^{\triangleleft}T + T^{\triangleright} \qquad (1)$$

This implies the incontrovertible loss of time-asymmetry and the full regaining of the principle of detailed balancing. In fact, from eq. (9) of my paper you have

$$A^{\dagger}/{}^{\dagger}P = I^{\dagger}/{}^{\dagger}I \Longrightarrow A/P = T^{-1}/T^{-1} \quad (2)$$

that is, P = A for a closed treatment of the Kobayashi-Maskava theory, as known anyhow.

But this is only a first part. The paper clearly stresses the existence of an intrinsic irreversibility, that is, the compatibility exists even if you turn the Kobayashi-Maskava theory into an open formulation. The reasons are simple. The time-reversal operator depends explicitly on spin, e.g. for s = ½

$$\tau = e^{-i\pi J_2}\Big|_{\hat{A}} C \quad , \hat{A} = \text{Ass. Alg.} \quad (3)$$

Now, nucleons are extended objects. The experiments by Slobodrian, Conzett, et al (and more directly, those by Rauch) indicate that these extended charge distributions can experience deformations under sufficient impacts and contact interactions with other nucleons. Without any claim of being unique, these combinations of rotations and small deformations of extended charge distributions are represented in the hadronic mechanics via the isotopy of the associative algebras of operators in which the expansion of the exponential of (3) can be defined,

$$\tau^* = e^{-i\pi J_2}\Big|_{\hat{A}^*} C \quad , \hat{A}^* = \text{Isotopic Ass.} \quad (4)$$

where the new associative product is A*B = ATB, and T is the isotopy operator (T=1 for point-like approximations of the conventional atomic mechanics). Now the departure of T from unit is already different for the two reactions studied by Slobodrian, Conzett, et al. The compatibility with the Kobayashi-Maskava theory is then self-evident. In fact, when you pass to leptons you have experimentally established much smaller charge distributions (for the electron it is less than 10^{-19} cm vz the 10^{-13} for nucleons). Assuming that charge distributions of such a small size can be meaningfullly deformed, the amount of the deformation must necessarily be much smaller than that of nuclear reactions involving spherical objects one million time bigger. The time-asymmetry, assuming that it can be meaningfully defined for the particles considered, is then ignorable for contemporary knowledge.

These very simple quantitative arguments will be presented in a separate brief note under preparation here which will be submitted for Rapid Communication to Phys. Rev. D (the topic does not deserve a letter for PRL because it is trivial). The fact that this paper on the compatibility is forthcoming has been indicated in the paper submitted to PRL. I hope that these arguments can remove all doubts you may have on the complete lack of relevance of electroweak interactions with the topic of the paper.

A similar situation exists for all the other points you mention. As an example, you indicate the unquestionable successes of QCD. This paper under no way can be considered an alternative to QCD. In fact, the paper does not deal with the problem of the hadronic structure, either directly or indirectly. As a result, problems such as the electric dipole moments of nucleons are basically outside the objective of the paper.

Quite frankly, I am under the perhaps erroneous impression that the viewpoint expressed in your letter (which is much along that of the referee) is suggested by your advisors, and motivated by fears that our studies at large (rather than this paper) might damage the interests of academicians committed to quarks and QCD. In fact, I am confident you see perhaps more clearly than me the technical points indicated above, repeated in the

letter submitted to PRL, and stressed in the literature of the hadronic mechanics. At any rate, please rest reassured that your view is not shared by other physicists in quark fields I have contacted. In fact, they see no relevant connection between quarks theory and the problem of time-symmetry in nuclear reactions. Most importantly, permit me to reassure you that these colleagues in quark fields see no threat whatsoever to their research by our efforts in nuclear reactions. Finally, you should keep in mind that our institute is actively involved in a number of CONTRIBUTIONS to quark lines via the use of the hadronic mechanics, as well as via conventional mechanics. After all, you should keep in mind that I am the originator of the series "Developments in the quark theory of hadrons" edited by Rosen and Lichtenberg (and actually I have supported this project with personal funds to make it a reality).

But most importantly, even if you ignore the fact that extended charge distributions cannot be rigid, you must consider the experimentally established reality of the macroscopic irreversibility. All theories of particle physics which are unable to recover in a quantitative, credible way this experimental reality must be rejected. It is unfortunate that regrettable circumstances had forced you not to be present at our international conference in Orleans. In fact, you would have seen a river of substantial problematic aspects for Hamiltonian theories to be truly able to achieve quantitative compatibility. After all, the non-Hamiltonian character of the real macroscopic world is experimentally established beyond any conceivable doubt.

SOME POLITICAL ASPECT. I am afraid that the lack of publication of my note without true, credible, technical criticisms would be ethically wrong. I must stress the ethical profile because the scientific profile leaves no room for academic dances. In fact, as you know well, (I) nucleons are not points, but extended objects; (II) extended objects simply cannot be rigid; and (III) deformations of the charge distributions necessarily imply time-irreversibility because of the structure of operator ($\hat{3}$) above. Thus, the only scientific argument open at this time is the amount of the time-asymmetry. This is the primary reason why I have contacted the Editor in Chief of PR, Dr. Lazarus. In fact, I inteded to provide all the necessary information to prevent the creation of a record of unethical refereeing at Journals of the APS.

What is at stake here is not a single paper. First of all, the paper is the culmination and the representative of the virtual entirety of the First International Conference on Nonpotential Interactions and their Lie-admissible Treatment held under joint support by the French and the U.S. Governments with four volumes of proceedings, and participants from virtually all developed and developing countries. What is at state is therefore whether the voice of all these valuable scientists should be permitted or suppressed.

But there is much more. What is at stake is whether the journals of the APS encourage, or ortherwise permit all valuable papers in the interests at large of this Country, or they permit the publication only of papers compatible with the financial-academic interests of quarks/QCD studies.

Put it differently, what is at stake here is the true ultimate spirit of this Land, that is, whether we do have indeed a free pursue of valuable scientific knowledge, or we do have indeed a totalitarian filtering or scientific thought along established financial-academic interests. In fact, the paper does not claim to be the sole recipient of physical thruth, and actually encourages studies along different lines, as the SOLE genuine way to pursue novel knowledge.

These issues are very serious indeed. As indicated to Dr. Lazarus in a recent personal letter, this is the land where my children will live, and I intend to do everything in my power, at whetever personal costs, to contribute to its good scientific health. The future of my children is at stake here. In fact, if a paper is not published because of political reasons only, and without any credible technical reason, then the same may happen for an unlimited number of papers in different fields.

The downspiral of the Country because of ethical reasons would then be inevitable, unless groups of individuals have the courage to act in disrespect of their personal interests.

But there is much more. On military grounds, you should remember that all military systems are non-Hamiltonian. The promotion (let alone the permission) of theories of non-Hamiltonian character has therefore direct military value. It is only the community of quark physicists, in its general immodesty, that claims to have reached final knowledge via a small Lagrangian.

On civilian grounds, the problem of the amount of the time-asymmetry has profound implications for controlled fusion because of its origin (deformation of the charge distribution) and consequences (e.g., alteration of the magnetif moment, as rather natural in nuclear physics).

But there are additional reasons that I cannot disclose here in the best interests of all.

MY PROPOSAL. After three reviews, PRL has been unable to identify even one, credible, technical error or criticism of the paper submitted. It is therefore unlike that additional referees will be able to provide them.

The compromise I recommend is therefore that of
- publishing the paper in the form available with any additional clarification considered recommendable; and
- publishing soon after or jointly another paper by another author such as you which criticizes my paper,e.g., as a Rapid Communication in Phys. Rev. D.

To put it explicitly, I am inviting here you to collect your negative views on the nuclear time-asymmetry and make them available to the scientific community at large in the form of an official paper for publication.

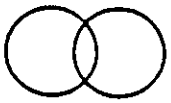
I would be delieghted to be the referee of such a paper, and ACCEPT IT FOR PUBLICATION.

You must understand that we sincerely welcome criticisms, and actually encourage them, as explicitly done in the paper, provided that they are done in a scientifically productive way. We simply cannot accept with grace unethical suppressions of plausible scientific views via a criptic process of an unknown referee.

At any rate, you should know that I have provided by now a complete disclosure of the case and of its documentation to colleagues and observers. Our final decision will be taken collectively (rather than by myself alone). The case, therefore, is already quite serious and under no circumstance should be under-estimated.

Sincerely

Ruggero Maria Santilli



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

September 18, 1982

Dr. GEORGE L. TRIGG
Editor
Physical Review Letters
1 Research Rd
RIDGE, N.Y. 11961

RE: "Use of the hadronic mechanics for the fit of the time-asymmetry...."
by R.M.Santilli, ref. PRL no. LR2111/L-1

Dear Dr. Trigg,

I am hereby formally asking that you include as part of the file on this paper the following copies of letters.

1. Letter by myself to Dr. Okubo (Rochester Univ) dated September 9, 1982;

2. Letter by Dr. Okubo to me dated September 14, 1982; and

3. Letter by myself to Dr. Okubo dated September 18, 1982.

Thank you.

Very Truly Yours,

Ruggero Maria Santilli

RMS-mlw

cc. Dr. Lazarus, Urbana, Illinois

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES · 1 RESEARCH ROAD
BOX 1000 · RIDGE, NEW YORK 11961
Telephone (516) 924-5533

PHYSICAL REVIEW C
Editor
H. H. BARSCHALL
University of Wisconsin-Madison
Associate Editors
G. J. DREISS
Editorial Offices
M. S. WEISS
Lawrence Livermore Laboratory

October 29, 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

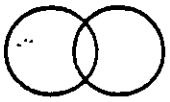
Dear Dr. Santilli:

Thank you for your note of Oct. 16 and the enclosed material. I am sorry that I did not have time to return your recent telephone call about the manuscript by the Quebec group. However, it is our policy not to discuss manuscripts with third parties.

Sincerely yours,

Gerard J. Dreiss
Associate Editor
Physical Review C

GJD/lf



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 1, 1982

Dr. G.J.DREISS,
Physical Review

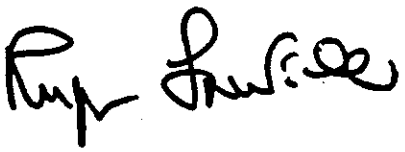
Dear Dr. Dreiss,

Just a few words to express my appreciation for the courtesy of your letter of October 29, 1982, as well as my full understanding of and agreement with its contents.

In case I can be of any assistance in the consideration of the paper by the Quebec group, please do not hesitate to call on me, either for possible technical reviews of certain theoretical aspects (the sole area of my expertise), or for consultation on advisability of accepting certain specific reports, of course, under the refereeing confidentiality.

From the courtesy of your letter, I am confident you have understood our objectives. We are only interested in the participation of Phys. Rev. in the scientific process of establishing or disproving the time-asymmetry (experimentally and theoretically) via scientific articles. For this purpose I am much in favor of the publication of papers presenting opposite views. Only the future will resolve the issue one way or another. What is vital for a healthy status of research is that plausible or otherwise valuable views are not suppressed at the level of the refereeing process.

Sincerely



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 2, 1982

Professor S. Okubo
Department of Physics
University of Rochester
ROCHESTER, New York 14727

Dear Professor Okubo,

I would like to formally bring to your attention the fact that the Kerkeley-Quebec-Bonn experimental group on the time-asymmetry has repeated the experiments, by confirming the original measures. A paper has been recently submitted to Phys. Rev. C as Rapid Communication. A courtesy copy of the paper is enclosed for your convenience.

Following specific agreements reached with Professor Lazarus, Editor in Chief of the Physical Review and Physical Review Letters, my letter providing a possible theoretical interpretation of the time-asymmetry for open nuclear reactions is under major reviews, for resubmission at some future time.

At this moment, permit me the liberty of recommending that you withdraw your comments on time-asymmetry via a formal letter to Dr. Lazarus, on the grounds that at the time of your comments you were not aware of the repetition of the experiments (which is definitely true). We believe that this is a perfectly justified action under the circumstances which will be beneficial to you as a scientists, as well as to the pursuit of novel physical knowledge. Also, your acceptance of our recommendation might halt a rapid deterioration of the case, which has already reached alarming proportions.

The courtesy of your communication as soon as possible of your decision on the matter would be appreciated.

Very Truly Yours,

Ruggero Maria Santilli
President

RMS-mlw

THE UNIVERSITY OF ROCHESTER
RIVER CAMPUS STATION
ROCHESTER, NEW YORK 14627

DEPARTMENT OF PHYSICS
AND ASTRONOMY

November 10, 1982

Prof. R. M. Santilli
The Institute for Basic Research
96 Prescott St.
Cambridge, MA 02138

Dear Prof. Santilli:

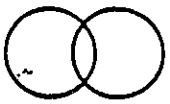
I am puzzled by remarks made in your recent letter of Nov. 02. I am embarrassed to confess that I was one of the referees of your paper as you rightly guessed. Although I did not recommend its publication to the Letters, I suggested that it should be published rather in Phys. Rev. Indeed, the urgency criteria for Letters, which the editors demand for referees, did not leave any other choice. However, I did not make any other written statement to Dr. Lazarus which you mentioned in your letter. As a matter of fact, I was obviously in a delicate situation, since you are my friend and since I believe basically the possible relevance of non-associative algebra to physics. Because of this delicacy, I requested of Dr. Lazarus that I would not any more serve as a referee of your paper for the second time, and suggested to him names of some physicists who might judge your paper impartially. That was the extent of my dealings with Dr. Lazarus.

I hope that this letter will clear up any misunderstanding.

Sincerely,

S. Okubo

SO:jm



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

December 14, 1982

Dr. D. LAZARUS, Editor in Chief
Physical Review and Physical Review Letters
Department of Physics, University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

I hereby respectfully submit for publication in **PHYSICAL REVIEW LETTERS** the enclosed paper entitled

A POSSIBLE TIME–ASYMMETRIC MODEL FOR OPEN NUCLEAR REACTIONS

As you can see, the paper deals with an intriguing, fundamental, open problem of contemporary physics: the origin of the irreversibility of our macroscopic world. As such, it touches aspects in separate branches of physics. I am therefore taking the liberty of recommending a comprehensive review and, for this task, I enclose some 20 copies of the paper and of this letter.

Confident in your benevolent understanding and cooperation, in this letter I shall identify some of the major technical aspects deserving specific review. The selection of referees in this case does not appear to be an easy task. In the hope of being of some assistance in this respect, I shall also identify the leading experts in each field considered. I shall remain at your disposal for mailing to you on request a copy of all needed references, including monographs and conference proceedings, as well as for any other assistance you may desire.

1. NEWTONIAN MECHANICS. The Newtonian foundations of the paper are evidently the first aspect deserving a specific inspection. This is recommendable also in view of recent advances in the field, with particular reference to the achievement of the Birkhoffian generalization of analytic mechanics for contact/nonpotential forces [see the monographs of ref.s 2,3]; which constitute the classical foundation of the analysis.

These advances have not yet reached the physics audience at large, and are known only to experts in the fields. To have meaningful referee reports, it is therefore essential that you select **referees with a record of publication in non–Hamiltonian systems.** The best I can recommend are

– **Professor R. BROUCKE**
Department of Aerospace
Engineering and Engineering Mechanics
University of Texas at Austin
AUSTIN, Texas 78712–1085

Professor J. KOBUSSEN
Swiss Federal Institute
for Reactor Research
CH–5303 WURENLINGEN, Switzerland

— Professor H. H. E. LEIPHOLZ
Solid Mechanics Division
University of Waterloo
WATERLOO, Ontario Canada

Professor K. HUSEYIN
Department of Systems Design
University of Waterloo
WATERLOO, Ontario N2L 3G1 Canada

2. STATISTICAL MECHANICS. The second branch of physics which should be taken into consideration for any refereeing on irreversibility is, of course, statistical mechanics. As you can see, I have attempted to give proper credit in the paper to Nobel Laureate

— Professor I. PRIGOGINE
Faculte des Sciences
Universite Libre de Bruxelles
1050 BRUXELLES Belgium

and

Center for Statistical Mechanics
The University of Texas
AUSTIN, Texas 78812

In fact, the studies by his group have been fundamental in the identification of the non—Hamiltonian character of irreversibility at the level of statistical ensemble.

Additional experts on the non—Hamiltonian origin of irreversibility that I recommend are

Professor J. FRONTEAU
Departément de Physique
Universite d'Orléans
45046 ORLÉANS CEDEX
France

Professor S. GUIASU
Departement de Mathématiques
Université du Québec a
Trois—Rivières
Case Postale 500
TROIS—RIVIÈRES,
Québec G9A 5H7 Canada

Professor A. TELLEZ—ARENAS
Departément de Physique
Universite d'Orléans
45046 ORLÉANS CEDEX
France

Admittedly, the non—Hamiltonian origin of irreversibility may still not be accepted by individual physicists. However, since it is incontrovertible at the Newtonian level, it is manifestly plausible in Statistical mechanics, to say the least.

One of the first tasks expected from you, as Editor in Chief of the Journals of the American Physical Society, is that of preventing the possible suppression of plausible fundamental views via the referees process. For this task, permit me to recommend, mot respectfully, that you exercise particular care in the refereeing of the statistical profile. Of course, individual statisticians may not necessarily share Prigogine's view on the origin of irreversibility. The important point is that these personal views by individual statisticians are not used to suppress plausible fundamental advances.

3. EXPERIMENTAL ASPECTS. As stated clearly in the paper, the experimental foundation of the paper is given by the apparent deformation of the charge distribution of hadrons during impact and penetration within nuclear matter, as measured in experiments [16]. In fact, the time—reversal operator is made up to two terms, a spin term and one for complex conjugation. A possible deformation of the charge distributions of nucleons "must" therefore imply a form of time—asymmetry.

It appears recommendable that, on experimental grounds, you consult above all the originator of experiments [16],

— Professor H. RAUCH
Atominstitut
Schuttelstrasse 115
A—1020 WIEN, Austria

The additional and more direct experimental basis of the paper is given by the measures of the time–asymmetry in certain nuclear reactions [ref.s 14]. Again, it is advisable to consult the team leaders

— **Professor R. J. SLOBODRIAN**
Laboratoire de Physique Nucléaire
Université Laval
QUÉBEC G1K 7P4 Canada

Professor H. E. CONZETT
Lawrence Berkeley Laboratory
University of California
BERKELEY, California 94720 USA

who are excellent theoreticians, besides being distinguished experimentalists.

As you are aware, an experimental group at Los Alamos is currently confuting the amount of time–asymmetry of the measures by the Slobodrian–Conzett group, ref. [15]. Again, I recommend the consultation of at least some member of this additional team, such as

— **Professor R. A. HARDEKOPF**
Los Alamos Scientific Labs.
Mail Stop 480
LOS ALAMOS New Mexico 87545

Professor L. VEESER
Los Alamos Scientific Labs.
Mail Stop D410
LOS ALAMOS, New Mexico 87545

The understanding is that the disagreement we are referring to here is for the **AMOUNT** of time–asymmetry. I expect that the Los Alamos group agrees with me that the time–reversal symmetry is indeed violated in **OPEN** (nonconservative) nuclear reactions. Therefore, and this should be stressed to avoid unnecessary incidents, the Los Alamos rebuffal of the Slobodrian–Conzett measures has **NO BEARING** on the paper submitted. In fact, the paper presents a possible model of time–asymmetry with the understanding that the amount of violation must be finalized via future experiments.

4. THEORETICAL ASPECTS. The paper submitted is an offspring of my failures to achieve a quantitative interpretation of the **EXPERIMENTALLY ESTABLISHED** irreversibility of our real world, with the **CONJECTURED** reversibility of the particle dynamics of the nuclear world.

To understand the problem, you should recall that, on one side,

— the time evolution of open systems of our real world is necessarily **NONHAMILTONIAN–NONCANONICAL** (different views may tacitly imply the validity of the perpetual motion in our environment....);

while, on the other side,

— the time evolution of currently predominant theoretical views in nuclear physics is of **HAMILTONIAN–UNITARY** nature.

My failures are due to the inability to reach the former via a large collection of the latters in any scientific way (that is, without politics and related language).

As a result of this situation, the paper proposes an irreversible particle mechanics via a simple generalization of the current (rather old) views, in the hope of contributing toward the future resolution of this magnificent open problem. By no means the paper claims the achievement of a final solution in favor of irreversibility. It merely claims plausibility. By the same token, and this should be stressed to prevent unnecessary incidents, no physicists can claim today final knowledge with his reversible/Hamiltonian/unitary particle dynamics. We must all face this situation and acknowledge that each of our personal views is tentative.

In the hope of minimizing some of the (rather numerous) prejudices in the field, the paper stresses (beginning with the title) that the irreversibility is referred to OPEN systems, while a reversible center-of-mass trajectory is recovered in full when the system is implemented into a closed form including the external terms.

Finally, the model tries to stress the expected dependence of the time–asymmetry from the local physical conditions, such as size of hadrons, energy of collisions, ect. This is so in the hope of indicating to possible readers in other fields (such as quarks and QCD) that the model submitted IS NOT in disagreement with their views [in actuality, the model opens up an array of intriguing possibilities of novel contributions to quarks and QCD I hope to have the opportunity to illustrate in some other paper].

It is evident that the best referees of the paper are experts in the field, that is, **physicists with a record of publications, specifically, in NON–Hamiltonian/NON–Lagrangian particle mechanics.** Different views would be equivalent to the submission of papers, say, on quarks to referees who have never written one paper on quarks, which is a manifestly unwarranted editorial practice. It is a fact that physicists "in good standing" at the American Physical Society who are experts in NON–Hamiltonian/NON–Lagrangian mechanics are today very rare. This is not a decifiency of the APS. Instead, it is an indication of novelty of the paper. Nevertheless, this is a fact that should be faced and acknowledged to avoid referees venturing judgments under the illusion of knowledge. It is evident that a non–expert in the field may reach a mature judgment. However, he/she must be willing to reach at least a superficial knowledge of a rather considerable volume of publications which constitute the mathematical and physical foundations of the paper.

The best experts in the field I can recommend to you are

— **Professor R. MIGNANI**
Istituto di Fisica
dell'Universita'
Piazzale Aldo Moro, 2
00185 ROMA Italy

Professor A. J. KALNAY
Instituto Venezolano
De Investigaciones Cientificas (IVIC)
Centro De Fisica
Apdo. 1827
CARACAS 1010 A, Venezuela

— **Professor G. EDER**
Atomic Institute of the Austrian
Universities
Schuettelstrasse 115.
1020 VIENNA Austria

Professor Y. NAMBU
Enrico Fermi Institute
University of Chicago
CHICAGO, Illinois 60637

Additional physicists you may consider who are outstanding, but do not have an extablished record of publication in non–Hamiltonian/NON–Lagrangian mechanics, are

— **Professor L. C. BIEDENHARN, Jr.**
Duke University
Department of Physics
DURHAM, North Carolina 27701

Professor S. OKUBO
Department of Physics and Astronomy
University of Rochester
ROCHESTER, New York 14260

5. MATHEMATICAL ASPECTS. The true foundations of the paper are those of the so–called "hadronic mechanics" [read: isotopic lifting of the Hilbert space]. The novelty of these studies is such that no theoretician, beginning with myself, can be considered an expert of the new mechanics. In fact, the only experts available at this time are mathematicians. This is a reality you should take into consideration to avoid potential basic misjudgments in the referee process, with the consequential creation of unnecessary incidents.

The leading mathematician in isotopic generalization of Hilbert spaces (and algebras) is

Professor H. C. Myung
Department of Mathematics
University of Northern Iowa
CEDAR FALLS, Iowa 50613

I believe you should consider his advice, of course, on the soundness of the mathematician foundations only.

Additional distinguished mathematicians, experts in the mathematical foundations of the paper, are

Professor M. L. TOMBER
Department of Mathematics
Michigan State University
EAST LANSING, Michigan
48824

Professor R. H. OEHMKE
Department of Mathematics
University of Iowa
IOWA CITY, Iowa 52240

Professor A. A. SAGLE
College of Natural Science
University of Hawaii at Hilo
1400 Kapiolani Street
HILO, Hawaii 96720

In summary, I suggest an in depth review by differentiated experts in all the major lines of the inquiry. The task of combining all reviews in a final judgment is, of course, yours.

6. IMPROVEMENTS. Permit me to express my best possible cooperation and gratitude for any suggestion of improvements by the referees. Often, however, one of the most difficult tasks for an author is to understand the improvements desired by the referee. Permit me, therefore, to encourage the referee to be as specific as possible in the desired corrections, not only in their identification by word, or sentence, or formula of the current paper, but also in the desired modification. Also, it is important to prevent that simple modifications suggested by the referee be interpreted as rejection because of lack of sufficiently clear language in the report.

To minimize these rather frequent confusions which end up to be damaging to the Journal, to the referee, and to the author, I have implemented at the HADRONIC JOURNAL the practice of presenting to the authors referee reports favoring a possible future publication, according to the following guidelines:

[a] we first indicate as clearly as possible that the paper may indeed be suitable for publication in case it is improved according to guidelines specified below.

[b] we then identify each and every word, statement, or formula recommended for revision, and for each of them suggest possible improvements; and

[c] often, we also enclose one copy of the paper with editorial markings on the critical passages, to make sure that the authors understands the points in need of revisions.

I do not know whether your Journal can implement a reporting policy of this type. Nevertheless, I passed it to you as one of the possible ways to avoid misrepresentations of the referee report because of their insufficient clarity on the truly essential issue: whether the referee is for or against publication of the paper following his suggested improvements.

7. INSPECTION OF REPORTS. Referee reports constitute a scientific document and, thus, they must be inspected for scientific content, value, and credibility in exactly the same way as the paper itself. It is now common practice at the HADRONIC JOURNAL to reject and return to the referees (rather than to the authors) all reports that are questionable on grounds of language, contents, objective, etc.

The paper submitted is a representative of a growing scientific current interested in exploring possible basic advances in the structure of our contemporary physical knowledge. Your handling of the paper will be important in influencing whether other papers along the same lines of inquiry will be submitted to your Journals, or other scientific conduits should be considered.

For these reasons it is essential, in my view, that referee reports be inspected for scientific contents and value. Comments and/or criticism without sufficient credibility should be returned to the referees, in my view, and they should not be released by your office. In fact, they can be damaging to your Journal.

But, most importantly, you should take into consideration the ultimate hope of the paper, that of promoting an orderly scientific dialogue in a fundamental open problem. Your most important task is therefore that of preventing the suppression of this scientific process at the referee level, and permitting instead the participation of the physics community at large.

The orderly scientific process of trial and error, via the presentations of plausible views in physics journals and their critical examination by other independent, researchers via papers also in physics journals, is, without doubt, the only way to pursue novel physical knowledge.

I have mailed copy of this letter only to Dr. P. W. Anderson (Princeton University) in his quality of Chairman of the Publication Committee of the American Physical Society. I have abstained from mailing copy of this submission to any other member of the editorial organization of your Journals, and left this task to your discretion.

I would like to take this opportunity to express to you and to your family my sincere and best Season Greetings.

Very truly yours,

Ruggero Maria Santilli

cc: Dr. P. W. ANDERSON

RMS/mlw

Encls.:

—two one—sided originals of manuscripts
—calculation of length
—twenty copies of manuscript, of this letter, and of of detailed calculations

P. S. —

As you can see, I do not receive papers for our journals, and so I have forwarded your message to Dr. Trigg.

Merry Christmas,

C. Z.

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

December 22, 1982

Dr. George L. Trigg, Editor
Physical Review Letters
1 Research Road
Box 1000
Ridge, NY 11961

Dear George:

Attached is a large number of copies of a paper which was just received from Dr. R. M. Santilli, intended for submission to Physical Review Letters. Dr. Santilli is evidently under the misimpression that I, rather than the actual Editors, receive papers for the journal.

There is also a quite long letter in which Dr. Santilli describes what he considers proper criteria for review of the paper, and suggests names of many possible referees. Naturally, all of this information is merely suggestive for you in your selection of referees, since the selection of referees is and has always been the perogative of the Editors.

Dr. Santilli also suggests in his letter certain procedural changes in use of referee reports which are not consistent with usual PRL policies. Naturally, you are expected to follow our established policies for review and acceptance of papers, and ensure that this paper receives the same fair and equitable handling that we give all papers submitted, no more and no less.

A copy of this letter is being sent to Dr. Santilli, and I presume he will receive the usual acknowledgement from you when the paper is actually received at PRL.

Sincerely,

David Lazarus

xc: Dr. P. W. Anderson
Dr. R. M. Santilli

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE NEW YORK 11961
Telephone (516) 924-5533

February 11, 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Manuscript No. LZ2206

Dear Dr. Santilli:

We have received at least one referee report on your manuscript entitled "Possible time-asymmetric model for open nuclear reactions".
There are no criticisms that require your attention now. Since a decision cannot be reached on the basis of the material at hand, we are seeking further advice.

Sincerely,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

GLT/jaw

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE, NEW YORK 11961
Telephone (516) 924-5533
Telex Number 971599
Cable Address PHYSREV RIDGENY

March 4, 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Manuscript No. LZ2206

Dear Dr. Santilli:

We have received at least one referee report on your manuscript entitled "Possible time-asymmetric model for open nuclear reactions." There are no criticisms that require your attention now. Since a decision cannot be reached on the basis of the material at hand, we are seeking further advice.

As for the receipt date, our instructions clearly state that manuscripts are to be sent to this office. If you choose to disregard the rules, you must accept the consequences. The date of receipt of any manuscript is the date it reaches this office.

Sincerely yours,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

GLT/jaw

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

April 2, 1983

Dr. GEORGE L. TRIGG, Editor
Physical Review Letters
1. Research Rd
RIDGE, N.Y. 11961

RE: Paper LZ2206 entitled
"Possible time-asymmetric model for open nuclear reactions"
Submitted in December 1982

Dear Dr. Trigg,

I would like to express my appreciation for your two notes of February 11, 1983 and March 4, 1983. Please ignore the issue of the date of submission because completely immaterial [at the time of the submission I was unaware of the fact that the "Editor in Chief" of your Journals is not an Editor].

Jointly I would like to lament the unusually long time that is taking for the consideration of the letter. In a few days it will be ONE FULL YEAR since the submission of the preceding letter LR111. The new letter LZ2206 has already been at your office for a period of time longer than the average time of publication (let alone review) of papers aligned with vested academic interests in control, not to mention the period of time occurring for the publication of papers signed by members of the editorial organization of your journal.

The reason why this is, for several members of our group, an astonishing occurrence is due to the incontrovertible character of the underlying scientific truth. The case deals with OPEN (NONCONSERVATIVE OR DISSIPATIVE) NUCLEAR reactions, that is, processes that have been hystorically treated via NONHERMITEAN Hamiltonians and NONUNITARY time evolutions

$$A' = \exp(-itH^{\dagger})A \exp(+itH) \;, \qquad H^{\dagger} \neq H \tag{1}$$

whose irreversibility is incontrovertible. Our model merely presents an algebraically consistent treatment of an already established fact of nuclear physics. Indeed, the brackets of the infinitesimal version of law (1)

$$idA/dt = AH^{\dagger} - HA \tag{2}$$

do not characterize a consistent algebra, trivially, because trilinear. Our Lie-admissible reformulation

$$\begin{aligned} A' &= \exp(-itET^{\triangleright}) \; A \; \exp(+it^{\triangleleft}TE) \\ H^{\dagger} &= ET^{\triangleright} \;; \qquad H = {}^{\triangleleft}TE, \quad (T^{\triangleright})^{\dagger} = {}^{\triangleleft}T, \; E^{\dagger} = E \end{aligned} \tag{3}$$

then permits the achievement of a consistent algebra for the infinitesimal behaviour

$$idA/dt = A{}^{\triangleleft}TH - ET^{\triangleright}A \tag{4}$$

as well as a number of advances that do not appear to be readily achievable via a time evolution with inconsistent algebraic structure, such as (1). In fact, the time-asymmetry

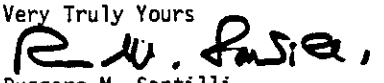
$$P^{\triangleright}/{}^{\triangleleft}A = {}^{\triangleleft}T \;/\; T^{\triangleright}, \tag{5}$$

is readily achievable via law (3), but not via law (1). Similar occurrences exist for several other aspects of OPEN reactions of EXTENDED CHARGE DISTRIBUTIONS (neutrons), such as the representation of their deformation under sufficiently intense external fields.

The experimental situation is equally distressing and my voice has been continually ignored without any credible counterargument. In fact, the experiments on time-asymmetry by the Berkeley-Quebec group and those by the Los Alamos rebuffal are all of OPEN character, trivially, because they deal with beams on FIXED EXTERNAL TARGETS.Under these conditions, the amount of the irreversibility is certainly open to debate, but the existence of the irreversibility should be out of the question to avoid shadows of scientific manipulations. In fact, to prove that he/she is in good faith, any experimentalist claiming exact time-reflection symmetry under open reactions must prove that law (1) is reversible, which is by far a quite difficult task, assuming that a credible proof can be established.

The letter LZ2206 merely intends to clarify this latter point. Such large delays in its consideration, whether accidental or premeditated, share the same risk: continued doubts of the existence of scientific manipulations at the journals of the APS in the interests of individual groups in academia, and in disrespect of the interests of the Country for scientific advances.

Very Truly Yours



Ruggero M. Santilli

cc.: Drs. D. Lazarus, D. Nordstrom, and G.J.Deiss, PR

P.S. With the passing of time, we have acquired new knowledge that may be useful to improve a few words of paper LZ2206. For instance, it may be appropriate to clarify that the operation of isotopic Hermiticity recalled at the bottom line of page 2 holds for the conditions stated in ref. 9, i.e., for isotopic enveloping algebras acting on a conventional Hilbert space. If the latter is also subjected to the same isotopic lifting of the envelope, than the operation of Hermiticity is the conventional one. If the two isotopies are different, then the operation of Hermiticity is even more general than that of ref. 9 as reviewed in p. 2.

Kindly advice whether this clarification (and a couple of others) should be mailed to you, or I should waitfor possible improvements of the papers suggested by a (true) referee.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES
1 RESEARCH ROAD BOX 1000 RIDGE, NEW YORK 11961
Telephone (516) 924-5533

6 April 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Possible time-asymmetric model for open nuclear reactions
By: Ruggero Maria Santilli

LZ2206

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

enc.

R2L

April 9, 1983

Dr. GEORGE L. TRIGG, Editor
Physical Review Letters
P.O.Box 1000, RIDGE, New York 11961

RE: final rejection dated April 6,1983,with three referees' reports on the paper LZ2206 entitled "Possible time-asymmetric model for open nuclear reactions", following rejection of paper LR2111 submitted on April 16, 1982.

Dear Dr. Trigg,

The available records point rather strongly toward apparent scientific misconduits occurred at your journals in the handling of theoretical and experimental papers on the problem of irreversibility of open nuclear reactions.

The claim is too diversified to be effectively expressed here. The bottom lines is constituted by the fact that the irreversibility of theoretical models based on nonunitary time evolutions for the description of open (e.g., dissipative) nuclear reactions is an absolutely incontrovertible scientific truth. None of the numerous referees' reports mailed to me over the full year of consideration of my papers has acknowledged, even indirectly, this incontrovertible fact. None of them exhibited even the most minute,true scientific content, or any suggestion whatsoever that could be valuable for the improvement of my papers, or any acknowledgment of the ultimate essence of the paper, as recently reviewed in my letter to you of April 2, 1983 (a mere Lie-admissible reformulation of nonunitary time evolutions, with consequential, manifest, incontrovertible irreversibility). Your suppression of the publication of this plausible view therefore supports quite strongly the allegation of editorial misconduits.

Additional, perhaps graver editorial misconduits are due to a number of apparent discriminatory practices at your journals regarding research conducted under governmental support. I am referring here to the fact that the calls of extreme editorial rigour implemented for my papers do not appear to be equally implemented for all other papers. This situation can be established beyond any reasonable doubt by conducting independent refereeing of papers already published in your journals, such as, to mention only one case, the extension of quark conjectures to the structure of the deuteron without any treatment of its most fundamental and well known property, the lack of exited states. Additional discriminatory practices can be potentially identified in the selection of referees. In fact, referees for papers on quark conjectures are solely and exclusively selected among experts in the field, that is, researchers with a substantial record of publication of papers in the field, as very well known. On the contrary, the referees selected for my papers had no meaningful knowledge whatsoever of the field of the paper (Lie-isotopies and Lie-admissible genotopies of Hilbert spaces), let alone a record of publication in the field. This additional, apparent,discriminatory practice on governmental research can also be established beyond a reasonable doubt by inspecting the referees' report themselves. You have in your file my comments on the preceding reports. Enclosed you will find my additional comments on the last three reports.

But the gravest editorial misconduits have apparently occurred, not in regard to my theoretical papers, but instead in regard to the experimental papers on irreversibility by the Berkeley-Québec collaboration. In fact, the first paper by this group (PRL 47, 1803 (1981)) was kept for over one-and-one-half years, in the apparent intent to give time to an experimental group at Los Alamos to prepare a rebuffal, and have it quoted in the former paper (as it was). More recently, the publication of new measures of polarizations indicating irreversibility by the Québec group (J. Pouliot et al) as rapid communication in Physical Review C,was suppressed at the refereeing level, despite the availability of additional supporting information, as appeared, e.g., in Nuclear Physics A394, 428 (1983). This latter episode followed editorial lines much similar to those of paper LZ2206, that is, by ignoring the fact that the irreversibility for open reactions (such as those considered by the experimenters) is incontrovertible, and only its amount is open to scientific debate.

It is evident that the appropriate editorial conduit in these experimental papers should have been that hystorically followed for the pursuit of novel human knowledge: publication of plausible novel results and, subsequently, their critical examination by independent researchers in separate papers. The rather voluminous file on irreversibility indicates, quite strongly, that you have decided to suppress possible advances at the editorial level, and assumed the astonishing role of arbiters of possible advances whenever they are manifestly or potentially against existing, vested academic interests [the comment does not apply when the papers are aligned with said interests, as one can see from the rapidity with which PRL publishes the papers authored by its editor R.K.ADAIR and his friends].

A most sustantial evidence supporting the allegation of editorial misconduits is provided by a letter of Professor S. OKUBO dated November 10, 1982 [copy enclosed], in which one can read that he recommended the publication of paper LR2111 in Phys. Rev. (rather than PRL). As verbally expressed to Dr. LAZARUS, Editor in Chief, and as confirmed in writing, such type of publication would have been perfectly acceptable to me. Evidence establishes that Professor OKUBO's recommendation WAS NOT followed by your journals, despite the fact that such alternative publication would have resolved the case. Thus, professor OKUBO's letter, not only supports the allegation of editorial misconduits, but, at the extreme, might also be interpreted as indicating a conceivable conspiracy by vested academic-financial-ethnic interests to suppress undesired advances in physical knowledge.

For the sake of clarity, I should indicate that, to my knowledge, the apparent scientific misconduits considered here do not violate existing Codes of Laws [with the potential exception of aspects regarding possible discriminations on papers under governmental support]. The same alleged scientific misconduits also cannot be qualified as being "scientifically unethical" because, as well known, the American Physical Society does not subscribe to a Code of Ethics, by therefore preventing in this way any ground for ethical judgment.

Nevertheless, the scientific, economic, and military damage caused to America, as well as to the human society at large, by your editorial practices has the potential of being much more damaging than ordinary crime.

On my part, I have provided over one full year period all conceivable efforts for an orderly resolution of the case. Since I cannot compromise with my own ethical standards, I feel obliged to undertake all the necessary steps so that the American public, as well as the international public, is fully informed of the entirety of the case regarding the handling by your journals of the theoretical and experimental papers on irreversibility, as well as of other apparent extremes of misconduits occurred in other sectors of the U.S. physics community, the disclosure being expected to be made at some appropriate future time either by myself, or via my estate in Europe, or via interested U.S. attorneys, physicists, and ordinary taxpayers.

Very Truly Yours

Ruggero Maria Santilli
96 Prescott Street
Cambridge, Massachusetts 02138

cc.: Drs. D. LAZARUS, D. NORDSTROM, and G.J.DREISS, Physical Review
Dr. N.D. PEWITT, Office of Science and Technology, The White House

COMMENTS ON THE ENCLOSED REFEREE REPORT NO. 1.ON PAPER LZ2206
by R.M.Santilli

This is a scientifically responsible report written in respectful language, but, regrettably, it cannot be used for judgment because the referee, quite honestly, acknowledges his/her lack of expertise in the field of the paper.

Note that this referee recommends SPECIFICALLY, that PRL selects referees who are true experts in

> "...the extensions of Lie algebra to the Lie-isotopic and Lie-admissible constructions described in the manuscript."

Regrettably, it does not appear that this sound, and quite natural suggestion was followed by PRL, as evident from an inspection of the subsequent reports.

In turn, the problems regarding paper LZ2206 are not given by reports made by unqualified referees, but rather by their selection by PRL as well as by the PRL formal acceptance of their report.

T SHOULD BE STRESSED THAT A LIST OF ALL TRUE EXPERTS IN THE FIELD OF THE PAPER IN NORTH AND SOUTH AMERICA AND IN EUROPE, INCLUDING OUTSTANDING SCIENTISTS, WAS MADE AVAILABLE TO ALL EDITORS OF THE PR AND PRL.

Referee's Report No. 1

Title: Possible time-asymmetric model for open...

Author: Ruggero M. Santilli

Ms.No.: LZ2206

This paper describes an attempt to connect the known time-irreversibility of macroscopic physical processes to an assumed time-irreversibility of fundamental nuclear and particle interactions. Although this assumption is contrary to the established theoretical view, it should not be rejected summarily. The established CP-violation in K^{o} decays implies T-violation via the CPT theorem, and this unique result still has no satisfactory explanation in terms of a T-asymmetric interaction with its manifestations in other particle physics processes.

In my view, the question is whether or not the theoretical development described in this paper has any real merit at the level of nuclear and particle physics, and I am not qualified to make such a judgement.

I recommend that you seek the advice of a nuclear or particle theorist who has some experience or knowledge of the extensions of Lie algebra to the Lie-isotopic and Lie-admissible constructions described in this manuscript.

COMMENTS ON THE ENCLOSED REFEREE REPORT NO. 2 ON PAPER LZ2206
by R.M.Santilli

This referee has no meaningful knowledge on the topic of the paper, as defined in report no. 1, let alone a proved record of expertise.

The occurrence can be established beyond reasonable doubts by the claim that there is no clear relation between Prigogine's (statistical) work and the model presented in the paper, despite the recall that the former originates via a "nonunitary transformation".

In fact, anybody with a minimum of knowledge of Lie-isotopy knows that a nonunitary transformation of the conventional Lie product produces exactly the Lie-isotopic time evolution of the paper, Eq. (1), p. 3 of LZ2206 (see, e.g., ref. 2, p. 225). Thus, the fundamental dynamical equations for the CLOSED-EXTERIOR treatment of the model are exactly the particle-version of Prigogine's statistical time evolution, only written in an algebraically understandable way [the clarification of the point was avoided in the note, not because of lack of space in a letter, but because so repetitive to appear verbose and even offensive to experts in the field].

The lack of any qualification whatsoever by this referee is further proved by his/her disclaim of the lack of relationship between Prigogine's nonunitary time evolutions and the non-Hamiltonian origin of irreversibility suggested in paper LZ2206.

Again, anybody with knowledge of the background work leading to the paper knows that the classical image of a nonunitary transformation of Heisenberg's equations CANNOT be Hamilton's equations (they are instead given by the non-Hamiltonian, Birkhoff's equations). Thus, under the conditions of the paper, the non-Hamiltonian character of the model is absolutely incontrovertible and established in all necessary details in the literature quoted in the paper [as an incidental note, Prigogine and his collaborators went to considerable pain in their papers to clarify the care needed before interpreting "H" as the energy under a nonunitary transformation. This is recalled here to established Prigogine's priority of the discovery].

It is therefore evident that this referee, not only is basically unknowleadgeble of the literature on Lie-isotopy and Lie-admissible genotopy, but he/she is basically deficient in the knowledge of Prigogine's work that lead to his Nobel price!

DESPITE THAT, THIS REFEREE PASSES JUDGMENT AND SUGGESTS THE REJECTION OF THE PAPER. IS THEN THIS DECISION MADE IN GOOD FAITH? OR IS THE DECISION THE RESULT OF A CALCULATED MANIPULATION OF SCIENTIFIC TRUTHS AIMED AT NONSCIENTIFIC OBJECTIVES?

SINCE THE MERE SHADOW OF A DOUBT ON ETHICAL ISSUES IN REFEREEING IS SUFFICIENT TO DISQUALIFY A REFEREE, THE AMERICAN PHYSICAL SOCIETY IS HEREBY URGED TO REMOUVE THIS REFEREE FROM THE ACTIVE FILE, AND, MOST IMPORTANTLY, TO ABSTEIN FROM THE SUBMISSION OF PAPERS IN HIS/HER OWN FIELD, LET ALONE OTHER FIELDS.

BUT THE MOST DISTRESSING ISSUE IS NOT THE INCOMPETENCE OF THE REFEREE, BUT INSTEAD THE FACT THAT PHYSICAL REVIEW LETTERS HAS FORMALLY ACCEPTED THE REPORT IN A REFEREEING PROCESS REGARDING RESEARCH CONDUCTED UNDER GOVERNMENTAL SUPPORT. IT IS THIS LATTER ASPECT THAT RAISES A HOST OF SCIENTIFIC, ETHICAL, AND LEGAL PROBLEMS.

Referee's report No. 2

We have read the paper by R.M. Santilli entitled: "A possible Time- Asymetric Model for Open Nuclear Reaction."

Unfortunately, this paper appears to be so obscure that we are unable to judge what is exactly claimed and even less, what is proven.

Certainly, there is no clear relation with the work of Prigogine et al. on the origin of irreversibility in statistical physics, certainly to what seems to be implied by this paper. That work starts with hamiltonian and shows that when well-defined assumptions are made on the nature of the system, the time-symmetry may be broken by a nonunitary transformation. This seems to have little to do with what the author calls the non-hamiltonian origin of irreversibility.

We cannot recommend this work for publication.

COMMENTS ON THE ENCLOSED REFEREE REPORT NO. 3 ON PAPER LZ2206
by R.M.Santilli

This referee too has no significant knowledge of the field of the paper. But, unlike the author of report No. 2, this referee enters into considerably more elaborations totally deprived of any meaning for the topic of the paper, not to mention gross misrepresentations (such as the quote of "non-Birkhoffian mechanics" ?!?!).

The referee recalls from Jacobson that an isotopic lifting ATB of an associative algebra AB is no generalization of the associative algebra itself. This is so trivial that the quote of Jacobson is verbose (my son in junior high school can see it). Paper LZ2206 does not deal with asbtract mathematical structures. It deals instead with the physical implications of different realizations such as ATB and AB. Specifically, it shows that the former permits the recovering of the exact time-reflection symmetry for the center of mass trajectory of extended systems with non-Hamiltonian internal forces, while the irreversibility occurs only for open interior processes.

A most incongrous claim by this referee is that the paper would be "almost completely inaccessible to the general readership of PRL". The pertinent question is then the following: is PRL publishing papers that are accessible to ALL physicists, or PRL publishes papers that are accessible only to experts in the field, or to readers that can become experts upon (and ONPY UPON) studying the literature quoted in the paper? The evidence in support of the latter alternative is to overwhelming to prevent the need of additional comments.

A further, hardly believable posture by this referee is that, since the experimental situation on irreversibility is unsettled, paper LZ2206 should not be published. But NOW (AND NOT YEARS FROM NOW) there is the need for theoretical elaborations, because this is the ONLY way for experimental studies to reach true maturity. The referee's posture under consideration is therefore strictly antiscientific, in my view.

But the most insidious (and for me offensive) suggestion is the last, to the effect that I should prepare a longer paper for submission (apparently) to Physical Review. Apart the fact that a longer paper would imply repetions over repetitions of results published and republished, the claim is rendered insidious because of the years of times that it has taken in the past for my publishing papers in Physical Reviews, whenever the topic (or even one sentence contained in it) was not fully aligned with existing interests or general views. Thus, the suggestion to write a longer paper is literally equivalent to the suppression of the publication of the model for all the necessary additional time (months) to write the new paper, as well as the continuation of this senseless expression of refereeing deprived of scientific sense, which could likely take a number of years.

AGAIN, THIS REFEREE PASSES JUDGMENT DESPITE ITS MANIFEST AND EXPLICITLY ADMITTED LACK OF EXPERTISE IN THE FIELD OF THE PAPER. IS THIS ACCIDENTAL OR CONSPIRATORIAL?

AGAIN, THE AMERICAN PHYSICAL SOCIETY IS URGED TO REMOUVE THIS REFEREE FROM ITS ACTIVE FILE, AND ABSTEIN FROM SUBMITTING PAPERS TO HIM/HER PARTICULARLY IN HIS/HER FIELD.

ARE THE EDITORS OF PRL USING THEIR NEXT DOOR NEIGHBOORS FOR MEDICAL ASSISTANCE, OR THEY USE TRUE, QUALIFIED PHYSICIANS? BUT THEN, WHY THEY HAVE INSISTED FOR ONE YEAR IN NOT USING QUALIFIED EXPERTS IN THE REVIEW OF PAPERS LR2111 AND LZ2206? WHY? HOW CAN THIS BE ONLY ACCIDENTAL?

REPORT OF REVIEWER No. 3

Title: Possible time-asymmetric model for open...

Author: Ruggero M. Santilli

This describes a novel attempt to understand time irreversibility in hadronic processes. This report addresses (a) the validity, (b) importance, (c) the interest of the paper for the readership of PRL.

As to the validity of the paper one cannot be categoric. The difficulty here is that the confines of the Letters' format means that the discussion is necessarily brief, and inevitably somewhat cryptic. This reviewer does not pretend to any special competence in the so-called non Birkhoffian mechanics. However, a possible difficulty arises: according to the algebraist Jacobson, an isotopic generalization of an associative algebra is in fact no generalization at all. In any event this reviewer feels that the algebraic generalization is interesting, but the relevance to time reversibility has not been adequately established--and indeed may not be able to be established within the confines of the Letters journal format.

As to the importance of the results: there is no question that the problem is one of great importance. However, the content of the paper appears to be, at this stage, largely speculative and not definitive.

In the opinion of this reviewer, the paper is almost completely inaccessible to the general readership of the PRL, and would lack interest for them. Part of this is the intrinsic difficulty of the subject and the general format, but part of it is also the fact that the paper is philosophic in tone, descriptive, and even at times verbose.

Since the experimental situation on the validity of the polarization assymetry theorem, ~~it~~ is at this stage controversial, with conflicting experimental evidence, and since the theory proposed here is at best tentative, and not definitive (the author takes pains to point out that there are an unlimited number of possibilities within the framework of his model (none of which is currently singled out)) it seems best to conclude that the publication of this paper is not advisable at this time.

Recommendation: Publication in the PRL is not recommended. It is suggested that the author prepare a longer, much more carefully crafted and explicit paper for a letters* journal.

* Apparently a slip, given the context. — Ed.

PART XIII–E:

CORRESPONDENCE

WITH

D. LAZARUS,

EDITOR IN CHIEF,

AMERICAN

PHYSICAL

SOCIETY

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 25, 1982

Dr. DAVID LAZARUS
Editor in Chief
The Physical Review and Physical Review Letters
1 Research Road
RIDGE, New York 11961

Dear Dr. Lazarus,

I am hereby asking your personal intervention in regard to my paper

"Use of the hadronic mechanics for the best fit of ....."

submitted to Physical Review Letters on April 19, 1982, ref. no. LR2111. A self-explanatory letter to the Editors is enclosed. In particular, I am asking your intervention to assist the Editors in the implementation of the request for two additional referee reports according to specifications (a), (b), and (c).

I would like to bring to your particular attention, the request that no referee from Harvard University, the Massachusetts Institute of Technology, and other local institutions are selected. The reasons are that there exists a rather considerable documentation regarding the opposition by academicians of these local institutions to the experimental, theoretical, and mathematical studies underlying the research presented in the paper. To give you an idea, I enclose copy of the formal prohibition by Harvard (enclosures # 1 and 2) to hold our Third Workshop on Lie–admissible Formulations there. Unfortunately for all of us, the meeting housed a considerable number of truly distinguished scientists (see the Table of Contents of the Proceedings, enclosure # 3). We, therefore, barely managed to avoid a public incident. I also enclose copy of the front page of an application for a federal grant regarding an experimental collaboration Austria–France–USA (enclosure # 4). As you can see, the application was signed in more than one Country, but it was NOT signed by MIT. I abstain from disclosing here the details as well as a number of related episodes. However, permit me to indicate that, again, we barely managed to avoid the appearance of the episode in the Foreign Press with a rather cold assessment of academic politics in the USA. Lately, we have seen the prohibition to list, in the Boston Area Physics Calendar, a seminar for physicists by a truly distinguished mathematician (enclosures # 5 and 6). The topic was the construction of the Lie–admissible groups via the use of changes of tolopogical coordinates. For your information, the Lie–admissible generalization of Lie algebras and groups is at the foundation of the time evolution

law suggested as possible in the paper [see equation (2)]. The prohibition implies a rather serious discrimination of research conducted under the federal support. I have, therefore, been advised not to provide you with additional disclosures at this time. Nevertheless, in case you are interested, I can ask the law firm in charge of the case to collaborate with you.

On more general grounds, the scenario of the situation in strong interactions is by far non—reassuring. It is an easy prediction that, unless our community of basic research manages somehow to contain the excesses of academic greed by physicists in position of power, a major crisis of unpredictable proportions will be unavoidable. It is a fact that the current scene is dominated by physicists committed to quark theories, their physical laws, and the underlying river of public funds. It is also a fact that these vested and organized scientific interests have provided systematic efforts to suffocate all possible searches for genuinely novel advances or alternatives. I am referring, here, to jeopardizing actions at the level of jobs, refereeing for grant applications, and submission of papers. To give you an idea, I enclose copy of a referee report (enclosure # 7) for a federal research proposal I submitted for my monographs "Foundations of Theoretical Mechanics," I, II, III. As you can see, the referee report consists of vulgarly offensive language combined with a total lack of scientific content. The point is that my manuscripts were accepted in the meantime for publication in one of the most selective series of research monographs in physics, that by Springer—Verlag. Understandably, vigorous complaints reached the highest possible levels in Washington, and I eventually provided my best efforts to avoid a scandal in the interest of our community.

But, bear in mind, these episodes are and remain "time bombs".

The situation at the Journals of the American Physical Society could predictably be a reflection of the scenario above. In fact, a segment of our community, as well as outside observers, are attempting to convey a growing concern on the conceivable manifestation of the problem at the editorial level. The scenario here is, essentially, an apparent, rather easy acceptance of papers on quarks, QCD, and related fields, joint with rather substantial difficulties experienced by all other papers of nonaligned character. I am myself the Editor in Chief of a Journal in Physics. Thus, I do favor the publication of all valuable papers in hadron physics, whether or not of quark alignment. Nevertheless, a few points should be made clear. On strict scientific grounds, quarks are at this moment a figment of academic imagination, without any experimental evidence comparable to that for the constituents of nuclei and atoms. In fact, all available evidence is in favor of the unitary classification of hadrons of Mendeleev type, but not necessarily of the desired, joint, structure model. Most importantly, you should keep in mind the growing concern for the lack of a rigorously established confinement of quarks. As you certainly know, we do not possess at this time explicit calculations proving that the probability of tunnel effects of quarks are identically (AND NOT APPROXIMATELY) null, while all so—called models of confinement are mainly qualitative. As a distinguished mathematician put it verbally to me,

> "The publication of a paper on quarks without a strict confinent by a journal in physics is equivalent to the publication of a paper on number theory by a journal in mathematics stating that 2 + 2 = 387.245693"

[I have denounced this situation to your Editors a number of times, in writing, apparently without any result whatever or containement of this historically paradoxical editorial case].

What is also distressing is the language in which these papers are generally written. In fact, the language is conveying the idea that quarks are truly real and established. Equally distressing is the feverish remanipulation of models to bring masses, parameters, etc., beyond the existing experimental capabilities. These, and numerous other episodes I prefer not to indicate here, are real reasons of concern for a fast growing segment of our community.

It is imperative that The Physical Review and The Physical Review Letters provide all necessary evidence and reassurance of being independent from conceivable lobbying by physicists of doubtful ethical motivation. The rules for achieving this are quite simple. Permit me the liberty of indicating them here.

SUGGESTED RULE ONE: Theoretical and experimental papers on quark conjectures, QCD, and related topics are plagued by increasing problematic aspects. It is essential that these papers experience exactly the same difficulties in publication as all other papers of nonaligned character.

EXAMPLE: As you can see from the official records of your Journals, the paper by Slobodrian, et al. [ref. 1 of the submitted paper] was submitted on August, 1980, and was published in December, 1981. Jointly, the Los–Alamos rebuffal [see the Note Added in Proof of my paper] was submitted in October, 1981 and was published in February, 1982. It is public knowledge that the former experiment is not aligned with current academic interests, while the latter experiment is. Also, and perhaps more significantly, it is public knowledge that the former experiment is substantially more general, accurate, and diversified than the second. In fact, the former is the result of a considerable and lengthly collaboration of experimentalists in the USA, Canada and West Germany that resulted in numerous measures for two reactions and their inverses. The latter experiment, instead, rushed four measurements only, on one reaction, while relying on the measurement by Slobodrian, et al, for the inverse reaction. Owing to these and other circumstances not disclosed here, I believe that the difference in the processing of these two papers was excessively imprudent. In fact, if the publication of the former experiment required sixteen months, the publication of the latter should have required a similar amount of time. At any rate, you should keep in mind that, in case of crisis, episodes such as this one might be investigated by appropriate senate committees. Your Editors could, therefore, be faced with requests of disclosing the referees' names and all refereeing proceedings to the investigating committee. By keeping this possibility in mind, it is imperative that similar differences be avoided at all costs in the future.

SUGGESTED RULE TWO: Referees should be experts in the field of the paper. This elemental rule does not appear to be necessarily applied in practice. In fact, papers on hadron physics are customarily referred to renowned experts in quark conjectures. The point is that these physicists usually have no knowledge whatsoever of research outside their beliefs. In short, being an established expert in quarks conjectures and related fields IS NOT necessarily a qualification for referring papers in hadron physics.

EXAMPLE: Please inspect the referee report of the paper submitted. You will immediately recognize the referee's total lack of knowledge in the experimental, theoretical, and mathema tical studies underlying the efforts to construct the "hadronic mechanics". I am referring to printed research pages now approaching the 10,000 mark, including over 10 volumes of proceedings of conferences, several research monographs, besides a large number of ordinary papers. I believe that, again, the selection of this referee has been excessively imprudent.

SUGGESTED RULE THREE: Referee reports should be examined for acceptance or rejection by using exactly the same criteria as those used for papers. More specifically, referee reports should be rejected when

(1) they contain offensive language

(2) they have manifest, ethically questionable, motivations; and, equally importantly,

(3) recommend acceptance or rejection without a clear technical content.

EXAMPLE: You can see the use of offensive language in the enclosed referee report for a research grant application. It should never have been accepted by the Federal Agency.

In closing, permit me the liberty of indicating, most respectfully but candidly, that I have contacted you for something substantially more important than the submission of a brief paper. In fact, what is ultimately at stake is the genuine lack of discrimination in governmental or private research during the editorial process at your Journals, as well as the genuine implementation of scientific freedom. In addition, there are clear national interests calling for the promotion, support, and pursuit of NOVEL physical knowledge.

Please intervene to prevent that excesses of academic greed create a dark permanent cloud in the beautiful history of the American Physical Society.

In the past, I have given more than sufficient proof of my committment to the orderly resolution of differences, and you can rest assured that the same committment shall persist in the future, of course, within limits set by ethics and human dignity.

If I can be of any assistance with more specific details, or in balancing excessively optimistic statements of quark–committed physicists, or in any other form, please do not hesitate to call me. You can count on my best and most loyal collaboration.

Very Truly Yours

Ruggero Maria Santilli
President of the IBR
and Editor in Chief, Hadronic Journal

RMS-mlw
enclosures: 1– Internal letter at Harvard University from Santilli to Hironaka dated April 25, 1980
2– Answer by Hironaka to Santilli dated May 2, 1980
3– Table of Contents on the Third Workshop on Lie–admissible Formulations under DOE support whose scheduled occurrence at Harvard had been prohibited
4– Front page of a research grant application under IBR administration for a joint AUSTRIA–FRANCE–USA collaboration that was not signed by the MIT representative
5– Letter by Santilli to the editor of the Boston Area Physics Calendar recommending the listing of a seminar by Professor A.A.SAGLE of the Department of Mathematics of the University of Hawaii at Hilo –May 19, 1982 [the listing was rejected]
6– Letter by Santilli to the chairman of the department of physics running the calendar, Dr. Schneps of Tufts University of April 27, 1982 asking for the listing of a seminar reviewing some recent problematic aspects of quark conjectures [this seminar too was not listed]
7– Copy of a referee report accepted by NSF on Santilli's grant application;
8– Copy of paper LR2111 submitted to Physical Review Letters
9– Copy of a paper outlining some of the problematic aspects of quark conjectures (Found. of Phys. vol. 11, p.383 (1981)) whose preprint had been distributed in 15,000 copies [this paper has never been quoted in the aligned quark literature to my knowledge]
10– Copy of the letter by Santilli to Trigg of May 25 suggesting implementation of due scientific process for paper LR2111
11– Copy of PRL referee report on paper LR2111
12– List of experts in the field of the paper for possible sole use of Dr.Lazarus as verification of PRL referees via independent consultations.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

(1)

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

April 25, 1980

Professor H. HIRONAKA
Chairman
Department of Mathematics

UNIVERSITY MAIL

Dear Professor Hironaka,

I acknowledge receipt of your recent note confirming the termination of my appointment on June 1, 1980, and indicating the possibility of my continuing to use the current office for a limited additional period of time (and definitely not beyond August 15, 1980).

For your information, and as a rather important part of my current research under DOE support, the THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS was tentatively scheduled in Cambridge (from August 4 to 9, 1980) several months ago.

The organization of this workshop is now close to completion. A list of participants is enclosed. In addition, we contemplate to have a number of distinguished guests (such as editors of physics Journals).

I assume you have no objection for having this scientific event at Harvard, and I am continuing the organization under this assumption.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
ecls.

c.c. Ass. Dean Leahy.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

(2)

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 2, 1980

Professor Ruggero Santilli
Department of Mathematics
Harvard University

Dear Dr. Santilli:

According to my letter of February 12, 1980 which you clearly received and acknowledged in your letter of April 25, 1980, your status at Harvard is to be totally ceased on May 31, 1980.

Therefore you have no right whatsoever to call for a meeting or conference, academic or otherwise, to be held on the premises of Harvard University after the date of the termination of your appointment, unless you were to obtain special permission from the appropriate administrative board of Harvard University. In any event, you have no authorization and no recommendation from our Mathematics Department for the Hadron Workshop to be held at the Science Center during the summer after May 31.

Sincerely yours,

Heisuke Hironaka
Chairman

HH/mjm

cc: Dean Richard G. Leahy

Enclosures

(3)

PROCEEDINGS OF THE THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS

Held at the New Harbor Campus of the University of Massachusetts in Boston from August 4 - 9, 1980

PART A : Mathematics, published in the
Hadronic Journal Volume 4, Number 2, February 1981

PART B : Theoretical Physics, published in the
Hadronic Journal Volume 4, Number 3, April 1981

PART C : Experimental Physics and Bibliography, published in the
Hadronic Journal Volume 4, Number 4, June 1981

The Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY

under contract number DE-AC02-80ER10651

HADRONIC JOURNAL

Volume 4, Number 2, 1981

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Held at New Harbor Campus of the University of Massachusetts in Boston
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* Corresponding participants

The Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY under contract number DE-AC02-80ER10651

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R. CALOI,* L. CASANA,* M.P. DE PASCALE,* M. MATTIOLI,* E. POLDI,* C. SCHAERF,* and M. VANNI* Universita degli Studi, Istituto di Fisica ed I.N.F.N., I-00185 Roma, Italy and
P. PELFER* and D. PROSPERI,* Universita degli Studi, Istituto di Fisica ed I.N.F.N., I-80138 Napoli, Italy and
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he Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY under contract number

)E-AC02-80ER10651

Research Grant Application

Submitted to the

U.S. DEPARTMENT OF ENERGY

(4)

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street
Cambridge, Massachusetts 02138
tel. (617) 864-9859

entitled

EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION

Proposed Starting Date:	Proposed Duration:	Amount Requested:
January 1, 1982	12 Months	$ 46,500

ENDORSEMENTS

H. Rauch
Principal Investigator
The Institute for Basic Research and Atominstitut
Cambridge, Massachusetts USA Wien, Austria
Tel. (617) 864-9859 Tel. (0222) 75 61 36

R.M. Santilli
Co-Investigator
The Institute for Basic Research
Cambridge, Massachusetts USA
Tel. (617) 864-9859

J. Summhammer
Co-Investigator
Atominstitut
Wien, Austria
Tel. (0222)75 61 36

A. Zeilinger
Co-Investigator
M.I.T. (and Atominstitut)
Cambridge, Massachusetts USA
Tel. (617) 253-4200

R.M. Santilli
President
The Institute for Basic Research
Soc. Sec. No. 032 46 3855
Tel. (617) 864-9859

Accounting Firm of the Institute
Vaccaro and Alkon CP, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
Att.: Mr. R. Alkon, President
Tel. (617) 969 6630

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
Att.: Mr. J. Grassia, Senior Partner
Tel. (617) 956-1700

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 19, 1982

5

Ms. CELIA MEES
Editor
Boston Area Physics Calendar
Tufts University
Physics Department
MEDFORD, Massachusetts 02155

Dear Ms. Mees,

Please list in the Calendar the following seminar

FRIDAY, APRIL 30

The Institute for Basic Research

2.30 - Harvard Grounds, 96 Prescott Street
(next to Fogg and GSD, entrance at the left court)
Algebraic identities, vector fields, and coordinate changes

Prof. [redacted], Univ. of [redacted], Dept. of Mathematics, and IBR, Division of Mathematics.

THE LISTING OF THIS SEMINAR WAS REJECTED

RMS.

Thank you.

Very Truly Yours

Ruggero Maria Santilli
President
RMS-mlw

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 27, 1982

Dr. JACK SCHNEPS
Chairman
Department of Physics
Tufts University
MEDFORD, Massachusetts 02155

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

(6)

Dear Dr. Schneps,

I am hereby asking that you list the following seminar in the Boston Area Physics Calendar for the week of May 16–21, 1982

WEDNESDAY, MAY 19

The Institute for Basic Research

2:30 p.m. — Enter at the left court of the Prescott House on Harvard Grounds at 96 Prescott Street, Cambridge (tel. 864 9859)
Experimental and theoretical reasons why I do not believe in quarks
Ruggero Maria Santilli, IBR, Division of Physics

Please note the following:

(1) This letter will reach you with plenty of time prior to the deadline for listings in the Calendar (1:00 p.m. Monday, May 10, 1982).

(2) In case the indication of the logistics of the Prescott House in the grounds of Mr. Harvard, to facilitate colleagues, is unwelcome, simply remove the words "Harvard Grounds".

(3) Following my conversation with Ms. CELIA MEES of April 19, 1982, and subsequent phone conversation with you on the same day, it is our understanding that you have accepted a formal request by the Chairman of the Lyman Laboratory of Physics at Harvard, Dr. KARL STRAUCH, as well as additional faculty there (apparently Drs. S. GLASHOW and S. COLEMAN, as well as others) not to list seminars organized by our Institute, irrespective of (a) the scientific status of the speakers; (b) its specific physical nature and (c) our conciliatory attitude toward the wording of the listings. You are therefore sharing with the indicated persons and institutions the responsibility of the act.

I urge you to withdraw from this apparent, scientifically insane behaviour, and list our seminars in exactly the same way as seminars are listed at your Department, Harvard, MIT and other local institutions, in the genuine spirit of the free pursuit of knowledge, as well as of this Land. I hope you understand the gravity of the gesture, and the reactions that, regrettably our Institute, as well as its numerous members scattered throughout the world, may be forced to implement.

Very truly yours,

Ruggero Maria Santilli
President
RMS/mlw

cc: Law Firm of the IBR
Board of Governors, IBR
All members of the Divisions of Physics and Mathematics, IBR
Ms. Celia Mees, Tufts Univ.

(7)

FORMAL REFEREE REPORT ON SANTILLI'S MONOGRAPHS "FOUNDATIONS OF THEORETICAL MECHANICS", VOLS I AND II, SPRINGER-VERLAG, IN PRESS, ACCEPTED AND RELEASED BY NSF OFFICERS.

I have examined the proposal by Dr. Ruggero M. Santilli PHY7703963 (returned under separate cover). My reaction to it is rather negative. I also thought that Santilli was on the borderline between being a third rate scientist and a crack pot and I do not think that the monumental work can change substantially my opinion. The idea of reading it thoroughly produces in me an incoercible revulsion and if you insist on it I am going to resign as a reviewer. The book is written in a pompous, immodest, self-glorifying style which I detest given also the absolute lack of physical content. In view of this criticism I find the total figure asked for the project quite extraordinary.

OVERALL RATING
☐ EXCELLENT
☐ VERY GOOD
☐ GOOD
☐ FAIR
■ POOR

NSF Form 173, Jan

PREPRINT OF THE INSTITUTE FOR BASIC RESEARCH
NUMBER DE–TP–82–9

USE OF THE HADRONIC MECHANICS FOR THE BEST FIT OF THE TIME–ASYMMETRY RECENTLY MEASURED BY SLOBODRIAN, CONZETT, ET AL

Ruggero Maria Santilli *

The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, U.S.A.

IBR reception date: April 14, 1982

Abstract

Strong nuclear interactions are assumed to have a non–Hamiltonian component due to contact among extended nucleons, which is represented via the hadronic generalization of the atomic mechanics currently under study by a number of authors. The theory is used for the description of the recent experimental discovery by Slobodrian, Conzett, et al. that the strong nuclear interactions violate the time–reversal symmetry. The fit of the experimental data provided by the hadronic mechanics is remarkable, and does not appear to be realizable via the use of the atomic mechanics.

* *Supported by the U.S.Department of Energy under Contract Number DE–AC02–80ER10651.A001*

Reprinted from FOUNDATIONS OF PHYSICS Vol. 11, Nos. 5/6, June 1981
Printed in Belgium

An Intriguing Legacy of Einstein, Fermi, Jordan, and Others: The Possible Invalidation of Quark Conjectures[1]

Ruggero Maria Santilli[2]

Received September 6, 1979

The objective of this paper is to present an outline of a number of criticisms of the quark models of hadron structure which have been present in the community of basic research for some time. The hope is that quark supporters will consider these criticisms and present possible counterarguments for a scientifically effective resolution of the issues. In particular, it is submitted that the problem of whether quarks exist as physical particles necessarily calls for the prior theoretical and experimental resolution of the question of the validity or invalidity, for hadronic structure, of the relativity and quantum mechanical laws established for atomic structure. The current theoretical studies leading to the conclusion that they are invalid are considered, together with the experimental situation. We also recall the doubts by Einstein, Fermi, Jordan, and others on the final character of contemporary physical knowledge. Most of all, this paper is an appeal to young minds of all ages. The possible invalidity for the strong interactions of the physical laws of the electromagnetic interactions, rather than constituting a scientific drawback, represents instead an invaluable impetus toward the search for covering laws specifically conceived for hadronic structure and strong interactions in general, a program which has already been initiated by a number of researchers. In turn, this situation appears to have all the ingredients for a new scientific renaissance, perhaps comparable to that of the early part of this century.

1. THE QUARK MODELS

Truly outstanding achievements have occurred in the study of the strongly interacting particles (hadrons) during the last decades. Beginning with the pioneering proposal by Gell-Mann[1] and Zweigh[2] of using the special

[1] Supported by the U.S. Department of Energy under contract numbers ER-78-S-02-4742.A000 and AS02-78ER04742.

[2] Department of Mathematics, Harvard University, Cambridge, Massachusetts.

0015-9018/81/0600-0383$03.00/0 © 1981 Plenum Publishing Corporation

(10)

April 19, 1978
Revised May 15, 1979

Preliminary draft

Any critical comment by interested colleagues for the finalization of this paper would be gratefully appreciated

AN INTRIGUING LEGACY BY ALBERT EINSTEIN:

THE EXPECTED INVALIDATION OF QUARK CONJECTURES

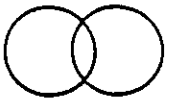
Ruggero Maria Santilli*

Science Center
Harvard University
Cambridge, Massachusetts 02138

* Supported by the U.S. DEPARTMENT OF ENERGY under contract number ER-78-S-02-4742.A000

This preprint has been printed and distributed to the scientific community by the Hadronic Press in 15,000 samples.

To be submitted for publication.



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 6, 1982

Dr. DAVID LAZARUS
Editor in Chief
The Physical Review and Physical Review Letters
1 Research Road
RIDGE, New York 11061

Dear Dr. Lazarus,

As a gesture of courtesy, I would like to inform you about recent developments concerning discovery of the violation of the time–reflection symmetry in

R. J. Slobodrian, H. E. Conzett, et al, Phys. Rev. Letters, **47**, 1804 (1981).

This information may also have some possible follow–up value in regard to my letter of May 25, 1982, to you.

1. You are aware about the following repetition of the experiment by R. A. Hardekopf, et al, in Phys. Rev., C25, 1090, (1982). Slobodrian and Conzett have found serious reasons to doubt the validity of the four measures conducted at Los Alamos. Copy of letters from Slobodrian to Veeser at Los Alamos are enclosed on a *confidential basis*. Experimentalists contacted by us have indicated that the apparent inconsistencies of the Los Alamos measures are truly sound.

2. The Québec–Berkeley experimental group has repeated again their measures and found values very close to the original ones. It appears that a communication by the experimentalists on these additional measures will be made publicly available in the near future.

3. Even assuming that they are correct, the four measures conducted at Los Alamos are not sufficient to establish the exact time–reflection symmetry. This point is treated in my paper submitted to Physical Review Letters on April 19, 1982, Ref. No. LR2111. Copy of an illustrated diagram is enclosed.

In addition to the direct information, you should also keep in mind the considerable amount of indirect information supporting the violation of the time–reflection symmetry under strong interactions.

I am referring here, for instance, to:

a. The available measure by Rauch's experimental team on the apparent deformation of the charge distribution of neutrons in the field of nuclei. As you know, the underlying rotational—asymmetry, if confirmed, will imply a necessary violation of the time symmetry. Copy of a paper by Rauch is enclosed.

b. An increasing number of theoretical studies indicate the existence of new, rather substantial, problematic aspects in the relationship between the experimentally established macroscopic irreversibility and the conjectural particle reversibility. These problems were studied at our recent International Conference at Orleans [see, for instance, a paper by Tellez—Arenas]. It is clear that the best resolution of this historical problem is that along the experiment by Slobodrian, Conzett, et al.

c. An additional array of problematic aspects is currently surfacing for a joint time—reversal symmetry combined with the established, broken space—reversal symmetry. I am referring to inconsistencies in the structure of the Special Theory of Relativity. After all, Einstein taught us the equivalence of space and time, and Dirac has stressed, since 1949, his expectation of a joint space—asymmetry and time—asymmetry.

Finally, I believe you should be informed that the NOBEL COMMITTEE in Stockholm, has apparently initiated the monitoring of the scientific events that are expected to unfold in the near future in regard to the time—asymmetry. This is the result of a world—wide wave of independent recommendations to the NOBEL COMMITTEE for the appointment of Professors Slobodrian and Conzett as NOBEL CANDIDATES. I enclose copies of letters of recommendations that have reached Stockholm in the past few months.

I hope that this information is of value to you and to your editors.

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

Enclosures

cc: Editor of Physical Review A,B,C, and D
and Physical Review Letters

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GÉNIE
CITÉ UNIVERSITAIRE
QUÉBEC P.Q. CANADA
G1K 7P4

16 February 1982

Professor Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachussetts 02138
USA

Dear Professor Santilli,

Thank you for your letter of February 8. 1982. Please find enclosed a copy of the letter I have sent to Dr. Robert Hardekopf concerning the Los Alamos experiment. It is my belief that they did not have sufficient energy resolution to separate the transition to the ground state in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction.

I am enclosing a list of references which may prove useful and pertinent to the general problem of time asymmetry. However, I would personally be inclined to look closely at spin-dependent effects, i.e., for example polarizations and analyzing powers: The crucial formulae for the observation of a spin 1/2 particle are

$$A_j = \frac{tr(T\sigma_j T^{\dagger})}{tr(TT^{\dagger})}$$

and

$$P_j = \frac{tr(\sigma_j T^i T^{i\dagger})}{tr(T^i T^{i\dagger})} \quad .$$

It is required that $T^i = T^{\dagger}$ for the validity of the polarization analyzing power equality. However, the theorem may breakdown for other reasons. For example, behind the formalism there is the assumption of operator linearity. Hermiticity of operators corresponding to observables is also implied. The SU(2) exact symmetry is also basic to enunciate the formal expressions for polarizations and analyzing powers. Hence a breakdown of this symmetry may entail an essential

.../2

breakdown of the theorem. As you have shown if such were the case one would face also an essential time asymmetry in hadronic processes.

There are other delicate points which I do not feel qualified enough to discuss in depth: the interference of the long range electromagnetic field with the hadronic field and the general implications of Lorentz invariance, space time structure, etc., for nuclear reactions. Causality violations in quantum systems may also introduce irreversibility effects. I enclose a copy of some pages from the book by Davies, in case you have not seen it yet, dealing with time asymmetry.

In closing, I would like to stress once more the point made at the Orléans conference: The sensitivity of spin-dependent effects to time asymmetry is high, hence the observed P-A difference may stem from rather modest causes.

With best regards.

Cordially,

RJS:dcv

R.J. Slobodrian

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GÉNIE
CITÉ UNIVERSITAIRE
QUÉBEC, P.Q., CANADA
G1K 7P4

8 February 1982

Dr. Robert A. Hardekopf
Los Alamos National Laboratory
Mail Stop 480
Los Alamos, New Mexico 87545
USA

Dear Bob:

As I wrote to you in December, we are now running once more on $^{9}Be(^{3}He,\vec{p})^{11}B$, with our new Si-polarimeter system. Our work has been somewhat slowed down by the breakdown of the van de Graaff belt and other (minor) problems. Nevertheless, our values with the new system thus far agree with all of our Si-polarimeter results, hence they continue to disagree with yours.

I have then studied your preprint and your NIM 114 (1974) 17 paper in detail. The latter shows calibrations with a 100μ and 300μ passing detector. However in your recent work on (t,p) and (^{3}He,p) you used a 500μ passing detector. Is it right to construe from your fig.2 of NIM that the analyzing power drops dramatically at about E_p = 11 MeV for 500μ?

You have tested target thickness effects with the 17 MeV triton beam, changing the ^{12}C target from 1.9 to 4.9 mg cm^{-2}, that gives ΔE = 100 keV and ΔE = 300 keV respectively. However, the energy spread of 14.3 MeV ^{3}He on a 4.7 mg cm^{-2} ^{9}Be target is ΔE = 1400 keV, about 4.5 times greater. Also r.m.s. multiple scattering effects are considerably higher. I am doubtful that this test could have given adequate information.

Referring now to your figure 5a) the arrows include a peak in your passing Si detector. From the text it is implied that it corresponds to the ground state peak of ^{11}B. However, I have calculated the ratio of ΔE's from the ground state and first excited states in a 500μ Si detector following the Ta and steel degraders.

.../2

I obtain $(\Delta E)_1/(\Delta E)_0 = 1.14$. The ratio for the centroids of your two peaks is 1.30, i.e., percentage wise there is 14% against 30%, a factor of two discrepancy.

Taking into account your 1.4 MeV spread due to the thick ^{9}Be target, about 0.2 MeV from finite kinematics, some 0.3 MeV from multiple scattering and 0.3 MeV from energy straggling, it seems impossible to separate cleanly, in a 500μ detector, the ΔE pulses from states 2.1 MeV apart, with 22 MeV incident energy. In fact I would say that it is impossible, we have our on-line accumulation of ΔE vs E_{Total}, with 2.7 mg cm^{-2} target and a 1000μ Si detector (without degraders) and there is no way of separating the ΔE peaks. It seems to me that your peak is the sum of the ground state and first excited states transitions. The second peak may be a residue of the doublet near 4.7 MeV excitation.

I would be grateful if you could look into the above points. It turns out that if the polarimeter were analyzing a composite peak of the ground and first excited states, the effective analyzing power should be lower than -0.63, and might change drastically with kinematic effects as a function of angle, particularly because the X-section of the first excited state is at least a factor of two larger than that of the ground state. In your tests with the ^{12}C target the situation is vastly different. The incident proton energy after the degrader is about 15 MeV and the first excited state of ^{14}C is at about 6 MeV.

With regards,

RJS:dcv

R.J. Slobodrian
Physics Department

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GÉNIE
CITÉ UNIVERSITAIRE
QUÉBEC P.Q. CANADA
G1K 7P4

April 20, 1982

Dr. Lynn Veeser
Los Alamos Scientific Laboratory
Mail Stop D410
Los Alamos, New Mexico 87545
USA

REF: p-14-82-U-163

Dear Dr. Veeser:

I am writing to you again concerning your helium polarimeter experiment. It would be helpful to me to have a detailed large scale drawing of it. In particular, to know the exact position of the 500 μm of Si of your passing detector and the diameter of it, i.e. the diameter of the active surface presented to the protons.

As I have commented before to Bob Hardekopf, the Ta degrader introduces a large r.m.s. multiple scattering angle to the proton beam. The polarimeter calibration, however, was carried out with a polarized beam, quite parallel, without degraders. The analyzing power of the polarimeter depends critically on the range of angles of the scattering off helium. Such range, for 62% of the protons when degraded by 587 mg cm^{-2} of Ta, is increased considerably, and it is no longer defined by the copper vanes to $\pm 7.5^{\circ}$. I obtain $\pm 16^{\circ}$. A quick calculation then gives a much lower analyzing power for your polarimeter.

Finally, it seems to me that the passing detector spectrum shown in your paper is ungated. I would be thankful if you could provide me with a coincidence spectrum of your passing detector with your side detectors. It is this spectrum which is crucial to determine the degree of separation of the ground state and 1st excited state in your experiment.

Sincerely yours,

R.J. SLOBODRIAN
Van de Graaff Laboratory

RJS:dcv

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GÉNIE
CITÉ UNIVERSITAIRE
QUÉBEC P.Q. CANADA
G1K 7P4

3 June 1982

Dr. Lynn Veeser
Los Alamos National Laboratory
Mail Stop D410
Los Alamos, New Mexico 87545
USA

Re: P-14-82-U-220

Dear Dr. Veeser,

Many thanks for your letter of May 21 and enclosed information. Peak fitting on your passing detector spectra for the 4.7 mg cm^{-2} Be target indicates to me that you may have 10% of the number of counts assigned to the ground state peak, coming really from the first excited state. You mention also radiation damage, if would be relevant to know your separation at the end, before changing detectors, as such damage results in low energy tails.

In my letter of May 20 (copy enclosed) I had asked the exact position of the 500 µm Si detector (passing detector) and / or a large scale drawing of the polarimeter. Is this information available? It is impossible to ascertain this from the NIM paper.*)

I have looked again at your published L and R detector spectra (I say again because last year I wrote to Bob about them). It seems to me that your procedure to account for backgrounds is not proper. The reason is simply that you have slit scattering and multiple scattering, this means that you have background particles that are real events from the point of view of a TAC as determined by your conditions. One can see this clearly in your figures 5d) and e). I have subtracted backgrounds by looking at the level "far" from the peaks. The asymmetry is $\varepsilon = -0.237$ which together with your value for $A = -0.63$ results in $P = 0.38$, to be compared with $P = 0.275$ obtained using your method, relying on accidental coincidences, which I believe is improper. The polarization value is increased by 50% with the alternative background subtraction.

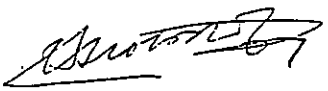
*) The NIM paper has a rectangle: ☐ which is the 500 µ thickness ?

.../2

Concerning the problem of the effective analyzing power of your polarimeter I have to disagree with your assessment of the effects of angular spread due to multiple scattering. In fact your scattering region is quite short, however, by the same argument you use, the r.m.s. scattering angle privileges the first vanes over the last ones. I have calculated $A_{eff} = -0.50$ for your polarimeter with 587 mg of tantalum. This again would increase P for your publised spectra to P = 0.48. Now, Bob explained to me that the peaks in the preprint (and publication) were obtained with the polarimeter at one side of the beam. If we now take your published average at 45^{o}, P = 0.165, and correct it in the same way the final result is P = 0.29 ! This value is certainly consistent with our own.

I would be grateful to receive your comments on the above points and the information requested.

Sincerely yours,



RJS:dcv

R.J. SLOBODRIAN
Van de Graaff Laboratory

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

January 19, 1982

Professor BENGT NAGEL
THE ROYAL SWEDISH ACADEMY OF SCIENCES
NOBEL COMMITTEE IN PHYSICS
P.O.Box 50004
S-STOCKHOLM, SWEDEN

Dear Professor Nagel,

I am taking the liberty of enclosing a copy of my recommendation submitted to the NOBEL COMMITTEE on the same date, suggesting the consideration of Professors R.J.SLOBODRIAN (Canada) and H.E. CONZETT (U.S.A.) as candidates for the Nobel Price in Physics of 1982.

The primary hope of the enclosed recommendation is that the NOBEL COMMITTEE initiates a monitoring of the scientific events that are expected to unfold in the underlying, truly fundamental aspect of contemporary physics, the possible origin of the irreversibility of our macroscopic world in the most elementary structure of matter, that of the strong (nuclear) interactions.

In case of interest by the part of the NOBEL COMMITTEE, I would be glad to cooperate to my best, and in the most confidential form possible, by providing all relevant information that is expected to materialize in the future years in the case.

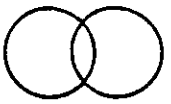
Thank you for your consideration and time.

Most Respectfully Yours

Ruggero Maria Santilli

RMS-vf

P.S. A number of colleagues from Europe, South America, North America and Australia have recently contacted me indicating their desire to submit a similar recommendation to the NOBEL COMMITTEE. It is my understanding that these independent letters, either have already reached Stockholm, or are in the process of arriving there.



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

January 19, 1982

NOBEL COMMITTEE FOR PHYSICS
OF THE ROYAL ACADEMY OF SCIENCE
Sturegatan 14
S–11436 STOCKHOLM, SWEDEN

Honorable Committee,

I am taking the liberty of recommending

Professor R.J. SLOBODRIAN
Laboratoire de Physique Théorique
Université Laval
QUÉBEC G1K 7P4, Canada

and

Professor H.E. CONZETT
Lawrence Berkeley Laboratory
The University of California
BERKELEY, California 94720

as CANDIDATES FOR THE NOBEL PRIZE IN PHYSICS FOR 1982.

My recommendation is based on the recent discovery by Professors SLOBODRIAN and CONZETT regarding the violation of the T–symmetry in nuclear physics, as announced in their recently published article

> *R.J. SLOBODRIAN, H.E. CONZETT, et al, "Evidence of time symmetry violation in the interactions of nuclear particles", Phys. Rev. Letters 47, 1803 (1982).*

I recently had the privilege of listening to an invited talk by Professor SLOBODRIAN delivered at the

> *FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE–ADMISSIBLE TREATMENT, held at the Département de Physique de L'Université d'Orléans, France, from January 5 to 9, 1982.*

Several additional talks by distinguished speakers in related fields were also delivered at this Conference. As a result of these and other circumstances, I believe that the discovery by Professors SLOBODRIAN and CONZETT is of truly fundamental physical relevance, with implications perhaps even greater than those of the discovery of the P–violation. I provide below a brief elaboration of the most salient aspects, while I remain at your disposal for a detailed and technical presentation.

HISTORICAL SIGNIFICANCE. The physical reality of our environment provides unequivocal evidence that macroscopic phenomena violate the T–symmetry. The structure of atoms, on the contrary, has resulted in verifying the T–symmetry. It has therefore often been assumed that the symmetry is also valid for elementary particle at large. This has lead researchers to attempt the interpretation of the macroscopic irreversibility via a large collection of elementary particle processes, each of which is reversible. None of these attempts has been able to overcome the numerous inconsistencies inherent in the problem, and to achieve acceptance by the scientific community at large. Jointly, we have seen an increasing number of authoritative studies stressing that the most natural interpretation of the macroscopic irreversibility is that it originates at the level of elementary particles and their interactions. The discovery by Professors SLOBODRIAN and CONZETT provides a resolution of this historical problem which, for a number of technical reasons I cannot review here, is apparently final.

PHYSICAL SIGNIFICANCE. As we know well, the violation of the P–symmetry was incorporated in physics without foundamental changes in the mathematical structure of the theoretical formulations. The discovery of the violation of the T–symmetry appears to have much deeper implications. The T–symmetry is at the foundation of dynamics inasmuch it is at the foundation of the time evolution. The discovery of the T-violation may therefore imply a revision of the fundamental dynamical equations of contemporary physics. For instance, according to specialized literature in the field, the forces which appear to be responsible for the breaking of the T–symmetry are the non–potential, non–Hamiltonian forces originating in contact phenomena, such as the mutual penetration of the wave packets of hadrons under the conditions of the strong interactions, the collision of molecules in statistical ensembles, etc. The ordinary Quantum Mechanics, since it is essentially Hamiltonian in character, is potentially unable to represent the type of T–symmetry breaking under consideration. Also, recent advances in the study of symmetry breaking have lead to the understanding that a Hamiltonian (total energy) can be conserved and invariant under a given discrete or connected symmetry, while the underlying equations of motion violate the symmetry. These and other occurrences have suggested the attempt to generalize Quantum Mechanics into a form specifically conceived for the strong interactions, which is now under study by an increasing number of mathematicians and physicists under the name of Hadronic Mechanics. A significant hope of these efforts, beginning with the T–violation, is to achieve knowledge which is relevant to controlled fusion.

MATHEMATICAL SIGNIFICANCE. The mathematical implications of the discovery by Professors SLOBODRIAN and CONZETT are equally far reaching. Simply stated, the discovery can provide a crucial impetus to the generalization of Lie's theory, e.g., of the Lie–Admissible type which is already under study by a number of pioneering mathematicians, and which is the mathematical structure of the Hadronic Mechanics. In the simplest possible terms, Heisenberg's time evolution can be seen, from a mathematical viewpoint, as a two-sided Lie module, one module for each direction of time. Quantum mechanics is then structurally T–symmetric in the sense that time reversal essentially map one module into the algebraically equivalent other. When the time evolution is realized according to the covering, Lie–admissible, two-sided modules, one reaches a theory which is intrinsically T–noninvariant irrespective of the invariance properties of the Hamiltonian, inasmuch time reversal maps each module (each direction of time) into an algebraically different module, thus resulting into irreversibility of processes under unrestricted forces. It should be noted here that the two–sided Lie–admissible modules (or other mathematically equivalent structures) demand a generalization of the virtual entirety of Lie's theory, from the enveloping algebras, to the Lie groups, to the representation theory, etc. The implications for the development of mathematics as well as physics, are then self–evident.

The historical, physical, and mathematical aspects indicated in this letter have been discussed in detail at the recent Orléans Conference, and are recorded in the Proceedings currently in print. Additional pertinent material is available from the Proceedings of the WORKSHOPS ON LIE–ADMISSIBLE FORMULATIONS held here in Cambridge–U.S.A. from 1978 to 1981, as well as from specialized literature in statistical mechanics and other disciplines.

In case this Honorable Committee desires more technical and detailed information regarding my personal recommendation for Professors SLOBODRIAN and CONZETT being CANDIDATES FOR THE NOBEL PRICE IN PHYSICS OF 1982, please let me know. It would be a pleasure to prepare a more detailed technical presentation, possibly with the assistance of other experts.

Hoping that I did not abuse of your courtesy and time, and thanking for your consideration, I remain

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics

RMS–vf

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

July 21, 1982

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

RE: LR2111: "Use of hadronic mechanics..."

Dear Dr. Santilli:

I am sorry to be so delayed in replying to your letter of May 28, but I wanted to have the time to review the complete file on your paper at the editorial office before attempting to understand the situation. I make only one trip a month to Ridge, in general, so that delays are sometimes inevitable.

As you know from your personal experience as a journal editor, strict criteria for acceptance or rejection of papers have to be established and rigorously maintained, or else the system would have no valid claim to objectivity. By very long tradition, all papers submitted to any of the Physical Review journals, whether from Nobel laureates or complete unknowns, must be referred to independent, expert referees selected by the Editors who must recommend acceptance of papers before they can be published. Our editors, while fine physicists themselves, cannot be expert in all fields of physics and must rely on the advice of outside experts to perfect papers submitted (which are rarely acceptable in precisely the original form) and to reject those papers which are unsuitable for our journals. The referees need not disprove the contentions of a paper to disapprove its acceptance; rather, the burden is on the authors to convince the referees that the paper is acceptable. Clearly, if a paper is not comprehensible to an expert referee, it will not be useful to a less well informed reader. No exceptions are ever made to this procedure, but authors are permitted to exclude certain specific referees, if they so choose.

In the case of your paper, in your initial submission which was received on April 19, 1982, no mention was made about excluding any specific referees, and the paper was routinely submitted to two physicists of considerable eminence for comment. One rejected it out of hand and the second wrote a rather detailed review which was sent to you. Your reply of May 26, together with earlier correspondence was sent to two additional referees, one of whom gave a detailed comment, but did not recommend acceptance of the paper. On the basis of all comments received from referees, the editors had no choice but to reject your paper.

In your letter to me on May 26, you requested that no referees from Boston area institutions be consulted about your paper, a rather large exclusion and one not mentioned earlier. By sheer chance, none of the earlier referees were, in fact, from Boston area institutions, none expressed any familiarity with you personally, and there is not the slightest reason to suspect that there was any personal animus in their appraisals of your paper. Thus, even by the post hoc rules of the game set by your letter of May 26, your paper received an eminently fair hearing and was rejected on objective grounds. No further consideration is merited.

I assure you that, however popular "quark theories" of elementary particles may appear to be, the theorists who expound such models are not an "establishment" which runs the American Physical Society or its journals. I am an experimental solid-state physicist myself and recognize no formal hierarchy in physics which could provide the "right" answers. Physics, by its very nature, is and ought to be contentious. We do not shirk from publishing controversial papers. As for your three "rules" for our journals, they correspond, in fact, to our current procedures: all experimental and theoretical papers whether based on quark models or otherwise, receive precisely the same refereeing procedure, hence the same "difficulties in publication"; your referees were, in fact, experts and physicists of great eminence whose opinions must be respected; referee reports are, in fact, all examined for any signs of personal animus and are rejected for the reasons you mention. I believe that our current editorial procedures, while possibly not perfect, are completely honest and objective and have resulted in our journals maintaining their reputations as the world's best.

Sincerely yours,

David Lazarus
Editor-in-Chief

xc: R. K. Adair
G. L. Trigg

VERY CONFIDENTIAL

September 10, 1982

Dear Dr. Lazarus,

I shall comment on your letter of July 21, 1982 sometimes in the near future in a formal way.

This note is to keep you informed that the continual rejection of my paper has forced us into a first step. In fact, I shall be in Washington on September 14-15-16, among other reasons, to consult with appropriate observers on what we consider needed to bring back the journals of the APS into the genuine fulfillment of national interests via the free pursue of truly novel advancements in physical knowledge. A variety of options will be discussed ranging from graceful acceptance, to the release to the international press of documented views of the situation.

You must understand that, like all other physicists, I had many papers rejected in my life and I have accepted them with grace. This time the situation is different.

An entire new mechanics has been constructed, the Birkhoffian mechanics, without one single paper appearing in journals of the APS. In fact, my monograph reviewing this achievement is just about to be released by the printer. Inspection of the references is then a silent but unequivocal identification of this very grave episode. The reason is simple and it is the usual one: referees have opposed such achievement to the point of disgusting reputable authors.

According to all indications, it appers that established academic interests have decided to repeat the exploit. I am referring to the construction this time of the hadronic mechanics [which is at a rather advanced stage already] again without one single paper appearing in the journals of the APS.

But the the construction of new theories capable of treating non-Hamiltonian systems [such as the Birkhoffian and the hadronic mechanics] is an important part of national interests [you should recall that all military systems are non-Hamiltonian], while the same theories are strictly outside personal interests of contemporary academicians.

We have therefore reached the delineation of all the necessary prerequisites for the typical case of direct conflict between national interests for the pursue of novel physical knowledge, and vested academic interests that are against such a pursue.

Graceful acceptance of such a situation then becomes an unequivocal indication of complicity. To be able to keep looking at our children with clear eyes we need a vigorous opposition, and the undertaking of all the necessary steps to eliminate this totalitarian conduction of research, and the restoration of the genuine freedom in scientific inquiry.

The problem at your journals is incontrovertibly documented by now: valuable research efforts must be published, particularly when dealing with aspects of fundamental character. Criticisms to the same papers should equally appear in print, when valuable. This is the ONLY way to pursue novel knowledge via a free scientific process. When entire new mechanics are built (and this happens only occasionally per each century!] and not a single paper appears in your journals, you have a problem.

This is the land where my children will live. I intend to dedicade my life to its future well being at whatever personal price. You should never doubt about my determination, and not to confuse my preceding gracefulness with weakness.

Sincerely
Ruggero Maria Santilli

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

September 27, 1982

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I am in receipt of your recent correspondence regarding your paper submitted to Physical Review Letters, LR 2111, "Use of the hardonic mechanics....."

You ask that I "intervene in favor of publication." You surely understand, particularly since you are editor of your own journal, that I cannot intervene in this manner for anyone in the world, when referees have not recommended acceptance of your paper. As I wrote to you earlier: no exceptions are ever made to the criterion of acceptance by impartial referees before a paper may be published in any of the archival journals of the American Physical Society (only the Bulletin of the American Physical Society publishes author-submitted abstracts without referral of any sort). Despite your strong statements to the contrary, referees have not been able to see sufficient merit in your paper to recommend its acceptance, even with revision. Accordingly, by our long-established rules for acceptance, your paper cannot be accepted.

You have expressed concern that there may be some sort of "conspiracy" against your work to suppress your opus, organized by "quark-committed physicists." As I wrote to you earlier, I know of no such cabal, nor would I tolerate it. To convince myself, if not you, I sent your paper without comments from prior referees or your rebuttals to yet another physicist, one who is clearly not committed to quark models. The reply was similar to the previous ones: there is not sufficiently original or important contributions to physics in your paper to merit publication- - the mere fact that your Equation (10) relates to a single experiment is not sufficient, without also demonstrating that it is not in disagreement with all other experiments and has specific predictive power for experiments not yet performed. Mathematical elegance is not equatable with important physics.

I am sorry, but the Editor's rejection of your paper, based on several referees' reports, must stand.

Sincerely yours,

David Lazarus
Editor-in-Chief

xc: G. L. Trigg

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

October 12, 1982

Ruggero Maria Santilli, Professor of Theoretical Physics and President

Dr. DAVID LAZARUS
Editor in Chief
The Physical Review and Physical Review Letters
1 Research Road
RIDGE, New York 19961

RE: Paper LR2111 submitted on April 19, 1982, to PRL entitled "Use of the hadronic mechanics for the fit of the time–asymmetry recently measured by Slobodrian, Conzett, et al" by R. M. Santilli (IBR preprint no. DE–TP–82–9

Dear Dr. Lazarus,

I would like to acknowledge your kind letters of July 21 and September 30,as well as our phone conversation of this past Thursday.

Permit me to stress from the outset that I have nothing but sincere gratitude for you and for Dr. G. L. TRIGG at PRL, not only for the time devoted to the case, but also for the courtesy of keeping me informed.

However, I feel obliged to express my reservations on the referees and on their selection. In the hope of contributing toward the continuation of our communications, I would like to summarize the case as seen from our profile.

STATUS OF PAPER. I understand you have accepted my moderate proposal to the effect of pausing for a couple of months in the consideration of this paper. This would give time to your editors to consider the experimental paper recently submitted to PR–C by Slobodrian et al on the repetition of the measures on the time–asymmetry, while giving me time for improving the paper to my best. Subsequently, I shall submit a revised version for one final review.

I would also like to reinstate that the submission to Phys. Rev. Letters is of mere indicational character, and that the possible consideration/publication of paper LR2111 by Phys. Rev. D, or Phys. Rev. C (say, as Rapid Communication) would be equally acceptable to us.

In fact, our primary objective is to have your Journals participate in the current laborious efforts to generalize quantum mechanics for extended particles. For this task, the selection of Phys. Rev. Letters, or Phys. Rev. D, or Phys. Rev. C, would be equally welcome.

YOUR INTERVENTION. Permit me to stress that I have not asked for your intervention to have my paper published. If I gave you this impression, please accept my apologies, while I assume all responsibilities. I have asked for your intervention to ensure due scientific process, that is, to ensure that the paper is subjected to a serious review by experts in the field, and that *a possible final rejection is motivated by errors, inconsistencies, and/or incompatibilities clearly identified and presented in the due scientific language.* I have insisted for this due scientific process in this case (but not in other cases in the past), because of a number of particular circumstances ranging from certain, unfortunate, preceding occurrences, to the number of observers monitoring the case, and to the negative implications for your Journals, as well as for the American Physical Society in case of unprofessional refereeing.

LACK OF CREDIBILITY OF AVAILABLE REFEREE REPORTS. I have seen reports only by two referees. The first was so unprofessional, to force the raising of ethical issues, as anybody can see from statements to the effect that "I do not know the Hadronic Journal that published the preceding literature, and, therefore, I recommend rejection". Besides all the hardly believable aspects reported elsewhere, this referee did not even understand the most crucial deficiency of the rebuffal to the Slobodrian–Conzett paper by Hardekopf et al. I am referring to their repetition of ONLY HALF of the measures–those of the polarization only–while relying on the measures by Slobodrian, Conzett

et al on the remaining measures--on the analyzing power--(see below for additional comments).

The second referee also forced the raising of ethical issues, contrary to our best predisposition. In fact, he insisted in the rejection of the paper via arguments based on quark conjectures on electroweak decays, while the paper deals with certain nuclear reactions involving the exchange of two nucleons.

To understand the case, you must understand the surprise of a number of observers to see that PRL took seriously reports of this type, while they should have been returned to their authors with the request to do better homeworks before implicating Journals of the APS in their personal dances.

Also, the claim that the paper is "mathematical" can do nothing but confirm doubt on the existence of politics in this case. In fact, the paper is entirely devoted to THE INTERPRETATION OF AN EXPERIMENT. Additional shadows of questionable scientific practice are created by claims of lack of originality. In fact, the paper deals with nothing less than a GENERALIZATION OF QUANTUM MECHANICS! How can you expect that physicists nowadays accept such distorsions of reality?

But the statement that creates the highest concern is that the paper must be in agreement with all available experimental information. In fact, when translated in plain language, the statement implies the suppression of all possible attempts at your Journals to pursue truly novel physical knowledge. In fact, to reach one single paper verifying criteria of such extreme exigency one should work for a decade, and write a few thousand pages of research.

Par contre, your Journals publish with considerably easiness a large number of papers based on the assumption that there exist 36 (or so) unidentified quarks, subject to a still doubtful confinement, under the additional hypothesis that ....., etc., etc., etc.

Under the conditions of such extreme disparities, the shadows of partisanship at your Journals with established academic interests is then unavoidable. In turn, this raises a host of rather serious problems I pray you will not overlook.

THE ERRORS IN THE REFEREE SELECTION. While the basic rule of ethically sound editorial practices is the scientific credibility of the report, its prerequisite is the selection of referees who are experts in the field. For instance, the papers on quarks published in your Journals have been ALL refereed by experts in quarks. In case you can document ONE exception, please make it public, because it would help considerably this case.

It is evident that the handling of my paper has violated this other fundamental rule. In fact, the lack of any meaningful knowledge by the referees of the topic is manifestly transparent. You must understand that I am referring to a rather voluminous mathematical, theoretical, and experimental literature that constitutes the foundation of the current efforts to generalize quantum mechanics, for over 10,000 pages of published research.

The proof is simple and incontrovertible: HAS ANY OF THE SELECTED REFEREES PUBLISHED EVEN ONE SINGLE PAPER ON CONTACT—NONHAMILTONIAN INTERACTIONS? If not, the only way for your Journals to dissipate allegations of partisanship, is to start sending papers on quarks (including electroweak theories) to reputable quark nonbelievers (there are quite a few!).

It appears that the referees have been selected on the mere basis of their "good standing" at your Journals in complete disregard of their knowledge of the topic. Again, this disparity of editorial practices in the transition from fields aligned with established scientific interests to others creates sizable problems.

PRECEDING UNFORTUNATE INSTANCES. As is well known in informed circles, the way PRL handled the experimental paper by Slobodrian, Conzett et al (PRL 47, 1803 (1981)) has caused considerable concern. One reason is that the paper was kept for an excessively long period of time, and was finally published only after academic groups of vested, opposing interests had sufficient time to hurry a counter—experiment, and have it quoted in the original paper by Slobodrian, Conzett, et al.

By comparison, the rebuffal was published with such a rapidity, to be truly surprising.

I believe that the difficulties experienced by the first paper, compared to the lack of difficulties experienced by the rebuffal have caused a considerable damage to your Journals, as well as to the American Physical Society. This is a fact, whether you accept it or not. To understand it (as well as to have an idea of the talks on the subjects in academic corridors thoughout the world) you must understand that, while the first paper was the result of a serious experimental work over several years by a number of experimentalists in three Countries (U.S.A., Canada, and West Germany), the rebuffal

(1) was rushed in a period of time too short to constitute final work;
(2) was written in a transparently political language (in fact, it claimed the lack of time—asymmetry, while simple calculations show clearly that the four countermeasures can accommodate an infinite variety of curves of polarization all different than those of the analyzing power).
(3) was based on the repetition of only HALF measures, as indicated earlier.

The Phys. Rev. C has recently received the submission of the new measures by Slobodrian et al. *I pray God that this paper is treated in exactly the same way as the Los—Alamos one, and that your editors will see the implications for a continuation of a disparity in the editorial processing of papers aligned and nonaligned with existing academic interests.*

OBSERVERS MONITORING THE TIME—ASYMMETRY. I brought to your attention the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS we held on January, 1982, at the Université d'Orléans, France, under support of the French Government, with some four volumes of proceedings, and participants from virtually all developed Countries. The conference studied in detail the experimental, theoretical, and mathematical aspects of the time—asymmetry, beginning at the classical Newtonian level, and then passing to the statistical, and to the nuclear—particle profile.

Paper LR2111 constitutes a relevant expression of this conference. Therefore, your final decision will be monitored, not only by the participants to the Conference, but also by all scholars throughout the world who are interested in a credible resolution to the vexing, historical problem of the origin of irreversibility.

I feel obliged to bring to your attention the additional fact that, following the International Conference, numerous scholars recommended Professor Slobodrian and Conzett to the Nobel Committee. Contrary to what you may hear from physicists who would be damaged by a confirmation of the time—asymmetry, it appears that some form of monitoring has been implemented by the Nobel Committee in this case.

I pray that your Journals, as well as the American Physical Society, will not come out of this case with the "dark shadow" that suggested my contacting you in the first place.

Finally, we still have additional observers that I prefer to keep confidential at this time in the best interests of all.

CANDID CONSLUSIONS. Permit me to express the essence of the case, most respectfully, but as candidly as possible.

The coordinated mathematical, theoretical, and experimental efforts to generalize the "atomic mechanics" into a form more suitable for extended particles have now been launched, and opposing academic interests cannot stop them. In trying to jeopardize these efforts, they can only lose their face.

The construction of the underlying classical image, the Birkhoffian generalization of Hamiltonian mechanics, has been achieved without one single paper appearing in PR or PRL, as repeatedly noted to you.

You must understand that, if we see a repetition of the case a second time, and the hadronic mechanics is built without one single paper appearing in your Journals, a scandal of international and historical proportions is unavoidable, whether you see it or not.

I could withdraw paper LR2111 from your Journals and publish it (rather easily I believe) in other Journals. However, this would result in nothing else than increased risks for a crisis at some later time and, as such, the withdrawal would be against the interests of the American Physical Society, in my view.

The primary function of your Journals vis—a—vis national interests is to pursue NOVEL *physical knowledge. If this task is made unreasonably difficult by established academic interests, the problem of potential conflict between your editorial practices and national interests is unavoidable.*

Very truly yours,

Ruggero M. Santilli

RMS/mlw

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 16, 1982

RE: paper LR211 submitted to PRL entl."Use of the hadronic mechanics for the fit of the time-asymmetry recently measured by Slobodrian Conzett, et al."

Dr.D. LAZARUS, Editor in Chief
The American Physical Society

Dear Dr. Lazarus,

I must express my indignation at a letter from Dr. C.M.SOMMERFIELD of Yale University I have just received (copy enclosed).

My entire struggle in this case is to have your Editors producing professional referee reports, with the clear identification of scientifically credible errors, inconsistencies, or incompatibility.I believe this is important for your Journals as well as for the APS in this instance, because of the number and nature of the observers monitoring the case, which include participants to a recent international conference on the origin of irreversibility in nature, as well as scholars interested in this historical problem. In addition, the Nobel Committee has received numerous recommendations from several Countries supporting the candidacy at some future time of Professors Slobodrian and Conzett (who first measured the time-asymmetry in nuclear physics), and a form of monitoring appears to be in place.

The letter by Dr. Sommerfield, under these circumstances, constitutes a clear disservice to your Journals as well as to the APS. In fact, letters of this type could,at the extreme, turn the case into a street fight. To begin, Dr. Sommerfield has no knowledge whatsoever of the field of the paper (NONHAMILTONIAN classical, statistical, and particle mechanics). Thus, his personal opinion has no meaningful scientific value beyond the level of curiosity. Furthermore, he claims that the referees are well known and respected physicists. But by whom? Is this because these referees belong to the group of academic-financial interests of which Dr. Sommerfield is well known to be an active member? At any rate, the lack of credibility and the unprofessional character of the reports (see my last letter to you of October 12, 1982) speak for themselves.

To prevent a completely un-necessary deterioration of this case, with international consequences, caused by Dr. Sommerfield's intervention, I beg you to confirm our rather moderate conclusions we reached by phone on October 6, 1982, to the effect that:

1. We shall pause for a couple of months in the consideration of this paper, to give time to your Editors to consider a paper recently submitted to Phys. Rev.-C by the Québec experimental group confirming the original measures of time-asymmetry (which constitutes a beautiful, if not necessarily final, EXPERIMENTAL confirmation of my paper);
2. I shall subsequently submit a revised version of my letter LR2111 for one, final review. This revised version shall stress in a clealer form the conjectural-speculative character of the paper, as well as its elementary nature, and include any change of style and or of contents deemed recommendable; while
3. You shall let me know the most appropriate Journal for this final re-submission, whether Phys. Rev. Letters, or Phys. Rev. C, or Phys. Rev. D.

Thank you.

Very Truly Yours

Ruggero Maria Santilli

cc.: Professors A.B.GIAMATTI and F.W.K.FIRK, Yale University.

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

October 19, 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Your letters of October 12 and October 16 just arrived in today's mail. The copy of Dr. Sommerfield's letter, referred to in your letter of October 16, was not enclosed, so I have not seen his report, which I presume was requested by the Editor as the standard first step in the formal author appeals process.

This letter will confirm my understanding of our telephone conversation as it affects the status of your paper submitted to PRL:

1. The matter of your earlier paper LR2111, "Use of the hadronic mechanics..." will be placed "on hold" for a couple of months until the editors have had time to consider the new paper by the Quebec group recently submitted to Phys. Rev. C regarding an experimental test of time-assymetry.

2. You plan to submit a revised version of LP2111 for further review. (By our rules, this will probably be considered de nuovo, as a new submission.)

3. Your revised paper may be submitted to any of our journals: PRL, or Phys. Rev. C or D, which you (not I) consider most appropriate/

4. You have the right to submit, along with your paper, a suggested list of (several) possible referees (which the Editors may, or may not wish to use as a basis for referee selection) as well as a list of persons whom you would specifically exclude as possible referees.

I am sending copies of this letter to the Editors of PRL, Phys. Rev. C and Phys. Rev. D.

Sincerely,

David Lazarus

xc: G. L. Trigg
H. H. Barschall
D. Nordstrom

CONFIDENTIAL

December6, 1982

Dr. D. Lazarus
Editor in Chief,
Physical Review Letters and Physical Reviews

Dear Dr. Lazarus,

I have been informed that Physical Review Letters is considering the publication in early 1983 of a paper by Dr. C. Rubbia and his co-workers concerning the alleged identification at CERN of two apparent "candidates" for the heavy bosons they have been looking for.

I am contacting you to recommend the maximal possible prudence in the handling of this case. Also, I am contacting you to express my viewpoint which, whatever its value, is sincerely intended in the interest of the American Physical Society, as I hope you will see.

The need for the utmost possible caution in this case stems from several aspects, such as

[a] the fact that we are gearing up here for a national call intended to promote the formulation, adoption and inforcement by the APS of a code of ethics; even though this action will be as orderly as possible, it will inevitably focus attention on all future developments at your Journals;

[b] Dr. Rubbia's view that he has apparent "candidates" is not sufficiently shared by his own colleagues at CERN and other places, to the best of the information that has reached me; you should therefore take into consideration the possibility that, under action [a], some of Dr. Rubbia's colleagues decide to express publicly his/her own view and the implications of such (not so unrealistic) scenario for our community;

[c] Dr. Rubbia has regrettably made some questionable statements to the press prior to the initiation of these experiments; as an example, the New York Time of mid August 1982 quoted the following statement by Dr. Rubbia: " when the experiment begins running full blast in October, 10 $W^{\pm}$ and one Z° particle should be seen dayly." As everybody knows, the reality has been far distant from these salesmen-type statements, and this may have a direct bearing on the implications of a possible publication by (any of) your Journals.

Permit me to express my view, most respectfully, for whatever its value. I believe that Dr. Rubbia paper should be published by Physical Review Letters or, in case of insufficient value, at least as rapid communication in Physical Review D. This is so because of my believe, now familiar to you, that all plausible physical views of fundamental character must be published, and then eventually proved wrong by other papers. The aspects in which utmost caution must be exercised are the following.

[1] the rapidity of publication; it is of the utmost importance, particularly during a forthcoming national call for a code of ethics, that the time of publication of Dr. Rubbia's paper be exactly the same as that of nonaligned papers, in the average of about one year; this rule of thumb would put publication at about end 1983; besides proving lack of partisanship (at least in this case), it will give you time to verify that the team at CERN is indeed aligned, and it will give time to Dr. Rubbia to verify each and every one of his statements;

[2] the language of publication is equally of vital importance for the American Physical Society; I am referring here to the need for a clear identification in the paper of the conjectural character of the claim, and the complete absence of excessive languages favoring the existence of quarks as physical particles, or implying it as established.

In case you give me the opportunity to review the paper as your personal adviser, or as a formal referee for its theoretical part (only), or in any way you prefer, I can provide you with more specific recommendation. Again, permit me to stress that I favor the publication, and you should not expect an a-priori rejection. Instead, I can advise you on what appears to be the best possible handling, of course not in the interest of Dr. Rubbia and his group, but instead in the best interest of the pursuit of knowledge and of the American Physical Society.

Nevertheless, I beg you not to feel obliged to mail me copy of the paper. I offered this possibility as a sincere manifestation of my desire to collaborate, particularly during the forthcoming call for the code of ethics, in order to minimize or otherwise prevent un-necessary deteriorations.

I have mailed one copy of this letter only to Dr. P.W.Anderson at Princeton University, but I have absteined from mailing any additional copy to members of the Editorial Organization of your Journals.

Best Personal Regards

Ruggero Maria Santilli
96 Prescott Street
Cambridge, Massachusetts 02138
tel. (617) 864 9859

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

December 17, 1982

Dr. R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Your letter of December 6 has reached me at the Editorial Office of the American Physical Society, where I am catching up on various matters this week.

I am completely unsympathetic with your request. Surely, as a journal editor yourself, you must be aware of the fact that all submissions to scientific journals are privileged communications, whose very existence must be presumed to be confidential (except for review pruposes), unless disclosed by the author. Even I have no right to see any submitted paper, unless this is required for review purposes. Accordingly, I have no knowledge of whether Rubbia has, or has not, submitted a paper to Physical Review Letters. In any event, it would be completely improper for me to copy such a paper for you, for any reason, unless you were selected as a referee by one of the Editors of the journal. If you wish a copy of the paper, if it exists, you must write to Rubbia yourself.

I should have thought that someone as concerned about the ethics of publication as yourself would have been more sensitive than to have requested me to do something completely unethical.

Sincerely,

David Lazarus
Editor-in-Chief

DL:pd

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

December 21, 1982

Dr. D. LAZARUS
Editor in Chief
American Physical Society
Department of Physics
University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

Quite regrettably, I must have a record of disagreement with your letter of December 17.

On my own letter of December 6, 1982, as you can see perhaps by reading it again, I submitted a delicate recommendation on a potentially dangerous topic for the APS, (a) in a way "most respectfully", (b) for "whatever its value", and (c) with the explicitly written statement (page 2, line 7)

"I beg you not to feel obliged to mail me a copy of the paper"

As you can see, it is evident that I did not "request" copy of the paper, as your letter tends to imply. After all, I do not even know whether the paper has been truly submitted, owing to the tentative information I am receiving from my contacts at CERN.

Also, I do not see how an editor can do something completely unethical by consulting physicists for additional advice on matters of considerable controversy, such as the alleged "candidates" at CERN are, but this is my personal view, and I am not pretending you to agree.

At any rate, your sentitivity to ethical issues is sincerely appreciated. It may be the focal point in which we can pull all of us together, resolve our differences in an orderly way, and avoid un-necessary public crisis.

Very Truly Yours

Ruggero M. Santilli

cc. Dr. Anderson (only).

P.S. You will be pleased to know that ref.s [2] and [3] of the new paper I recently submitted to you ("A possible time-asymmetric model for open nuclear reactions") have been printed and are now available via ordinary channels (these are the vol. II of my two series of monographs, one with Springer-Verlag and one with Hadronic Press). I thought that the referees might be interested in the information. I confirm the availability on request of temporary copies for the referees convenience.

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

January 6, 1983

Dr. R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I am again at the APS Editorial Office, where your letter of December 21 just reached me after some delay, since I have been away from Urbana for a couple of weeks.

I think that some of the confusion in our letters may be caused by your misunderstanding of my role vis-a-vis our journals. As Editor-in-Chief for the American Physical Society, I have executive responsibility for all of our journals, but I am not an Editor of any of them. Editors receive submitted manuscripts, select referees, conduct correspondence with authors, etc., etc., all directed to selecting (and rejecting) papers for their individual journals. Each journal has one or more Editors: Physical Review A, Physical Review B, Physical Review C, Physical Review D, Physical Review Letters (3 Editors) and Reviews of Modern Physics.Each journal also has one or more Associate and/or Assistant Editors who aid the full Editors in their work.

My role is to worry about the finances of our journals, to establish policy, to interact with the active physics community (of which I am a part), to handle author appeals and other "sticky" situations: in short, to represent the whole of the Society in the operations of all of our publications. Thus I never enter into the matter of selecting referees or soliciting opinions, unless on specific request of an author or an editor. My role is not that of "super-editor," but more that of Chairman of the Users' Group, with financial responsibility.

One small point: our typical time delay between submission and publication is far less than one year, as you suggest. It is closer to 3-4 months, which is still far too long.

Sincerely,

David Lazarus

xc: P. W. Anderson

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

February 8, 1983

Professor R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

I have just learned, via a CERN presse release, that Rubbia's paper describing the alleged discovery of the intermediate vector boson will be published in Physics Letters B, 25 February 1983.

Physics Letters is not published by the American Physical Society.

Sincerely,

David Lazarus

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

April 25, 1983

Dr. R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Paper LZ 2206

Dear Dr. Santilli:

I am sorry to be a bit delayed in replying to your recent note, attached to a copy of your letter of April 9 to George Trigg. All our Editors were away at the Baltimore APS meeting last week, and I wanted a chance to speak with Dr. Trigg before I wrote to you, to be sure that I was aware of all the facts regarding the paper.

First, let me point out that Professor Okubo, by his own request (noted in his letter to you of November 10, 1982), was not a referee on paper LZ 2206; he was a referee, as he stated to you, on your earlier paper LR 2111, and it was that paper which he suggested might be more suitable for Phys. Rev. None of the referees suggested that paper LZ 2206 might be better for Phys. Rev., and no Phys. Rev. editors have ever seen it. Clearly, therefore, there is no way in which it can be summarily accepted for Phys. Rev., since, in fact, it has never been submitted to Phys. Rev., either by you or by referrral of the PRL Editors.

I have read through the comments of the three reviewers of this paper with some care, particularly since I do know their identities. All three are very respectable physicists and leaders in the field, and referee no. 2, who dismissed the paper summarily, is a Nobel laureate. You could go ahead and ask that the paper be submitted to Phys. Rev. D, but my guess is that it would probably elicit similar responses from referees. Instead, I suggest that you look again at all three referees responses and, wearing your editor's hat, ask yourself what advide you might give to an author whose paper, as submitted, elicted these reponses from responsible, even famous, physicist-reviewers. Even more important, ask yourself, as an author, "To whom is this paper really addressed? Who may be expected to read it? What should they learn from reading it?" In this vein, it makes no sense to continue fighting back and forth about finding a referee who is sufficiently well versed in the very estoteric subject addressed by the paper (and, I presume, by your earlier papers which we have had to reject) who can persuade the Editor that the paper should be published. It would still, presumably, be incomprehensible to most of the world's theorists who, apparently, do not even understand your notation and equations, much less their importance. It would be even less comprehensible to less sophisticated general readers whom you would, presumably, like to convince of the importance of your work. Note carefully that referees 1 and 3 do feel that there is probably merit in the work but clearly cannot themselves understand it sufficiently to pass judgement on it. Referee 2 cannot even read the paper, and clearly finds it completely "obscure."

As you well know, authors are often the worst judges of the comprehensibility of their own papers. Facts and statements which are obvious to them (after thinking hard about the subject, possibly for years) are often completely vague to a less well informed reader, even one very expert in other facets of the subject. The purpose of any paper which merits publication, at least in the journals of the American Physical Society, must be to teach a sensible subset of readers something new. We are not running a "Vanity Press" for the benefit of our authors. The two words "teach" and "new" are the operative definitions of acceptance or rejection, and these are always to be judged with reference to their benefit only to readers. We never reject papers simply because their are not "main-line." Controversy in physics is expected,natural, and even healthy. Your papers are not being rejected because they are bad physics (demonstrably bad), or trivial (not "new"), or "anti-establishment." They are being rejected simply because they are not comprehensible to a very large set of your peers. Einstein may have been "anti-establishment" in 1905, but his three famous papers were published in Annalen der Physik, because they were well written and comprehensible.... indeed, they are models of clear,written physics.

Remember that a paper must answer, in advance, all those "little" questions which a responsible reader may ask. Accordingly, it carries a greater burden on the author than is necessary for a speaker on the same subject, who is physically present to answer questions.

If you are writing your papers to be read by readers who are not already expert in Lie-associated, Lie- admissable, and Lie-isotopic constructions, then admit that papers, as you are now writing them, are not comprehensible to such readers.(If, on the other hand, you are writing only for readers who are already experts in this area, the Physical Review journals are not suitable vehicles for your papers.)

I strongly suggest that your consider rewriting your paper completely, very possibly for Physical Review rather than PRL, where you will not be constrained to a very few pages, and try to make it completely comprehensible to a reasonably unsophisticated reader. You might wish to consider a somewhat "neutral" co-author, perhaps someone like Professor Okubo, or possibly Francis Low, or someone else of comparable stature who is experienced in writing comprehensible papers on esoteric subjects. Alternatively, you may wish to write a paper yourself, but bounce it off several such persons before submitting it for publication, and be prepared to revise it massively if the responses indicate that it is unclear. I always ask someone else to read through my own papers before I submit them, and have often gone through several drafts before the paper is actually mailed off to the journal, and my papers are about as non-controversial as you can get! Where papers are controversial and subject to possible misinterpretation, it is even more incumbent on the author to ensure that his submitted paper is absolutely clear and free from errors.

I would like your papers to be acceptable to our journals; I love a good fight, particularly between theorists! I hope you will take my comments as friendly suggestions, in the way they are intended.

Sincerely,

David Lazarus

xc: S. Okubo
G. L. Trigg
D. L. Nordstom
F. E. Low

Dr. D. LAZARUS, Editor in Chief
The American Physical Society

April 29, 1983

Dear Dr. Lazarus,

I appreciated the courtesy of your letter of April 25, 1983. Regrettably, it appears that you have been unable to address the real problems for predictable and understandable reasons. I shall therefore keep the submission of a (revised) version of my note LR2111=LZ 2206 at the European Editor I have contacted. Also, I regret to inform you that I do not contemplate to submit additional papers to APS Journals for the foreseable future (I am writing a considerable number of them for the final stage of my terminal DOE grant).The only exception has been my recent submission of paper DDR231 to Phys. Rev. D (under legal assistance beginning with the submission). This is due to the fact that the indignation of members of our team had reached alarming proportion because of the suppression of the quotation of rather massive references in papers printed in your Journals. As president of the I.B.R. I thought that perhaps I should try to minimize the risks of a direct, open confrontation. But I am still doubtful that my submission was indeed the right thing to do.

It is very regrettable that you could not address the alleged misconduits that have occurred, primarily, in the handling of experimental papers on time-asymmetry, and then on my own theoretical note. There is no point to repeat them here. Perhaps, you should understand why I do not want to waste my time with APS journalsfor the foreseable future. If I put my editorial hat, I would have released the following report on papers LR2111=LZ2206:

> "Paper LR2111(orLZ2206) is not suitable for publication in its current form. However, the paper could be considered for possible publication as a Rapid Communication in Phys. Rev. D (or C), provided that Santilli complies with the following suggestions: (1) that he clarifies the connection between his model and Prigogine's statistics; (2) that he identifies more clearly the non-Hamiltonian origin of the irreversibility (plus any other suggested improvement) and, last but not least, (3) that he prepares a longer, more detailed paper on the same topic to be submitted jointly with the revised letter."

My reaction to a constructive refereeing of this type would have been, first, of gratitude, and second, of full and complete cooperation.

Instead, all the numerous referees' reports released by your office stated nothing but REJECT, REJECT, REJECT, and then attempted unbeliavable mumbo-jambo dances in the dream of"smoking out" the rejection. Your seemingly sound suggestion (write a longer and more detailed paper) is therefore shattered by incontrovertible evidence established by over a decade of occurrences of this type. In fact, it would be equivalent to permitting the suppression of the model for a number of additional years. It is evident that the only way to avoid these dark shadows would have been the usual ways followed by papers aligned with vested academic-financial-ethnic interests: publish a short letter (which can be understood, in general, by very very few) and, subsequently, publish a long detailed paper. We should not forget that scientific rigour is at the foundation of any sound advance. However, excesses in the request of scientific rigour are generally a facade for manipulations, particularly when addressing potentially fundamental advances.

You mention that referee no. 2 of paper LZ2206 is a Nobel laureate. This is exactly the same as telling a jewish physicist who survided a concentration camp that the referee of his paper is a famous german scientist.In my letter to you ofNovember 27, 1983 I told you the episode of my visit at Lyman laboratory, where the triplet Glashow-Weinberg-Coleman, two of whom are Nobel laureates, specifically and intentionally created severe hardship on my children and on my family by preventing my drawing my own salary from my own grant. The very mention that referee 2 of paper LZ2206 is a Nobel laureate is a confirmation of the lack of acknowledgment at the journals of the APS of an editorial problem that, according to an increasing number of observers, has now reached the dimension of threat to National interests because of its dimension, diversification and high level of manifestation (see enclosures).In the final analysis, the selection of a (US) Nobel laureate as a referee of my paper may be seen as demonstrably unethical because no (US) Nobel laureate has any meaningful knowledge and record of expertise in the field of the paper (isotopies and genotopies of Hilbert spaces and Lie algebras).

But the apparent scientific crime committed with paper LR2111=LZ2206 is considerably broader then the mere suppression of a theoretical model. As repeatedly indicated to you, the paper was the representative of a new scientific current involving an increasing number of experimentalists, theoreticians, and mathematicians, as well as of a new institute of research, funded and organized via (for us) immense sacrifices.The suppression of paper LR2111=LZ2206 has implied, whether directly or indirectly, the rejection of a considerable number of research grant applications submitted to U.S. Federal Agencies by distinguished U.S. and foreign scholars. In fact, the rejection of the mathematical applications was essentially based on the claim that the Lie-admissible algebras do not have physical relevance because the APS journals do not publish papers on the tppic.The rejection of the physical applications was explicitly and repeatedly based on the statement that I do not publish papers in APS journals, and, as one referee put it, the only one I did publish in 1980 "was held back for more than a year before acceptance."

You know well that this Country's God is the "$". Each and every action at your journals has a direct or indirect financial implication. In our case, not only the words, but at times even the typewriters of your referees and those rejecting the I.B.R. grant applications are the same. The password in this latter case is: SUPPRESS, SUPPRESS, SUPPRESS the I.B.R. After all, we have received a truly impressive, massive rejection of applications (totaling over $ 5M over the next five years), in two instances even when the majority of the referees(the 2/3, to be exact) warmly suggested support.It is evident that a few academic barons will be pleased by the on-going assassination of the I.B.R. But, in reality, who will be the real loser? The answer is evident: America is the real loser. Also, where it started? It is evident: at your journals.

As repeatedly stated to you, my letter on the Lie-admissible treatment of open nuclear reactions was a Rubicon. This was the case for several reasons, substantially outside my control. The full year of hysterical reject, reject, reject by your office has forced the crossing of the river. Irreparable damage has now been done. Both you and me are left with nothing else than prepare for the consequences.

In the final part of your letter, you suggest that I should write a longer version of my paper in collaboration with S. Okubo or F.E.Low. Evidently, I would be honored to collaborate with any of them. However, the very mention of their names is a further indication of your lack of knowledge of the gravity of the decay of the U.S. physics community. For your information, in 1980 I wanted to spend a couple of months at Rochester to follow Prof. Okobo's lectures and learn from him (as well as, hopefully, to collaborate with him). My application was REJECTED by the department of physics at Rochester, as Okubo can testify, even though, as explicitly stated in the application, I was interested only in VISITING and the totality of the expenses would have been supported by my DOE grant. The cases occurred at M.I.T. are substantially more grave than this little dance of greed at Rochester. In fact, besides being at the basis of the very birth of the I.B.R., they touch aspects that are too delicate to be treated in this letter[you will hopefully read them one day].

The truth is that the U.S. physics community is slowly dying because of internal suffocation due to extremes of greed.Despite their substantial character, in number and quality, my experiences are nothing but an insignificant corner of putrescence. Multiply my esperiences many many times over. Think at cases such as the recent, public disqualification of Edward Teller in national televisions and newsmedia, and then you have an idea of the dimension of the problem.

Even though I acknowledge your effort (for which I am grateful), your letter contains absolutely no light, by therefore confirming the only alternative left to physicists concerned for the future of our children: GO PUBLIC, GO PUBLIC, GO PUBLIC.

Very Truly Yours

Ruggero M. Santilli

cc. Drs. Trigg , Nordstrom and Dreiss, PRL and PR , and The White House.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

May 25, 1983

Professor D. LAZARUS
Editor in Chief
American Physical Society
Department of Physics, University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

I must express my continued, extreme reservations regarding the editorial-refereeing practices at your Journals. I enclose a self-explanatory letter to Drs. Marchidon, Antippa, and Everett, authors of a paper printed at Phys. Rev. D27, 1740 (1983). The paper essentially claims that "... the slighest extension" of Einstein's special relativity is "... in violent conflict with what is observed in nature."

It is unbelievable how papers of this inspiration can pass your seemingly severe refereeing. The fact is that the severity is applied only for topics non-aligned with vested academic-financial-ethnic interests, while topics that are aligned with said interests are passed with support despite enormous distorsions of the reality.

Everybody can see politics here, but the bad one. In fact, papers of this type, once regrettably printed in your journal, can kill the imagination in young minds at birth. But, is this exactly what desired by the ring of academic barons surrounding your journal? Suppress undesired advances at birth?

How can it be possible that a growing number of international observers see huge editorial problems at your journals (some even talk of "potential crime against humanity"), and you people see nothing?

Very Truly Yours

R.M.Santilli

cc. Phys. Rev. D

P.S. You should be informed that, as expected, my paper LR2111-LZ2206 on the irreversibility of open nuclear reactions has been accepted without modification by a European letter journal after less than three weeks of consideration (while the same paper was rejected for over one year at your journal with the total and absolute lack of any constructive criticism whatsoever by your barons). This is a further element confirming that the problems exist, specifically, here in the U.S. and, specifically, at your journals.

Dr. Lazarus,

In case your editors are willing to honor Professor Okubo recommendation (to publish my paper in Phys. Rev. rather than PRL), you can count on my best possible collaboration, including my excuses for all that has happened on the case.

However, to do so, I now need a formal letter from the editor of the journal considered appropriate. In fact, I have already submitted the paper to a European Editor of a letter journal. I can withdraw it only following a formal letter from your own editor.

I mentioned this possibility as the very last attempt to avoid a truly senseless situation for all of us. The final decision is yours.

Sincerely,

R.M.Santilli

Copy to Drs. Tri... Mond Khan and Drei...

PART XIII–F:

REQUESTS OF RESIGNATION OF C. M. SOMMERFIELD AND R. K. ADAIR, AS EDITORS OF PHYS. REV. LETTERS

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE, NEW YORK 11961
Telephone (516) 924-5533

September 30, 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Ground
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

The dossier on your manuscript LR2111 on time asymmetry has been sent to me in my capacity as Associate Editor of Physical Review Letters. My task is to determine if the referees have properly performed their jobs in evaluating the paper. In the present case the referees, all of whom are well-known and respected physicists, have done just that. Thus I can find no grounds for reversing their unanimous recommendation that the manuscript not be published in the Letters.

Best regards.

Sincerely,

Charles M. Sommerfield

Charles M. Sommerfield
Divisional Associate Editor
Physical Review Letters

CMS/bsk

MAIL RECEIVED
OCT 6 1982
PHYS. REV.-P.R.L.

Ref. Mat. for LR3111

Time-reversal violation: new polarization measurements in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction

J. Pouliot, P. Bricault, J.G. Dufour[a], L. Potvin
C. Rioux[b], R. Roy, and R.J. Slobodrian

Laboratoire de Physique Nucléaire, Université Laval
Québec G1K 7P4, Canada

PACS numbers: 24.70.+s, 11.30.Er, 25.40.Jt, 25.60.Fb, 29.75.+x

Abstract

New measurements of the proton polarization in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction at 14 MeV incident energy have been carried out with a setup in three different configurations based on proton polarimeters equipped with Si or C analyzers. Our results corroborate previous measurements which have shown significant differences between polarizations in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction and analyzing powers in the inverse reaction $^{11}B(\vec{p},^{3}He)^{9}Be$, implying violation of time-reversal invariance through the failure of the polarization-analyzing power theorem.

Keywords

[NUCLEAR REACTIONS $^{9}Be(^{3}He,\vec{p})^{11}B$; E = 13.6 MeV; measured P(θ), θ(lab) = 40°, 42°, 44°, 45°, 50°.]

NOTE OF JUNE 1, 1984: THIS IS THE FRONT PAGE ON THE EXPERIMENTAL ARTICLE ON TIME-ASYMMETRY UNDER CONSIDERATION BY PHYS. REV.C INADVERTENTLY ENCLOSED BY C.M.SOMMERFIELD IN HIS LETTER OF SEPTEMBER 30, 1982.

October 16, 1982

Dr. CHARLES M. SOMMERFIELD
Department of Physics
Yale University
NEW HAVEN, Connecticut 06520

CERTIFIED MAIL
RETURN RECEIPT REQUESTED

Dr. Sommerfield,

As a member of the American Physical Society, I am hereby requesting that

you tender your resignation from your position of divisional associate editor of the Physical Review Letters,

and terminate all your editorial functions at the Journals of the APS as soon as possible.

This request is the result of your unsolicited letter of September 30, 1982 (which reached me only on October 14, 1982) in which you misused your editorial position, you violated basic codes of our profession, and created doubts on the editorial processing which are damaging to the APS.

In fact, you passed judgement as a physicist on my paper LR2111 submitted to Physical Review Letters dealing with the vast field of non-Lagrangian/non-Hamiltonian, Newtonian, statistical, and particle dynamics in which you have no established record whatsoever of expertise. In addition, the contents of your letter indicates that you did not take the responsibility to become acquainted, even minimally, with this vast new field.

Episodes of this type generally admit the explanation that the editorial action is taken in the sole, intended, specific benefit of particular academic interests, or because of recommendations from members of the same group of academic interests, in disrespect of National interests for the pursuit of novel physical knowledge. In order to prevent even the remote possibility of shadows of this type on the editorial sector of the APS, you are hereby requested to resign.

You must be fully aware that this is a formal request of resignation and that, in case of its lack of due consideration, all necessary action will be implemented as vigorously as possible, as permitted by the codes of laws and of the APS, not to exclude individual and/or group action, in order to protect National interests as well as the image of the APS throughout the World.

Ruggero Maria Santilli
Member of the American Physical Society
96 Prescott, Street, Cambridge, Massachusetts 02138

cc: Dr. D. LAZARUS, Editor in Chief, APS
Observers

P.S. You should be made aware that, jointly with your letter of September 30, 1982 rejecting my paper LR2111 on a theoretical treatment of time-asymmetry, I received not one, but two copies (apparently because of a mailing mixup) of the recent paper by the Quebec experimental group submitted to PR-C which confirms the original measures of time-asymmetry, by theorefore providing a beautiful EXPERIMENTAL confirmation of my own paper.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE, NEW YORK 11961
Telephone (516) 924-5533

PHYSICAL REVIEW LETTERS
Editor
ROBERT K. ADAIR
Department of Physics
Yale University
New Haven, Conn. 06520
Tel. 203-436-1582
HOME 50 Deepwood Dr.
Hamden, Conn. 06517
Tel 203-777-2955

Oct. 27, 1982

Prof. R.M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Prof. Santilli;

In my capacity as Editor of Phys. Rev. Letters and Chairman of the Divisional Associate Editors, I am responding to your curious letter of Oct. 16 to Charles Sommerfield in his capacity as Divisional Associate Editor.

I am not writing to object to your request (?) that he resign. The first Amendment to the US Constitution gives you the absolute right to ask any one, President, Pope, or Editor, to resign. And President, Pope, or Editor can ignore you.

Instead, I am writing to correct some misapprehension you seem to harbor concerning the duties of an editor and the editorial process. Sommerfield's letter to you was <u>not</u> unsolicited. It was solicited by <u>you</u> in the act you took of submitting your paper for consideration by Phys. Rev. Letters. When you submit a paper to a journal you <u>solicit</u> editorial consideration and Sommerfield's letter to you was a part of that consideration process; a process described in some detail in the center-fold inserted in the first issue of the present volume of PRL. Moreover, you do not seem to understand that Sommerfield acted, as he should, not as a referee but as an editor. I would hope that it is obvious to you that we cannot, and never intend to, have a special editor expert in every conceivable subset of physics. I know that Charles is far from i gnorant of the areas of mechanics which exercise you, but his job is to judge the evidence from referees closer to the subject and not to judge the paper per se.

In your letter to David Lazarus, you speak of the possibility of submitting a revised version of your paper to Phys. Rev.

Letters. I must point out to you that your paper LR211 has been rejected and we will not consider again a paper which is quite similar to LR211.

Sincerely

RK Adair

Robert K. Adair

cc: G.L. Trigg
David Lazarus
Charles Sommerfield

CERTIFIED LETTER-RETURN RECEIPT REQUESTED

November 1, 1982

Dr. ROBERT K. ADAIR
Chairman, Divisional Associate Editors
Physical Review Letters
Department of Physics, Yale University
NEW HAVEN, Connecticut 06520

Dr. Adair,

It was instructively edifying to read in your letter of October 27, 1982 that you associate yourself and Dr. C. Sommerfield with popes and presidents.

I am under the impression that you understood absolutely nothing of the entire issue of my paper LR2111 submitted to Phys. Rev. Letters. However, the position that Yale University continues to give you presupposes you have the full mental capacities to understand the issue. In this latter case a more probable occurrence is that you simply mimic lack of understanding for the pursuance of objectives to be identified at the appropriate time.

As said countless times by now, PRL has the following two alternatives for paper LR211.

ALTERNATIVE I. Paper LR211 is rejected because of the clear identification of scientifically credible errors, inconsistencies, or incompatibilities presented in due scientific language. In this case you should expect nothing more than my respectful and graceful acceptance.

ALTERNATIVE II. PRL continues to reject the paper on the basis that the available referee reports are credible. In this case I shall oppose the decision in any conceivable way permitted by law, beginning with the filing of law suits to you and Dr. Sommerfield, first, as individuals, and second, as associate editors.

All my efforts have been devoted to the implementation of the best possible scientific process in this case, owing to the number of observers, and of international implications, in the best possible interest of the American Physical Society.

Your letter is a total, uncompromisable rejection of this orderly scientific process, on mere grounds that "the professor says so, and therefore it is so".

The action by you and your friend Dr. Sommerfield could be tolerated if it occurred in countries under totalitarial control, whether of political or ethnic color. It appears you forget that we are in the United States of America. If aspects of questionable conduct occurred within public offices are brought to the attention of the public at large, the persons involved are socially dead here, sooner or later. It is only a matter of time.

You associate yourself to presidents, but you forget President Nixon.

Your letter constitutes the second, completely unsolicited intervention in the case. As such it can only prove your personal, uncontrollable desire to prevent the publication of the paper, as well as to support your personal friend Dr. Sommerfield, in complete disrespect of the interests of the American Physical Society, as evidentiated by your presumptuous assumption that PRL will not consider again paper LR2111.

In addition, your letter constitutes the second, unsolicited attempt intended to falsify or otherwise annull specific agreements in regards to paper LR211 reached with Dr. Lazarus as Editor in Chief of Physical Reviews and Physical Review Letters.

In view of these and other circumstances, I am hereby requesting (sic) that you also resign from your editorial post at the Physical Review Letters, and terminate all your associations with the Journals of the American Physical Society.

Finally, I must take all possible precautions, in the interest of the American Physical Society, to truncate this insanity of unsolicited interventions in the orderly scientific process regarding paper LR2111, beginning with formal requests to the appropriate bodies to initiate investigative committees.

Ruggero Maria Santilli, Member of the American Physical Society
cc. Drs.A.B.GIAMATTI and F.W.K.FIRK, Yale University; Drs. D.LAZARUS,G. TRIGG, G.J.DREISS, and D. NORDSTROM, Phys. Rev. and Phys. Rev. Lett.; selected observers.

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AND
PHYSICAL REVIEW LETTERS

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PHYSICAL REVIEW LETTERS
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Department of Physics
Yale University
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Tel. 203-436-1582
HOME: 50 Deepwood Dr.
Hamden, Conn. 06517
Tel. 203-777-2955

Nov. 12, 1982

Prof. R.M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Prof. Santilli;

I am confident that I understand the issues involved in connection with my rejection of your paper LR2111. Clearly, you do not. Physical Review Letters does not select or reject papers according to your Alternatives (I and II). As is well known by physicists of the community, Phys. Rev. Letters operates under a mandate of the American Physical Society as a selective journal. From the set of papers submitted to the journal, a selection (of less than 50%) is accepted by the line-editors and myself for publication on the basis of our judgement that those papers will be of special interest to our general readership. That judgement, which is certainly somewhat subjective, is made after consultative procedures discussed in many PRL editorials and described in a center-fold included in the first issue of the current volume of the journal. The papers we do not accept are not, for the most part, rejected as being incorrect; they are not accepted because we editors do not feel that they fit the needs of the journal. I did not reject your paper because of any judgement by me that the paper was wrong: I rejected your paper because I decided that the objectives of the journal would be better served by other selections.

For better or worse, most scientific journals are selective journals where a portion of submitted papers are selected by the editors of the journal using whatever criteria they choose. Indeed, some journals -- Science, for example -- publish no more than 10% of submissions. The existence, and policies, of such journals have then a long tradition and the right of journals to publish material of their choice has a firm legal foundation in the First Amendment.

I can only presume from your curious remarks about "unsolicited intervention" by me, that you do not know that I hold the position of Editor of Physical Review Letters under appointment by the American Physical Society and am charged with the respon-

sibility of final decision on journal editorial matters by the Society. Hence, the final responsibility for the acceptance or rejection of papers is mine and you may conclude that what disagreements you have with the Editors -- and Associate Editors -- are disagreements with me. Moreover, inasmuch as your letters to officers of the journal are business letters, those letters are my concern and it is my responsibility to respond to those letters as I choose. I assure you that upon termination of your correspondence with Phys. Rev. Letters, you will receive no more letters from me.

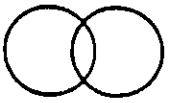
As for your "request" that I resign; after more than four years at this job I have asked to be relieved in the fullness of time but, for the moment, I have more work to do and must reluctantly reject that request.

Sincerely

RK Adair

Robert K. Adair

cc: D. Lazarus
G.L. Trigg
G.L. Wells



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 8, 1982

Professor DAVID LAZARUS
Editor in Chief
Physical Reviews and Physical Review Letters
Department of Physics
University of Illinois
URBANA, Illinois 61801

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

Dear Professor Lazarus,

Following a meeting of our Board of Governors, we hereby formally ask that you provide us with all pertinent information regarding the procedure for the initiation of investigations and/or investigative committees by the American Physical Society [or other appropriate institutional body] on possible improprieties by associate editors of your Journals acting either alone or as a possible conspiratory group, and that, in case you are unable to provide the information, you identify the appropriate officer of the APS for the securing of the information. In particular, we would appreciate the courtesy of a copy of the bylaws of the APS [or a reference to their publication] as well as of any other official document treating the procedure for the initiation of formal investigations. Please let us know all the expenses, and they will be promptly reimbursed to you.

We have informed Drs. Giamatti and Firk at Yale University of our best intention to permit a replacement of Drs. Sommerfield and Adair in their respective editorial posts at Physical Review Letters in a way as orderly as possible. Also, we have indicated that the situation at this moment can still be somewhat contained, by therefore permitting the replacement of Drs. Sommerfield and Adair, within reason, in the form preferred by them. The understanding is that formal action should be undertaken as soon as possible, owing to the history of rapid deteriorations of the case. We are referring, for instance, to a possible official announcement by the American Physical Society of the availability of openings of the positions currently held by Drs. Sommerfield and Adair, with a copy forwarded to our office, which would clearly halt all actions aiming at their resignation.

Regreattably, time is running out. You must understand that the action to have Drs. Sommerfield and Adair terminate all their editorial associations with your Journals will be relentless, continuous, and uncompromisable. A chain of actions toward the achievement of this objective are scheduled for implementation in a sequential and progressive way. This letter is only the very first step intended to identify the proper procedures within the context of the APS.

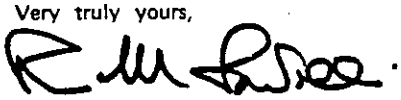
Also, you should be aware that the case of paper LR2111 can be brought at any moment now to the attention of the international press. To maintain the fundamental values of our democracy, it is therefore essential that you provide the information requested in this letter in a way

as exhaustive as possible, and, within reason, as promptly as possible.

In regard to the status of paper LR2111, we would like to confirm that we disregard the unsolicited letters by Drs. Sommerfield and Adair, and consider as valid ONLY your letter of October 19, as Editor in Chief. This is clearly essential to contain possible investigations to Drs. Sommerfield and Adair, and to prevent an unnecessary implication of your Journals at large.

Finally, we would like to stress that the scientific processing of paper LR2111 should be considered as completely independent from individual, institutional, or class actions that might be initiated to have Drs. Sommerfield and Adair terminate their editorial functions at your Journals.

Very truly yours,



Ruggero Maria Santilli
President

RMS/mlw

cc: Drs. G. L. TRIGG, H. H. BARSCHALL, D. NORDSTROM, and G. J. DREISS,
Phys. Rev. and Phys. Rev. Letters
Drs. A. B. GIAMATTI and F. W. K. FIRK, Yale University
Selected Observers

P.S. As a gesture of personal courtesy and respect for your person and for your Office, I enclose an outline of my forthcoming Volume II of *Foundations of Theoretical Mechanics* with Springer–Verlag entitled *Birkhoffian Generalization of Hamiltonian Mechanics*. I should be in a position to mail you a complimentary copy within a few weeks.

This monograph reviews and somewhat expands a considerable number of independent contributions in mechanics, algebra and geometry, some of which dating back from the past century, intended for the treatment of closed systems of extended particles with action-at-a-distance, potential forces as well as contact interactions for which the notion of potential (Hamiltonian) has no physical (no mathematical) basis. The name of "Birkhoffian Mechanics" has been selected for the new mechanics for certain historical reasons presented in the text.

Needless to say, this monograph presents not only the classical but also most of the operator foundations of paper LR2111, as evident from even a superficial reading of the paper. As an example, the foundations of the isotopic, left and right, generalizations of Schrodinger's equation (which are at the basis of paper LR2111) are treated in detail beginning from the Birkhoffian generalization of the Hamilton–Jacobi theory, as you can see from the enclosed outline.

The putrescence of our community of basic research has reached such an apparent level, that Drs. Sommerfield and Adair did not even bother to ask for a courtesy preview of the monograph for their own personal curiosity, let alone as a fundamental ethical rule before venturing editorial judgments. In the final analysis, the monograph presents the only genuinely new mechanics built during their life–time. This is, of course, only a minute aspect of their apparent misconduits, which include: disregard of the experimental evidence favoring the time–asymmetry in open nuclear reactions; disrespect of statistical (by now historical) needs for a credible resolution of the problem of the origin of irreversibility; ignorance of the complete lack of any identified error in the paper; etc. All these and other aspects will be duly presented and documented in the applications for the initiation of investigations on the alleged misconduits to be presented to the APS as well as to other independent bodies.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

November 27, 1982

Dr. D. LAZARUS, Editor in Chief
Physical Review and Physical Review Letters
Department of Physics, University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

I acknowledge receipt of your kind letter of November 24, 1982, as well as of an additional, unsolicited, unfriendly letter of Dr. Adair (Yale University) dated November 12, 1982.

Permit me to confirm, if at all needed, my sincere sentiments of respect and cooperation with your person. Your letter contains an adequate answer to our Institutional request of information of which I am grateful. This information has been passed to our Board of Governors as well as other members of our group for consideration. We are all aware of the difficulties for setting up investigative committees owing to the very regrettable fact that the APS does not subscribe to a code of ethics, as costumary for other societies (an occurrence which, alone, calls for proper reflection). Permit me only to disagree, quite gently, with your impression thatI have an "uncollegial" attitude . If you knew my academic life, you would agree that this is not the case. In fact, I have proved my tolerance in the past, even for excesses that would make you sick [1]. The mere fact that these episodes did not appear in the press is a proof of my collegial attitude. But the case of Drs. Sommerfield and Adair is too grave to be overlooked, or treated lightly. In fact, while preceding questionable experiences dealt with me alone, the case under consideration has clear elements of National interested which simply cannot be ignored.

As seen from our side, the situation of paper LR211 is quite straighforward, and constitutes no problem. In fact, the case was resolved during our friendly phone conversation of late September 1982. You will recall that, on my own initiative, and as a manifestation of my moderate attitude, I suggested a two-months pause in the case, also to give the opportunity to Phys. Rev. C to consider recent experimental studies supporting paper LR2111. We concluded that, depending on the circumstances, I may write a new paper for possible consideration by PRL. These lines were kindly confirmed in your letter of October 19, 1982. The case was therefore resolved along the best possible scientific lines of mutual respect and cooperation.

I subsequently asked for the resignation of Dr. Sommerfield because he wrote an unsolicited letter subsequent to our agreements, and in apparent disrespect of a number of questionable aspects, not to mention the complete lack of usefulness under the circumstances. I subsequently asked for the additional resignation of Dr. Adair because of the nature of his additional, unsolicited intervention in favor of his friend and colleague at Yale, Dr. Sommerfield.

I am under the impression that you underestimate the gravity of the unsolicited statements by Drs. Sommerfield and Adair, as well as the gravity of the continuation of their unsolicited interventions. As a first example, please read again the second unsolicited letter of Dr. Adair of November 12, 1982 (of which you should have a copy). Besides open encouragement for the "termination of your [mine] correspondence with Phys. Rev. Letters" and other aspects, the letter constitutes an implicit confirmation that the rejection of the paper was based on political, rather than scientific reasons. But paper LR2111 deals with possible basic advances [isotopic liftings of the Hilbert space with consequential possible generalization of quantum mechanics for strong interactions] which, as such, should be of PRIMARY interest to PRL. On the other side, the topic of the paper is manifestly nonaligned with existing, vested, academic interests. The most logical explanation suggested by Dr.

Adair's letter (lacking evidence to the contrary) is that rejection was based in the apparent intent of protecting existing, vested academic interests, in disrespect of the pursuit of novel physical knowledge. By keeping in mind that the case of paper LR2111 is not expected to be an isolated one, Drs. Adair's letter confirms that the problem of editorial practices at Phys. Rev. Letters may have reached the dimension of a potential threath to National interests.

The ultimate issue you should address to yourself, as Editor in Chief, is whether shadows of such gravity should be dismissed simply because claimed to be untrue, or they should be dispelled as a result of an extensive, detailed, and comprehensive examination by a number of appropriate, independent, committees. After all, the shadows are not new.

But we are still at the very beginning of the case. In his first unsolicited letter of October 27, 1982, Dr. Adair had the courage of stating (among other things)

> Phys. Rev. Letters..."will not consider a paper which is quite similar to LR2111."

This clearly implies the dishonoring of specific agreements reached with you as Editor in Chief, as well as the exclusion from the future consideration of the totality of efforts currently going on in experimental-theoretical-mathematical circles for the construction of the hadronic mechanics, exactly as I had feared in the first place in my original contacts with you. This is evident from the fact that the time evolution studied in paper LR2111 is at the foundation of ALL these efforts, now summing up to over 10,000 pages of research (virtually none of which published in your Journals), as well as the participation of a number of governments.

AS A RESULT, I HAVE INITIATED, I SHALL CONTINUE, AND, IN DUE TIME, I SHALL MULTIPLY ALL POSSIBLE OR OTHERWISE CONCEIVABLE EFFORTS PERMITTED BY LAW TO HAVE DRS. SOMMERFIELD AND ADAIR TERMINATE ALL THEIR EDITORIAL FUNCTIONS AT YOUR JOURNALS. I hope you understand that, despite my best and most moderate attitude, I HAVE NO OTHER ALTERNATIVE. IN fact, the only alternative permitted by Drs. Sommerfield and Adair is that your Journals should be excluded from the ongoing scientific efforts to generalize quantum mechanics for the strong interactions, and this could likely imply a possible future incident of huge proportions. I beg you to see the situation also from our viewpoint. You will agree that, under the indicated antiscientific-antinational attitudes, it is better to promote a containable crisis now, than a potentially explosive, international scandal tomorrow.

Permit me to reassure you that I do have my own doubts on this admittedly depressive scenario. Nevertheless, a history of complementary episodes accumulated through the years appear to confirm, rather than dispel, the scenario [2]. I sincerely wish this was not the case, and I would rejoice in case proved wrong by concrete evidence.

But this is still the beginning of the case. There are additional reasons for the actions I am considering, which are substantially more distressing, because they might imply an escalation of the crisis of unthinkable proportions. I indicated to you other times, and I confirm it here, that it is in the best interest of our community that I am silent on these additional aspects at this time.

What is important here is that you understand the potential damage that may be produced by the continuation of the unsolicited interventions by Drs. Adair and Sommerfield to other quite valuable Editors of Phys. Rev. Letters and Phys. Rev. I am referring to Editors such as Drs. Trigg, Nordstrom, and Dreiss (to mention only a few) whose integrity is beyond any shadow of doubt, as proved by a long history of independence from scientific interests (contrary to the history of association to current scientific interests by Drs. Sommerfield and Adair). I beg you to take all the necessary action so that no damage whatsoever is suffered by Drs. Trigg, Nordstrom, Dreiss, and so many other valuable physicists serving your Journals. Needless to say, you can count on my best possible assistance in this respect.

This is THE LAST LETTER I shall write to you on the matter. The two months pause of our agreement are about to expire, and a number of decisions must now be taken. I therefore believe that it is important for all that the situation (as seen from our side) is spelled out as clearly as possible. Under the current circumstances, created by the unsolicited letters and the acceptance of the validity of their statement, our only possibilities are the following.

(A) Drs. Sommerfield and Adair resign in writing, with a clear indication of the date of termination of all their editorial functions at your Journals. You can rest assured that, in this case, no action whatsoever will be initiated on my part, other than the continuation of an orderly conduction of research. The understanding is that I shall monitor the election of possible new editors [3] and that I am not a candidate.

(B) Drs. Sommerfield and Adair do not resign, but I receive substantial evidence that they shall be totally severed by all conceivable future considerations at PRL of papers on the hadronic mechanics. In this case you can rest assured that my action shall be as moderate as possible. Jointly, you must understand that certain actions, such as the promotion of a number of investigations on the case, "must" be undertaken because National interests must go beyond personal interests, whether mine or yours.

(C) Drs. Sommerfield and Adair remain in their current editorial posts and continue to participate in the consideration of papers dealing with the hadronic mechanics. Then, you should be certain that a comprehensive effort will be launched aiming at the promotion of all the necessary consideration of the problem of ethics in physics, beginning with a national campaign aimed at the need that the American Physical Society formulates, adopts, and inforces a code of ethics.

In case you see any other possibility, besides those listed above, providing solid evidence of due scientific process at Phys. Rev. Letters, please let me know (even by phone). You can count on my best possible collaboration. The only point I beg you to understand is that time is running out fast.

Sincerely Yours



Ruggero M. Santilli
Member of the American Physical Society

cc.:Drs.TRIGG, DREISS and NORDSTROM, PRL and PR
Drs. GIAMATTI and FIRK, Yale University
Dr. P. W. Anderson, Princeton University
Selected observers

[1] This statement calls for the indication of at least the following episode, from which numerous others followed. In the morning of September 1, 1977 I initiated a visit at Lyman Laboratory of Harvard University as "honorary research fellow". In the afternoon of the same day my supervisor Prof. Giorgi received a phone call from Washington amounting to an invitation for me to apply for a governmental research grant. The application was subsequently filed (with my affiliation at Lyman), and immediately funded. I discovered at that time that I could not draw a salary because of the honorary character of my appointment, according to Harvard statute. I therefore respectfully applied for the removal of the word "honorary" in my title, so that I could draw a salary. Several MONTHS passed without any action on my request. And in fact, a solution was finally reached only the SUBSEQUENT MONTH OF JUNE 1978, via my appointment as research associate at the Department of Mathematics at Harvard. To understand truly the case, you must understand that at that time I had a family of four to support, including two children of tender age, and my wife then a graduate student. The prohibition for me to receive a salary, which was notoriously due to Coleman-Glashow-Weinberg, therefore resulted in severe hardship in my children. In fact, I had no other income; all my savings evaporated after the first months,

and my unemployement benefits (I drew from Newton Corner, Ma) expired in early 1978. Thus, to truly understand the case, you must be in substantial need of money to feed and house your children, while a considerable amount of federal support is sitting in a bank, including your salary, and you are prohibited to draw it by colleagues! I leave it to you to judge your fellows Coleman-Glashow-Weinberg. Here I want only to indicate my "collegial" attitude. In fact, my first volume of "Foundations of Theoretical Mechanics" with Springer-Verlag, written at Lyman under these insane human conditions, carries a gentle and thankful acknowledgement to people at Lyman, as you can see from the enclosed copies. BUT I BEG YOU NOT TO DRAW ERRONEOUS CONCLUSIONS. THIS BEHAVIOUR OF EUROPEAN KINDNESS ON MY PART IS LONG GONE. NOW I ATTACK AT THE FIRST SIGN OF MISCONDUIT.

[2] This statement also calls for an additional example. You should be aware that the Department of Physics of Yale University, to which both Dr. Sommerfield and Dr. Adair belong, has built a considerable reputation of OPPOSING the investigations we are here talking about in between the lines [insufficiency of Einstein's special relativity for strong interactions, as made conceivable by extended charge distributions in conditions of mutual penetration], to the point of apparently suppressing the exposure of young minds at Yale to the Hadronic Journal and other conduits struggling in the search of light in this magnificent problem. I sincerely hope that this information is wrong. Yet, the Administrative Office of the Hadronic Journal confirms that Yale's libraries have received for years all necessary information on the Journal, and no subscription was ever solicited. Also, it is clear that Yale did not passed the subscription to the Hadronic Journal because of financial problems. The most plausible explanation is therefore that rumored around, that is, of <u>political</u> nature, much similar to that surrounding paper LR2111. Again, I sincerely wish that this information is proved to be wrong by clear evidence. Copy of a recent letter of the Administration of the Hadronic Journal to the people at Yale is enclosed for your perusal, because it may give you an idea that I am not alone in my doubts.

[3] Owing to occurrence [2] it is evident that possible editorial replacements should not originate at Yale. In fact, this would likely result in a MULTIPLICATION OF TROUBLES.

PART XIII–G:

COPIES OF THE FRONT PAGES OF THE THEORETICAL AND EXPERIMENTAL PAPERS ON TIME–ASYMMETRY REJECTED BY THE A.P.S. JOURNALS AND PUBLISHED ELSEWHERE

PREPRINT OF THE INSTITUTE FOR BASIC RESEARCH NUMBER DE—TP—82—9

USE OF THE HADRONIC MECHANICS FOR THE BEST FIT OF THE TIME—ASYMMETRY RECENTLY MEASURED BY SLOBODRIAN, CONZETT, ET AL.

Ruggero Maria Santilli *
The Institute for Basic Research
Harvard Grounds,
96 Prescott Street
Cambridge, Massachusetts 02138

FIRST DRAFT

IBR reception date: April 14, 1982

Abstract

Strong nuclear interactions are assumed to have a non—Hamiltonian component due to contacts among the extended nucleons, which is represented via the hadronic generalization of the atomic mechanics currently under study by a number of authors. The theory is used for the description of the recent experimental discovery by Slobodrian, Conzett, et al that the strong nuclear interactions violate the time—reversal symmetry. The fit of the experimental data provided by the hadronic mechanics is remarkable, and nonrealizable via the use of the atomic mechanics.

* Supported by the U.S.Department of Energy under Contract Number DE—AC02—80ER10651.A001

LR2111/L-1 1.

USE OF THE HADRONIC MECHANICS FOR THE FIT OF THE TIME–ASYMMETRY RECENTLY MEASURED BY SLOBODRIAN, CONZETT, ET AL.

Ruggero Maria Santilli *
The Institute for Basic Research, 96 Prescott Street, Cambridge, Massachusetts 02138

(RECEIVED 19 APRIL 1982)

New RRL 5-7-82 rcvd 5-28-82 new 9-13-82

It is shown that the hadronic generalization of the atomic mechanics currently under study by a number of researchers, can produce a fit of the time–asymmetry under strong nuclear interactions by Slobodrian, Conzett, et al., that does not appear to be possible via theories conceived for the electromagnetic interactions.

A series of experiments conducted over a number of years by Slobodrian, Conzett, et al.[1–3], has produced evidence of the violation of the time–reversal symmetry under strong nuclear interactions (here referred to as "time–asymmetry"). These results were predicted by Dirac in 1949[4], and their roots can be traced back to the birth of the equivalence between space and time, in the sense that the experimentally established space–asymmetry in nuclear physics[5] should occur jointly with a time–asymmetry.

It is evident that results[1–3], if confirmed by future experiments, will provide a resolution of the historical problem of the origin of irreversibility. This aspect was studies in detail at the recent Orléans International Conference[6]. Particular emphasis was put on the existence of rather serious problematic aspects in *quantitative* studies attempting a reconciliation between the *experimentally established* macroscopic irreversibility, and the *currently conjectural* reversibility of particle dynamics, or between the noncanonical character of the time evolution of Newtonian systems [as needed to avoid approximations of the type of the perpetual motion], and the conjectured unitary character of the evolution of the miscroscopic constituents. As shown in detail by Tellez–Arenas[7], these (and other) problematic aspects can be apparently resolved if one assumes the rather natural hypothesis that the macroscopic irreversibility and noncanonicity see their origin in contact/non-Hamiltonian forces among *extended* constituents, whether particles, atoms, or molecules.

These ideas have promoted the construction of two, interrelated, new disciplines that are becoming known under the names of "Birkhoffian mechanics" and "hadronic mechanics". The former is a (classical) generalization of the conventional Hamiltonian mechanics for the local treatment of nonpotential systems, which is the result of a considerable number of contributions in mechanics, algebra, and geometry[8]. The latter is a generalization of the "atomic mechanics" (the ordinary QM) currently under study for the representation of hadrons as extended particles, with consequential contact/non-Hamiltonian (and non-Lagrangian) interactions besides the conventional ones[6–10]. Both new mechanics are made possible by recent studies by mathematicians on generalized formulations of Lie's theory called of Lie–isotopic and of Lie–admissible type [see in ref.[9] the papers by G.M.Benkart, D.J.Britten, Y.Ilamed, M.Kôiv, J. Lôhmus, H.C.Myung, R.H. Oehmke, S.Okubo, J.MOsborn, A.A.Sagle, L.Sorgsepp, M.L.Tomber, G.P.Wene, et al.]. In fact, the Birkhoffian and hadronic mechanics are realizations of the generalized Lie theory via functions on a contangent bundle and operators on a Hilbert space, respectively, with consequential rather remarkable unity of thought.

In this note I shall use the axioms and dynamical equations of the hadronic mechanics as for–

SECOND IMPROVED VERSION

* Supported by the Department of Energy under contract number DE-AC02-80ER10651.A002.

A POSSIBLE TIME–ASYMMETRIC MODEL FOR OPEN NUCLEAR REACTIONS

Ruggero Maria Santilli*

The Institute for Basic Research, 96 Prescott Street, Cambridge, Massachusetts 02138

Submitted to Physical Review Letters on December 14, 1982

We show that an isotopic lifting of the Hilbert space implies a time–asymmetry for open nuclear reactions, while recovering time-reversal invariance for center-of-mass trajectories of the implementation of the systems into a closed form. The conceptual, mathematical, and experimental plausibilities of the model are indicated.

Without doubt, the origin of the time–asymmetry of our macroscopic world constitutes one of the most intriguing (and fundamental) open problems of contemporary physics.

At the *Newtonian level,* the situation is sufficiently (yet incompletely) understood. Consider our Earth as seen from an outside observer. Its center-of-mass trajectory is manifestly time–symmetric. Nevertheless, interior, open (nonconservative) systems are manifestly time–asymmetric. Particularly important for this note is the fact that the time–asymmetry results to be ultimately due to the *non-Hamiltonian* character of the forces, and to the consequential, *non-canonical* nature of the time evolution, as established, say, by a satellite during re-entry. Besides conventional, closed Hamiltonian systems (e.g. the planetary and atomic systems), nature clearly exhibits more general systems of closed non-Hamiltonian type, i.e., systems verifying conventional conservation laws of total quantities, yet the internal forces are outside the capabilities of Hamiltonian mechanics. This novel situation has stimulated the construction of the so-called Birkhoffian[1] generalization of Hamiltonian mechanics[2] for the exterior closed treatment, and of the complementary Birkhoff–admissible mechanics[3] for the interior open case.

At the *statistical level,* fundamental advances in the non-Hamiltonian origin of irreversibility have been made by Prigogine[4] and his group for both classical and quantum mechanical statistical ensembles. Further advances have been made by Fronteau, Tellez-Arenas, Salmon, Guiasu, Grmela, et al, this time for the non-Hamiltonian origin of irreversibility at the level of each individual constituent of a statistical ensemble, as reported at the recent Orléans International Conference[5]. The unity of thought of these statistical studies with the Newtonian profile is remarkable. In fact, the Birkhoffian mechanics is a rather natural analytic counterpart of Prigogine's statistics for closed systems, while the Birkhoff–admissible mechanics is the analytic basis of the statistics advocated by Fronteau et al for open systems, with the understanding that a deeper unity of mathematical structure exists[2,3,6].

At the *particle level,* the situation is fundamentally unresolved to this writing. A primary objective of this note is that of stressing the need for a systematic consideration of all plausible views on the problem, owing to its relevance. In fact, as it has been the case at the Newtonian and at the statistical level, irreversibility may imply a revision of the foundations of particle dynamics, with implications ranging from controlled fusion to solid state physics, as well as to other branches of sciences, such as theoretical biology.

Considerable difficulties have been recently identified for the compatibility between conventional Hamiltonian/unitary time evolutions of particles and the established irreversibility of the physical world[5]. Some of these difficulties are due to the manifest problematic aspects of any quantitative attempt to achieve the established *non-canonical* time evolution of the Newtonian systems of our environment via a large collection of *unitary* time evolutions for its constituents. Other difficulties are of statistical/thermodynamical nature.

THIRD IMPROVED VERSION

LETTERE AL NUOVO CIMENTO VOL. 37, N. 9 2 Luglio 1983

A Possible, Lie-Admissible, Time-Asymmetric Model for Open Nuclear Reactions.

R. M. SANTILLI (*)

The Institute for Basic Research - 96 Prescott Street, Cambridge, Mass. 02138, U.S.A.

(ricevuto il 20 Aprile 1983; manoscritto revisionato ricevuto il 9 Maggio 1983)

PACS. 11.30. – Symmetry and conservation laws.

FINAL PUBLICATION

Summary. – We show that an isotopic lifting of the Hilbert space implies a time-asymmetry for open nuclear reactions, while recovering the time-reversal invariance for center-of-mass trajectories of the implementation of the system into a closed form. The conceptual, mathematical, and experimental plausibilities of the model are indicated.

Without doubt, the origin of the time asymmetry of our macroscopic world constitutes one of the most intriguing (and fundamental) open problems of contemporary physics.

At the *Newtonian level*, the situation is sufficiently (yet incompletely) understood. Consider our Earth as seen from an outside observer. Its center-of-mass trajectory is manifestly time symmetric. Nevertheless, interior, open (nonconservative) systems are manifestly time asymmetric. Particularly important for this note is the fact that the time asymmetry results to be ultimately due to the *non-Hamiltonian* character of the forces, and to the consequential, *noncanonical* nature of the time evolution, as established, say, by a satellite during re-entry. Besides conventional, closed Hamiltonian systems (*e.g.* the planetary and atomic systems), Nature clearly exhibits more general systems of closed non-Hamiltonian type, *i.e.* systems verifying conventional conservation laws of total quantities, yet the internal forces are outside the capabilities of Hamiltonian mechanics. This novel situation has stimulated the construction of the so-called Birkhoffian [1] generalization of Hamiltonian mechanics [2] for the exterior closed treatment, and of the complementary Birkhoff-admissible mechanics [3] for the interior open case.

(*) Supported by the U.S. Department of Energy under contract no. DE-AC02-80ER10651.A002.

(1) G. D. BIRKHOFF: *Dynamical Systems*, Amer. Math. Soc. Providence, R.I. (1927).

(2) R. M. SANTILLI: *Foundations of Theoretical Mechanics*, Vol. II: *Birkhoffian Generalization of Hamiltonian Mechanics* (New York, N.Y. and Heidelberg, 1982).

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Time-reversal violation: new polarization measurements in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction

J. Pouliot, P. Bricault, J.G. Dufour[(a)], L. Potvin
C. Rioux[(b)], R. Roy, and R.J. Slobodrian

Laboratoire de Physique Nucléaire, Université Laval
Québec G1K 7P4, Canada

PACS numbers: 24.70.+s, 11.30.Er, 25.40.Jt, 25.60.Fb, 29.75.+x

Abstract

New measurements of the proton polarization in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction at 14 MeV incident energy have been carried out with a setup in three different configurations based on proton polarimeters equipped with Si or C analyzers. Our results corroborate previous measurements which have shown significant differences between polarizations in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction and analyzing powers in the inverse reaction $^{11}B(\vec{p},^{3}He)^{9}Be$, implying violation of time-reversal invariance through the failure of the polarization-analyzing power theorem.

Keywords

[NUCLEAR REACTIONS $^{9}Be(^{3}He,\vec{p})^{11}B$; E = 13.6 MeV; measured P(θ), θ(lab) = 40°, 42°, 44°, 45°, 50°.]

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ASYMÉTRIE DU TEMPS: POLARISATION ET POUVOIR D'ANALYSE DANS LES RÉACTIONS NUCLÉAIRES

C. RIOUX[1], R. ROY et R. J. SLOBODRIAN

Laboratoire de physique nucléaire, Département de physique, Université Laval, Québec G1K 7 P4, Canada

et

H. E. CONZETT

Lawrence Berkeley Laboratory, University of California, Berkeley, CA 94720, USA

Received 30 July 1982

Abstract: Measurements of the proton polarization in the reactions ^{7}Li(^{3}He, $\vec{p}$)^{9}Be and ^{9}Be(^{3}He, $\vec{p}$)^{11}B and of the analyzing powers of the inverse reactions, initiated by polarized protons at the same c.m. energies, show significant differences which imply the failure of the polarization-analyzing-power theorem and, *prima facie*, of time-reversal invariance in these reactions. The reaction ^{2}H(^{3}He, $\vec{p}$)^{4}He and its inverse have also been investigated and show some smaller differences. A discussion of the instrumental asymmetries is presented.

E NUCLEAR REACTIONS ^{2}H, ^{7}Li, ^{9}Be(^{3}He, p), 14 MeV; measured polarization. ^{4}He(polarized p, ^{3}He), E = 28.88, 29.77, 30.40 MeV; ^{9}Be(polarized p, ^{3}He), E = 23.06 MeV; ^{11}B(polarized p, ^{3}He), E = 22–23 MeV; measured $A(\theta)$. Natural, enriched targets.

1. Introduction

La découverte en 1964 de la violation de la symétrie CP lors de la désintégration du méson-K neutre [1]) a relancé l'intérêt pour la vérification de l'invariance sous renversement du temps (T). Cette violation de CP implique une violation équivalente de T afin de conserver le théorème CPT [2]) dont l'importance et les fortes évidences expérimentales de validité [3]) sont difficilement discutables.

Dans le cadre de la physique nucléaire, deux moyens ont été principalement retenus pour vérifier T; ce sont la balance détaillée et le théorème de polarisation-pouvoir d'analyse [4]). Ces deux voies ont en commun le principe d'invariance sous renversement du temps comme condition nécessaire et suffisante à leur démonstration

[1] Ce travail fait partie des exigences pour l'obtention du Ph.D.; adresse présente: Lawrence Berkeley Laboratory, Bldg. 88, Berkeley, CA 94720, USA.

PART XIV:

YALE

UNIVERSITY

Yale University *New Haven, Connecticut 06520*

PHYSICS DEPARTMENT
217 Prospect Street

May 15, 1979

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of May 7, 1979 and the copies of the papers by yourself and Ktorides, Myung and yourself. Please accept my apology for not having answered your earlier letter.

The questions you raise are certainly fundamental ones and will undoubtedly be with us for many years. I do have a few comments on your paper which are given below.

1. I have looked at Kim's paper (Lett. Nuovo Cimento 12, 591 (1975)) which incidentally deals with the muon lifetime and hence is probably not very strongly linked with hadronic interactions. The experiment which Kim suggests, however, could, in my opinion, be done well enough (at FNAL or the CERN SPS) to see the effect he calculates for a fundamental length of ~ 5×10^{-16} cm. It would be a major effort comparable to a "standard" high energy physics experiment at these laboratories.

2. I am enclosing a paper which will appear shortly in Physical Review Letters which reports a test of special relativity via a high γ g-2 measurement. I understand that the same PRL issue will have an article by the CERN g-2 group on the same subject. In one sense these are very "sensitive" tests in that they go to very large values of γ ($\sim 10^4$). However, I believe there is, at this time, no generally accepted calculation linking an hypothesized fundamental length and the size of any violation of the relativistic prediction of spin rotations. Of course the g-2 value of the electron, like the muon lifetime test of Kim, has little direct connection with hadronic interactions.

3. In hadronic interactions, various groups have tested the forward dispersion relations which are traditionally derived from causality, unitarity, and the crossing relations. My own group is completing such an experiment with π^+, π^-, k^+, k^-, p, $\bar{p}$ scattering on protons with energies from 70 to 200 GeV. No violations have as yet been observed but again there seems to be, at present, no theoretical framework to translate the experimental results into limits on the validity of special relativity.

4. One can contemplate experiments which measure the possible deviation of hadronic particle lifetimes from the relativistic predictions. For those hadrons, e.g. π's, or k's, which decay via the weak interaction, one can expect, with some effort, to achieve accuracies in relative lifetimes (at two energies) perhaps as good as 0.1%. Would these be interesting? For the hadrons which decay via strong (or even electromagnetic) interactions one would have to measure the energy width of the state and relative accuracies better than 10% sound difficult, to me at least.

As you know, most of the recent tests of special relativity have been carried out as a kind of "fallout" from experiments which were designed primarily for other purposes. I do not myself know of any plan to do a major experiment, primarily designed to test relativity. I believe the reason for this is twofold. First, relativity has worked so well whenever it has been tested that enthusiasm to test it again is naturally not large. Secondly, there is no alternate theory which is comparable in scope or self consistency which can be used to determine what constitutes an "interesting" level of sensitivity in testing relativity. The ideas of Kin and Redei do set some limits but their theory is necessarily phenomenological and is not really an alternate to relativity. It is more a way of parameterizing an hypothetical breakdown of special relativity.

I hope these few remarks will be interesting to you. Incidentally, if you would like to know more about the high γ g-z experiment you might correspond with Professor Peter S. Cooper here at Yale.

Sincerely,

Jack Sandweiss

Jack Sandweiss
Professor of Physics

JS/ja

Enclosure:

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 16, 1979

Professor JACK SANDWEISS
Physics Department
Yale University
NEW HAVEN, Connecticut 06520

Dear Professor Sandweiss,

I would like to express my appreciation for the courtesy of your letter of May 15, 1979.

Your kind comments will be invaluable, not only for my own research, but also for my conduction of the HADRONIC JOURNAL.

Again, I am not an experimentalist and, as such, I do not have sufficient knowledge to assess the situation. Nevertheless, the following comments might be of some value for the theoretical profile.

I am in full agreement with your general assessement that the questions under consideration will remain with us for some time. In defense of the experimentalists I would like to add that what is still missing is sufficient maturity for the treatment, at the theoretical level, of the possible invalidity of the special relativity. This is, after all, the reason why I have suggested a joint effort by theoretists and experimenters. In this way, experimenters may acquire awareness on the theoretical needs, while jointly providing the theoretists with a better identification of their function.

On more specific grounds, the following comments might be of some value.

1. Kim's original proposal of 1975, as stated, does not appear to be truly relevant to hadron physics because, as you correctly point out, it is related to the muons. I am a firm believer of the special relativity for the electromagnetic interactions and, thus, I do not see much need to test it again in this arena. Nevertheless, the proposal has been subsequently elaborated and extended to the light mesons (see Kim's article in the HJ 1, 1343 (1978)). This profile appears to be different, and constitutes the formulation of the proposal in which a number of theoretists are interested in. More specifically, the issue of experimentally detecting the existence or lack of existence of a fundamental length, oddly, is considered of secondary nature on theoretical grounds. What is considered of primary physical relevance is the possibility of gaining some experimental information on the true fundamental problem,whether

the strong interactions are local or nonlocal. In other words, it is the mechanics of the proposal by Kim which has attracted interest. If the proposal could be sufficiently modified and elaborated (of, course, for the case of the light mesons) up to the necessary maturity, it could be one way to resolve the problem of the nature of the strong interactions. I assume you are aware that if the nonlocal nature of the strong interactions can be experimentally established, the invalidation of the special relativity within a hadron is consequential (as outlined in my recent article you received, as well as in the monograph specifically devoted to this subject, ref. 14a). Another aspect of Kim's proposal which has also attracted attention is the possible link of the already experimentally established violations of discrete symmetries with nonlocal strong hadronic forces. The question then raised by Kim's proposal is whether the available experimental data on violation of discrete symmetries could be reinspected to ascertain whether such nonlocal nature is admitted or not. According to Kim's view (presented in the HJ) the violation of discrete symmetries would be nothing else that the "tip of the iceberg", that is, they are a manifestation of the violation of the entire Poincare symmetry at the structure level, and not only its discrete part. I should add that, on theoretical grounds, an unorthodox, "heretical" (so to say) view is implicit in this issue. I am here referring to insights on strong interactions via "weak" processes. The unorthodox view is that the term "weak interactions" will have only a limited life in physics. The weak decays of light mesons are seen as an expression of the structure of these particles (because they are spontaneous). As such, these decays are seen as possessing vital informations on the nature of the strong hadronic forces.
To summarize, Kim's proposal has a number of intriguing aspects from theoretical profiles. First, there is the possibility whether the measures on time lifes of light mesons can be experimentally linked to the nonlocal nature of the strong hadronic forces. Even partial results would be invaluable, that is, the experimental finalization that, even though these nonlocal forces cannot be established, at the same time they cannot be ruled out either. Second, there is the issue whether the same objective can be achieved via a simple reinspection of available data of violations of discrete symmetries (without even doing a new experiment at this time).

2. Thank you for sending me copy of the forthcoming article in the PRL on the test of the special relativity via g-2 measurements. I have inspected the article and find it excellent indeed. Nevertheless, I see no connection at all with the issues under consideration. Indeed, the test refers to the typical arena of unequivocal applicability of the special relativity, the electromagnetic interactions.

3. The experiments your group is conducting (via scattering of hadrons on protons) are indeed quite relevant for the issues under consideration. You might be interested to know, however, that these are

precisely the experiments under controversy. You might be interested to know in more details the reason of this controversy (only alluded in p. 87 of my recent paper). I beg you not to consider these remarks as offensive. My only desire is to inform you of dissident views.
The major criticism is that experiments of this nature do not have a final experimental character (they are called "conjectural experiments" or "quasi-experiments" by extremists). The reason is that experiments of this nature are heavily based on theoretical models. Furthermore, these theoretical models, such as causality, unitarity etc. are all based on conventional local formulations which do not take into account the extended character of the particles. That is, these theoretical models undoubtedly possess physical value, but such a value is only a first, crude, approximation for an expected, subsequent advancement. The criticism then goes by saying that the final data are a mere reflection of these theoretical approximations. In other words, the expectation is that, by using a more adequate representation of the strong interactions one might, in principle, reach fundamentally different data by using exactly the same experimental set up.
I do not know the theoretical methods you use in these experiments. In case you are interested to a more detailed and technical presentation of these criticisms, please let me know in more detail the specific theoretical formulas you use for the elaboration of the data (e.g., which type of cross section and on what theory it is based,etc.).
In any case, a job of identifying the incontrovertible aspect of these experiments and the impact of theoretical models in the data computation, appears advisable, also to prevent expansions of current controversy (p. 87 of my paper).
In defense of experimentalists I would like to stress that alternative theoretical models which could be comparatively used jointly with conventional models in data elaborations, are simply lacking at this moment. More specifically, I am not aware of any theoretical study at this moment which computes the cross section under local nonselfadjoint strong interactions (as an approximation of nonlocal settings). This is expected to be a feasible job, e.g., by expanding conventional quantum mechanical techniques for generalized Schrödinger's equations of type (4.6) of my paper. The point is that this job has not been done by theoretists at this very moment, although studies of this type are expected to be done soon.
In conclusion, what may be of some value for your group is the awareness that a number of researchers are working on the generalization of the theoretical models which are expectedly used in your experiments. If these generalizations will actually materialize, they potentially imply a fundamentally different elaboration of data.
My research interest is now precisely in these issues. In essence, after having reached a rudimentary generalization of Galilei's relativity in classical and "quantum" mechanics for local nonselfadjoint strong forces, I am interested in the implications for aspects, such as causality, unitarity, etc. At this moment, I simply see no way to even partially salvage conventional treatments under the condition that

page 4.

the particles are extended in size and under interaction with a necessary state of penetration of the wave packets (to activate the strong interactions). For instance, while microcausality appears to me unequivocal for these particles under long range electromagnetic interactions (for which the point-like approximation is excellent - sec. 3 of my paper), I am unable to even consistently define the same microcausality under the broader conditions considered above. At this moment, a generalization appears to be essential for consistency in the mere formulation. A similar situation occurs for other topics.
After working for a number of years on these issues I have therefore reached the rather distressing (but scientifically stimulating) conclusion that the virtual totality of contemporary theoretical physics is inapplicable to strongly interacting particles when represented as extended objects of dimension equal to the range of the strong interactions. These are Contentions 1 and 3 of my paper.

4. The questions you raise in this point are, in my view, scientifically invaluable. They relate to the extension of Kim's proposal to light mesons (point 1). I believe that a paper on the study of the feasibility of these data with current technonogy would be invaluable. Please consider the possibility that some of your associates conduct a study of this nature. In case the HADRONIC JOURNAL is considered for publication, you can rest assured that studies of this type would have utmost priority.

As concluding comments, I would like to agree with you on your assessement of the fascinating effectiveness of the special relativity until now. Yet, it appears that unequivocal evidence is available only for the electromagnetic interactions. In any case, the use of the same relativity for the strong has not preserved the physical effectiveness, resulting in the by now vexing state of affairs of quarks reported in my paper.

I also agree with you that what is much needed is an alternative (or broader) relativity, specifically conceived for extended particles in a state of penetration of their charge volumes (or wave packets). You might be interested to know that a rather feverish research activity is going on to study the generalization of Galilei's relativity I have recently proposed via the Lie-admissible algebras (HJ 1, 223 and 574). A number of mathematicians and physicists are involved (directly or indirectly) in these studies in the USA and in Europe.

The reason for this interest, as it appears to me, is the possibility of this broader relativity of allowing the interpretation of the constituents of light mesons as being produced free in the spontaneous decays. This possibility is strictly precluded by conventional laws based on point-like particles. It is centered on a more general notion of intrinsic quantities (spin, charge, etc.) which is apparently characterized in a rather direct way by a covering Lie-admissible relativity (e.g., Eqs. (4.34) and (4.37) of my recent paper).

page 5.

In conclusion, the reason why a number of physists are interested in experiments to test the special relativity under strong interactions is that a possible invalidity would allow a resolution of the fundamental problem of the hadronic constituents.

If I can be of any assistance, please do not hesitate to contact me. Again, permit me to express my appreciation for your consideration, interest, and time.

Sincerely

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS/ml

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 21, 1982

Professor A. B. GIAMATTI
President
Yale University
NEW HAVEN, Connecticut 06520

Dear Professor Giamatti,

A series of regrettable circumstances has forced me to request on October 16, that Dr. CHARLES M. SOMMERFIELD, a member of your department of physics, tenders his resignation from his position of divisional associate editor of the Physical Review Letters, and terminates all his editorial associations with the Journals of the American Physical Society. Copy of my letter requesting the resignation is enclosed, jointly with copies of two recent letters dated October 12, and 16, to Professor D. LAZARUS, Editor in Chief of the Journals of the APS. In case you desire additional information on Dr. Sommerfield's side, please feel free to contact Professor Lazarus at the address of the letters enclosed. In case you desire, for completeness, additional information of the other side, I would be happy to provide you on request with copy of the complete (rather voluminous) file on the case.

This letter is intended for the specific purpose of reassuring you of my best possible predisposition to protect the interests of YALE UNIVERSITY, and to prevent that the personal decision by Dr. Sommerfield is detrimental to your campus. For this purpose, I feel obliged to indicate as candidly and firmly as possible that the action to have Dr. Sommerfield leave the Journals of the APS will be relentless, progressive, and uncompromisable.

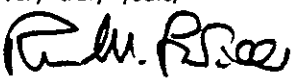
At this moment the situation is fully contained. As a result, we are now in a position to permit the replacement of Dr. Sommerfield in a way as smooth as possible, and, within reason, in the way preferred by your faculty member. However, delays and/or resistances, will force an escalation of the situation with the public disclosure of a number of aspects of the current scene in physics which can only be detrimental to all, let alone Yale University. To prevent this unnecessary deterioration, it is essential that a copy of the letter of resignation by Dr. Sommerfield reaches my desk as soon as possible. As leader of Yale University, I thought you should have the opportunity to know.

I expect you will agree with me that academic politics has affected the acquisition of novel human knowledge since immemorable times. I do not know whether you are aware of the fact that, recently, the problem has reached such a dimension to constitute a real threat to National interests. This is due to the nature, dimension, and organization of the efforts to suppress the acquisition of novel physical knowledge which is against vested, academic—financial—ethnic interests. The mere birth of our new institute in the hearth of Cambridge's academic community (of which I have been a member for some time) with the participation of so many distinguished scholars is tangible proof

of the impossibility to conduct our research at existing institutions in the city, despite the availability of governmental support, because of documented interferences by academicians in administrative control (which have reached at times unbelievable extremes of misconduit). The request of resignation of Dr. Sommerfield is only one case of a rather considerable effort under way by a number of concerned scientists to improve the scientific ethics, as a necessary condition for our survival.

Lack of action would be equivalent to the supine acceptance of the down spiral of this beautiful Country because of excesses in academic greed. This, I cannot accept silently, at whatever personal price: I want to look at my children with clear eyes.

Very truly yours,



Ruggero M. Santilli

RMS/mlw

Enclosures

cc: Dr. Professor F. W. K. FIRK, Chairman, Department of Physics
Yale University



Professor F.K.W.FINK, Chairman
Department of Physics
Yale University
NEW HAVEN, Connecticut 06520

October 25, 1982

Dear Professor Fink,

Yale University is renowed for the completeness of its libraries, with particular reference to its vast subscriptions to technical Journals in physics and mathematics. Yet, your university does not subscribe to the HADRONIC JOURNAL, despite the fact that our Journal has now entered the sixth year of regular and successful publication, and that it is now an established vehicle of research with a fast growing number of subscribers all over the world. It is evident that your physics library IS NOT COMPLETE without the Hadronic Journal.

Every year since 1978 we have mailed to your department, as well as to general libraries at Yale University, information about our Journal. As you know, our Journal is the forerunner in the promotion of experimental, theoretical, and mathematical knowdledge on the rather fundamental physical problem whether the [extended] charge distribution of hadrons is perfectly rigid under strong interactions, or it experiences small deformations. In this latter case, we would have departures from the exact character of the rotational symmetry, with far reaching implications, not only for basic research at large, but also for important aspects of National interests, such as the impact on controlled fusion. In turn, implications of this nature, once matched with the plausibility of the deformations, render the study of the problem simply mandatory, particularly when the use of public funds is involved, with consequential ethical needs for scientific accountability.

It is public knowledge that your physicists are continuing to publish articles with the tacit assumption of the perfectly rigid charge distribution of hadrons [i.e., of the exact rotational symmetry], and are continuing to use public funds along these lines, despite the now established conjectural character of the basic assumptions.

It has been brought to our attention that Yale University has not subscribed to the HADRONIC JOURNAL until now apparently because of the opposition by individual faculty members at your department, rather than because of financial difficulties.

If this is the case, permit me to bring to your attention the fact that such an occurrence:

[1] would imply the suppression of valuable scientific information at your campus in the interests of a minoritarian group;

[2] would infringe on the rights of library users at large, with particular reference to graduate students and researchers; and, last but not least,

[3] would raise the possibility of discrimination of research at Yale University under governmental support.

We enclose for your information a list of articles published in all volumes of the HADRONIC JOURNAL until 1978, as well as front pages and table of contents of international workshops and conferences which are part of the Journal's scientific activities. We hope you can see in this way the number of distinguished scientists who have contributed to our Journal, as well as the number of governments who are supporting nowadays the experimental verification of conventional physical laws under strong interactions.

If we can be of any assistance, please do not hesitate to let us know.

Very Truly Yours

[redacted]C.

cc.: [redacted]

Professor A.B.GIAMATTI, President, Yale University.

PART XV:

ANNALS

OF

PHYSICS

"Santilli has performed a real service in reviving beautiful old ideas and extending them to field theories. Such scholarly virtue is rare these days, and is very important."

REFEREE, Annals of Physics .

for the series of papers on the Inverse Problem in Field Theories published in Volumes 103 and 105 (1977).

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

October 20, 1977

Professor A. M. JAFFE, Editor,
ANNALS OF PHYSICS
Harvard University

Dear Professor Jaffe,

I hereby submit for publication in Annals of Physics my papers entitled

(1) Isotopic breaking of gauge symmetry,
(2) Need of subjecting the validity of Einstein's special relativity within a hadron to an experimental verification,
(3) Need of subjecting the validity of Pauli's exclusion principle within a hadron to an experimental verification,
(4) Possible applicability within a hadron of Lie-admissible coverings of established disciplines,
(5) Possible identification of the hadronic constituents with the electrons under the assumption of Lie-admissible covering disciplines.

Two copies of each paper are enclosed.

I would consider it a personal courtesy if a decision can be reached as soon as possible. I have worked at these papers several years, as you can see, and I am now in need of a speedy identification of their publisher.

Prior to this submission, I have submitted the material for review to a number of collegues as well as presented it this summer at European departments. I am here taking the liberty of including copy of a review by Professor A. Shimony (now at the University of Geneva) in case could be of some assistance.

I remain at your disposal for any additional material you might need.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Office No. 495 3212
Home No. 969 3465

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

Professor HERMAN FESHBACH
Chief Editor
Annals of Physics
MIT, Cambridge, Ma 02139

November 8, 1977

Dear Herman,

As you eventually know, I have submitted to Annals of Physics, via Professor R. Jaffe, the five enclosed notes. I am here taking the liberty of indicating the background motivations for this submittion.

As you remember from my reports while visiting your Department, since the time of my graduate studies in Torino I have been interested in the old idea (e.g., Enrico Fermi) that strong interactions are local but not derivable from a potential (as an approximation of an expected nonlocal setting) with particular reference with the problem of the hadronic structure.

At my arrival at MIT in January of 1976 I initiated the laborious task of reaching the necessary maturity of presentation of the essential results of my solitary journey which lasted for over a decade.

This resulted in a series of nine papers (I mailed you their abstracts some time ago) on the following five sequential steps.

(1) The delicate, but in my opinion necessary study on a possible nonapplicability of the Galilei and Einstein relativities for the assumed nature of the hadronic forces. The methodology I used for this step is that of the Inverse Problem I had studied from 1973 until recently, with particular reference to my papers in Annals of Physics (although not disclosed in my previous publications, this is a reason why I have spent so much of my time on the Inverse Problem).

(2) The equally delicate but in my opinion also necessary study of the possible existence of coverings of the Galilei and Einstein relativities for the considered type of hadronic force. The methodology I used for this study is that of the Lie-admissible problem I have been involved in since 1965.

(3) Study of the extension of the methodologies of the Inverse Problem and of the Lie-admissible problem to (generally nonintegrable) subsidiary constraints which appears to be needed to recover the experimentally proved validity of established relativities for the behaviour of the hadrons as a whole under electromagnetic interactions. The methodology I used for this step is essentially a physicist's version of the Problem of Bolza of the CV.

(4) Study of the quantization of the methods of the Inverse Problem and of the Lie-admissible problem with a conceptual emphasis focused on the assumed hadronic structure. For this step the Lie-admissible algebras turned out to be, without any doubt, the most interesting research topic I have been involved in.

(5) construction of one explicit model of hadronic structure and confrontation of the predictions with the available experimental data. As you know from my previous reports to you, my central objective is that of attempting a conceivable but explicit identification of the hadronic constituents with physical particles and of the consequential removal of the problem of confinement.

Predictably, I went through (truly) many redraftings of these papers. In spring of 1977, even though still far from final maturity, I had reached a stage which allowed me to submit the papers for confidential review to few collegues. In this way I kept improving the presentation. The reaction on the latest versions by collegues with a genuine scientific vision as well as mature capability of selfcritical examination of the currest status of our knowledge has been beyond my best expectation. To give you an indication, I enclose on a confidential basis copy of a review of these nine papers by Professor A. Shimony, now at the University of Geneva (please feel free to contact him if you so desire).

I then spent the subsequent summer to deliver a series of invited talks on these papers in Europe (Instituut voor Theoretische Mechanica of Gent, the Institut of Theoretische Physik of Zürich, and the departments of Physics of Trieste, Naples and Lecce). The encouragements I received everywhere (please feel free to contact the Heads of the indicated departments) have been also beyond my best expectation. In any case this gave me the opportunity of many hours of direct confrontations with experts on differentiated topics. On my return to the States in August I felt to have reached sufficient maturity for submission.

However, I decided not to submit to Annals of Physics this series of nine papers because of their length (over 900 pages). Publication by other Journals must be excluded because the cost will exceed $ 25,000. A major reduction of the technical arguments had also to be excluded for the simple reason that the methods I use are simply unknown in contemporary theoretical physics. An excessive reduction in their presentation would then inevitably result in misrepresentations.

As a result of this situation, I decided to submit to Annals of Physics five condensed papers (for an anticipated total of less than 30 printed pages) for the, for me, essential need of securing the papernity of the main ideas through journal publication. Jointly, I submitted the series of papers for publication as one or more monographs.

I am pleased to report that these monographs have been accepted for publication by Hadronic Press (a new publisher for fast distribution of original monographs in basis research) under the title "Lie-admissible approach to the hadronic structure", Volumes I, II and III. Their appearance will be advertised at the time of appearance of my five notes. The material is now under editorial finalization. I am also pleased to report that my monographs on the Inverse Problem have been formally accepted for publication by Springer-Verlag under the title "Foundations of Theoretical Physics", Volumes I, II and III.

Sincerely

Ruggero

Ruggero Maria Santilli

c.c.: Professors R. Jaffe and
R. Jackiw

Professor H. Feshbach,
Editor in Chief,
Annals of Physics, MIT
Cambridge, Ma 02138

Dec. 9, 1977

Dear Herman,

I would like to confirm our phone conversation of December 2, 1977 following the decision by the Board of Editors of Annals of Physics to hear a second referee on my five brief notes submitted on October 20, 1977.

The submitted notes have been written to be conceptually understandable by an experimentalist. Neverthless, they are technically ununderstandable to the best educated tearetician. In my opinion, this is an indication of their novelty. The methods which I have developed for these studies are simply new. No physicist can technically understand my papers unless he studies in all details: (a) my series of papers on the Inverse Problem in Annals of Physics, (b) my series of papers in several journals as well as books on the Lie-admissible problem, and (c) the rather vast body of literature quoted in these papers. Lacking this knowledge, it would be the same as pretending that a physicist can technically understand, say, the Thomas-Fermi model without any knowledge whatsoever of quantum mechanics. I should stress that all these references are duly quoted in the submitted papers and that my two series of forthcoming monographs (those on the Inverse Problem with Springer-Verlag and those on the Lie-admissible problem with Hadronic Press, Inc.), which are also quoted, are intended to provide a presentation of my techniques understandable by a first year graduate student.

I am at the disposal of the Editorial Board of Annals of Physics to provide any editorial, technical or linguistic improvement which is considered advisable and valuable. The submitted material, in my opinion, should be presented toghether because, if presented in subsequent stages or in different journals, could create misrepresentations. It is my understanding that it is immaterial for Annals of Physics whether the material is presented in one single paper or in five short papers, as you indicated me in our phone conversation of December 2, 1977. Copies of my monographs on the Inverse Problem are filed at MIT and additional copies were given to you in March 1977. A copy of my monographs on the Lie-admissible problem has been mailed to you by Hadronic Press, Inc. Additional copies are at your disposal for the intent of providing all possible evaluational material which is needed by Annals of Physics.

There is one aspect on which we should communicate candidly. The submitted papers are not of the typically minute incremental nature of which all of us are submerged. Instead, they touch some truly fundamental problems of hadronic physics which are unresolved on both theoretical and experimental grounds. More insidiously, the papers can represent a potential danger to the financial interests which have been constructe over the years by the U.S. governmental-academic complex on the idea of quark as the constituent of hadronic matter. I sincerely hope that a decision is taken by Annals of Physics on scientific grounds alone, and that a possible rejection is fully motivate on unequivocal technical grounds.

Sincerely

Ruggero Santilli

Ruggero Maria Santilli

c.c.: Professor A. Jaffe and R. Jackiw.

ANNALS OF PHYSICS

Editor-in-Chief:
HERMAN FESHBACH
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Assistant Editors:
BERNARD T. FELD
ROMAN W. JACKIW
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Consulting Editor:
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Publishers:
ACADEMIC PRESS, Inc.
111 Fifth Avenue
New York, New York 10003

May 22, 1978

Ruggero Santilli
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Ruggero:

We have now received our referees' reports on your papers. They are negative and we have therefore decided not to publish your work.

Sincerely yours,

Herman Feshbach
Editor

Arthur, please let me know whether you are interested in a complimentary

HARVARD UNIVERSITY
DEPARTMENT OF PHYSICS
LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 4, 1978

Professor HERMAN FESHBACH
Editor
Annals of Physics
MIT
Cambridge, Massachusetts 02139

Dear Herman,

I acknowledge receipt of your letter of May 22, 1978 indicating the rejection of my papers submitted on October 20, 1977 calling for an experimental verification of the basic laws within a hadron.

I understand your decision and you can rest assured that I respect it in full. As you know, the submitted papers were truly rudimentary. I am now publishing a series of technical papers on this topic. However, the complete technical presentation is that of my monographs in print with Springer-Verlag and Hadronic Press. You might be interested to know that the reaction by numerous collegues on this call for experiments is truly encouraging.

I would appreciate whether you can release to me technical criticisms on my papers, if any. I am sure you realize that, besides being common practice in editorial matters, this would be a scientific service. You can rest assured that I do not intend to present my countercriticisms, nor I intend to submit another paper to Annals of Physics on this fundamental problem of hadron physics. I am simply eager to know technical criticisms on my studies of the problem so that I can take them in due account.

More specifically, I am interested in critical comments on the central issue: whether an experimental verification of established relativity and quantum mechanical laws for the hadronic constituents is needed or not. Since the papers have been rejected, I assume that your referee has expressed his personal negative opinion. Has he presented a technical argument supporting such personal opinion? or has he quoted papers in which the validity of the laws considered within the arena considered is resolved in the needed unequivocal way (all available papers on unitary structure models of hadrons generally assume in a tacit form the validity)?

Nowadays, besides me, a number of physicists are working on the topic and rather intriguing papers are expected. Please let me know whether you are interested in being informed on a personal basis. On my part, I would be happy to keep you informed of the most relevant steps.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS|is

c.c.: Professors A. Jaffe and R. Jackiw

ANNALS OF PHYSICS

MIT Rm. 6-214

Editor-in-Chief:
HERMAN FESHBACH
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Assistant Editors:
BERNARD T. FELD
ROMAN W. JACKIW
ARTHUR M. JAFFE
RICHARD WILSON

Consulting Editor:
P.M. MORSE

Publishers:
ACADEMIC PRESS, Inc.
111 Fifth Avenue
New York, New York 10003

June 14, 1978

Ruggero Maria Santilli
Department of Physics
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Ruggero:

We had your paper reviewed by two referees. In regard to possible modifications you might want to introduce in order to publish it elsewhere, I think only the second review would be useful. I therefore enclose that review.

Sincerely yours,
Herman Feshbach
Herman Feshbach
Editor

Enclosure

I have studied the three papers by R. Santilli (as well as the two papers which were originally submitted but withdrawn). These papers deal with topics of interest and one can see the beginning of original ideas in them. But none of the papers are good enough to warrant publication as they stand. They look like author's notes for lectures, rather than scientific papers.

My suggestion is as follows: 1) The author should combine the first three papers into one single paper. 2) He should leave out all hints, allusions and conjectures but instead state the aim of the paper clearly. 3) He should deal with classical discrete systems, quantum discrete systems, classical field themes, etc. in separate sections. 4) Spend more effort in the writing of the paper. 5) If possible get someone to help in proof reading and editing.

I am sorry that the review has been delayed but I dislike making negative decisions. But with the best intentions I cannot recommend publication of the present manuscripts.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

April 15, 1980

Professor HERMAN FESHBACH
Editor
ANNALS OF PHYSICS
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Herman,

As a gesture of courtesy, I am enclosing some material related to an editorial impasse (intended as a temporary suspension of judgment) recently occurred at the HADRONIC JOURNAL.

It consists of the inability to accept for publication at this time a considerable number of papers in several applications of nonrelativistic quantum mechanics with generalized Hamiltonians (conventional Hamiltonians, say, of elm type are excluded).

I believe that this occurrence may interest ANNALS OF PHYSICS, and it would be a pleasure for me to provide any needed additional information. Actually, at the HADRONIC JOURNAL we have opened a special file on this intriguing case which is at the disposal of qualified referees of other Journals.

Needless to say, any contribution by you or by the friends of ANNALS OF PHYSICS which might help in resolving this impasse either for or against publication, would be sincerely welcomed.

It was a pleasure to see you briefly the past week.

Best Personal Regards

Ruggero

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

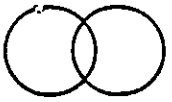
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c.c.: Professors B.T.FELD,A.M.JAFFE,R.W.JACKIW and R. WILSON,
Assistant Editors of ANNALS OF PHYSICS
Professors J. BARDEEN, J.D.BJORKEN, L.D.FADEEV,P.G.DE GENNES,
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J.PEOPLES,J.SWINGER,I.I.SHAPIRO,I.TALMI,G.H.WILKINSON, AND
A. ZICHICHI, Members of the Editorial Council of ANNALS OF PHYSICS

PART XVI:

NUCLEAR

PHYSICS



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

July12, 1983

Professor Ruggero Maria Santilli, President

To the Editors of Nuclear Physics B, PARTICLE PHYSICS
NORDITA,Blegdamsvej 17
DK-2100 COPENHAGEN, DENMARK

Dear Sirs/Madams

I here respectfully submit for publication in your Journal, the enclosed manuscripts entitled

LIE-ISOTOPIC LIFTINGS OF LIE SYMMETRIES,
I: GENERAL CONSIDEEATIONS and
II: LIFTING OF ROTATIONS

Two copies of the papers are enclosed (one with editorial markings). The papers have not been submitted to other Journals, nor they will be submitted during your consideration. In case of publication, all copyrights are hereby assigned to North-Holland Publishing Company.

The following additional elements might be of some usefulness in the consideration. This submission regards the first two papers of the series. In case of acceptance, I would like to submit the subsequent papers of this series also to your Journal. Paper III entitled Lifting of the Lorentz Symmetry, is close to completion. A summary of paper III has been published in Lettere Nuovo Cimento. Copy of the letter is enclosed for the convenience of the referee. A possible paper IV currently under preparation, deals with quantization and additional applications.

I have selected your Journal because this series is particularly written for articles already published by you. In fact, Paper III will be particularly devoted to the elaboration of the studies

H.B.Nielsen and I Picek, Nuclear Physics B211, 269 (1982)

In actuality, the entire series might be viewed as an effort to identify the relativity underlying the metric used by Nielsen and Picek for the fit of the current data on the mean life of pions and kaons as well as other aspects. Paper I presents the general background; Paper II treats the space-subcase of the metric, while Paper III treats the complete space-time case.

An additional paper closely related to the series is

C. Rioux, et al Nuclear Physics A394, 428 (1983)

on the measures of violation of time-reversal symmetry in certain nuclear reactions. In fact, Paper IV is specifically intended to provide a fit of the experimental data by Rioux et al published in your Journal, as well as to indicate the apparent relationship between the work by Nielsen and Picek on the Lorentz-asymmetry, and those by Rioux et al on the time-asymmetry.

In case you are interested in scholars familiar with the (rather specialized) work of the papers, I might indicate the following.

Professor W. BEIGLBOCK, Institut für Angewandte Mathematik
Univesität Heidelberg, Im Neuenheimer 5, D-6900 HEIDELBERG 1, West Germany

[Professor Beiglbock is the Editor of Springer-Verlag that was in charge of my Volumes I and II of "Foundations of Theoretical Mechanics"; Vol. II in particular constitutes the foundation of the papers]

Professor R. MIGNANI, Istituto di Fisica, Universita' degli Studi La Sapienza, Piazzale Aldo Moro, I-00185 ROME, Italy

[Professor Mignani is a leading expert in the techniques of the papers called Lie-isotopies, particularly from a physical viewpoint]

Professor G. EDER, Atominstitut, Schuettelstrasse 115, A-1020 WIEN, Austria

[Prof. Eder, Director of the Theor. Phys. Div. of the Atominstitut, is a leading expert in the application of the generalized theory of rotations to nuclear physics, with particular reference to the interpretation of the origin of anomalous magnetic moments and precessions].

A list of additional experts is at your disposal on request, including a list of mathematicians on the Lie-isotopic theory.

Thanking you for your consideration, I remain

Yours Very Truly

Ruggero M. Santilli

RMS-mlw
encls.

P.S. I shall remain here at the I.B.R. until August 8. Thereafter, I shall be traveling in Europe, to be back here in early September.

NUCLEAR PHYSICS

JOURNAL DEVOTED TO THE EXPERIMENTAL AND THEORETICAL STUDY OF THE FUNDAMENTAL CONSTITUENTS OF MATTER AND THEIR INTERACTIONS

Editorial Office of
"NUCLEAR PHYSICS"
c/o Nordita
Blegdamsvej 17
2100 COPENHAGEN Ø
DENMARK
Tel.: (01) 38 97 18
Telex: 15216 nbi dk

Professor R.M. Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138
USA

23 September 1983

Lie-isotopic liftings ... general considerations (Ref. 7275)

Lie isotopic liftings ... lifting of rotations (Ref. 7276)

Dear Professor Santilli,

The above papers have been reviewed by the referee, whose report is herewith enclosed.

In view of this, we regret that they cannot be accepted for publication in Nuclear Physics B.

Yours sincerely,

K. Jones

for The Editors

enc.
KJ/kam

REFEREE'S REPORT

Author: R.M.Santilli

Titles: Lie-isotopic liftings of Lie symmetries,
I.General considerations
II.Lifting of rotations .

In these papers the author hopes to exploit the mathematical notion of isotopy, i.e. the fact that the product operation in an associative algebra, $a,b \in \mathcal{A} \longrightarrow ab \in \mathcal{A}$, can be replaced by the operation $a,b \in \mathcal{A} \rightarrow agb \equiv a*b$, ($g$ fixed $\in \mathcal{A}$) without disturbing the basic axioms of the algebra. This allows one to regard a Lie algebra of matrices as a member of an isotopy class with general Lie bracket defined by the commutator $x*y - y*x$. In the second paper, the author seeks to apply these ideas to describe deformations of the Euclidean metric: at each point of space the matrix used to define the operation $*$ is nothing but the metric itself.

I do not recommend publication of these papers in Nuclear Physics for the following reasons:

a) The ratio of mathematical formalism to physically interesting results is too high; it is more typical of a journal of applied mathematics .

b) The physical interpretation of the formalism is not satisfactory. The author's concept of metric, given in I I,eq. (2.10), does not coincide with the standard terminology of differential geometry, where the metric defines a bilinear form on the tangent space at each point of a manifold, rather than the very general non-linear function of the coordinates defined by (2.10). Because it is tied to a preferred origin of coordinates, I doubt that this quantity will play any essential role in the physics of deformable bodies, inhomogeneous, anisotropic media, etc. The use of an analogous expression in relativity theory (see "Lie-isotopic lifting of the special relativity....") seems equally unpromising.

Note also: the discussion of the rotation group is not quite correct (but easily corrected). Eq.(2.2a), with (2.4) is not the Euler-angle decomposition of an arbitrary rotation. The quantity (2.2b) is not the inverse of (2.2a) in general(wrong order of factors). Eq. (2.12b)applies, I assume, only to one-dimensional subgroups.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

October 11, 1983

To the Editors of
NUCLEAR PHYSICS
c/o Nordita
Blegdamsvej 17
2100 COPENHAGEN, Denmark

RE: "Lie–isotopic liftings of
Lie symmetries, I and II"
ref.s numbers 7275 and 7276

Dear Colleagues,

Permit me to express, most respectfully but most firmly, my disappointment for the lack of scientific content of your referee report, as well as for his/her apparent lack of expertise in the field of the papers.

The papers were rejected on grounds of the fact that the metric used is "very general" and it is not restricted, in the referee's viewpoint, to the definition of a metric on the tangent space. First, this is untrue. In fact, the dependence of the metric on velocities is explicitly indicated in the papers. Second, the restriction suggested by the referee, if implemented, would prohibit one of the primary objectives of the Lie–isotopy, the incorporation of gravitation. Third, the Lie–isotopic theory must be formulated for the most general possible metric, and definitively not for one of its possible versions.

Admittedly, the paper could be improved with the indication that metric can be referred to its version of the contemporary differential geometry, although the lack of need of specific restrictions on the metric for the general formulation of the Lie–isotopic theory should be jointly indicated. But, as one can see, this is a manifestly secondary point.

Most of all, my disappointment originates from the statement that the Lie–isotopic generalization of Lie theory and of Lie symmetries is "unpromising". The referee and the Editors of NUCLEAR PHYSICS are not apparently aware of the fact that:

- The Lie–isotopic theory has already produced a GENERALIZATION OF CLASSICAL HAMILTONIAN MECHANICS, called Birkhoffian Mechanics for certain historical reasons;
- The Lie–isotopic theory is also at the foundation of the so–called "hadronic mechanics", a possible generalization of quantum mechanics for extended, deformable hadrons;
- Furthermore, the Lie–isotopic theory is at the foundation of a number of additional advances, such as a generalization of GAlilei's relativity in Newtonian mechanics for closed systems of extended particles with internal, non–Hamiltonian, contact forces; a generalization of non–Abenian gauge symmetry; and others.

How can a physicist claim that this is "unpromising"? A sample of informative material is enclosed for the Editors perusal.

Owing to the above (and other) aspects, I am respectfully asking that the refereeing conducted on papers 7275 and 7276 be ignored, and additional, independent referees be identified.

More particular, I am recommending an depth refereeing by EXPERTS in the field, that is, scientists with at least some record of publication in Lie–isotopy (or its more general version of Lie–admissibility). I am also recommending two independent refereeings, one by mathematicians on the mathematical structure of the Lie–isotopic symmetries, and one by physicists on the applications to particles physics, especially to nuclear physics. A list of experts is enclosed in case of any value.

If such new refereeing cannot be done and the rejection is final, please let me know as soon as possible, so that I can submit the papers elsewhere.

Very truly yours,

Ruggero M. Santilli

RMS/mlw

Enclosures

NUCLEAR PHYSICS

JOURNAL DEVOTED TO THE EXPERIMENTAL AND THEORETICAL STUDY OF THE FUNDAMENTAL CONSTITUENTS OF MATTER AND THEIR INTERACTIONS

Editorial Office of
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c/o Nordita
Blegdamsvej 17
2100 COPENHAGEN Ø
DENMARK
Tel.: (01) 38 97 18
Telex: 15216 nbi dk

Professor R.M. Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138
USA

28 November 1983

Lie isotopic ... I: general considerations (Ref. 7275)

Lie isotopic ... II: lifting of rotations (Ref. 7276)

Dear Professor Santilli,

Thank you for your letter and enclosures of 11 October 1983 concerning the above papers.

The referees are top experts in their field and are chosen by the editors of the journal.

I have also examined the file and agree with the recommendation of the referee, that the material presented is not well-suited for publication in Nuclear Physics B.

I regret having to make this decision final.

The material is being returned to you under separate cover.

Yours sincerely,

K. Jones

for H.R. Rubinstein
Supervisory Editor
Nuclear Physics B

KJ/kam

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

February 9, 1984

Professor H. R. RUBINSTEIN, Supervising Editor
Nuclear Physics B
NORDITA, Copenhagen, Denmark

RE: Rejection of papers # 7275 and 7276
via letter dated November 28, 1983

Dear Professor Rubinstein,

Regrettably, I feel obliged to clarify the following points.

Point 1. The papers, at the time of the submission, were definitely not mature for publication, nor they were ever intended to be. In fact, I knew of a number of errors and imperfections, and several others have been subsequently brought to my attention by colleagues. The papers were submitted with the specific intent of soliciting constructively critical comments by your referees and editors, so that the subsequent, expected, rewritings would have been patterned along the lines recommended by your Journal.

Point 2. Your editorial consideration of the papers consisted of a total, absolute and complete lack of constructive scientific process. What you and your associates have said is simply this: The papers are rejected. Period.

Point 3. You are well familiar with the contents of the papers. It must be reviewed here. The papers dealt with a vexing open problem of nuclear physics, whose lack of proper consideration is creating a considerable lack of scientific accountability for all of us, including you and your associates, vis-a-vis the taxpayer. I am referring to the fact that nucleons, once admitted as extended charge distributions, are expected to experience a deformation of their charge distribution under sufficiently intense external fields, with consequential, manifest breaking of the symmetry under the group of (conventional) rotations, and a number of other consequences, such as the alteration of the magnetic moments. In turn, the resolution of fundamental aspects of this type is expected to be useful if not essential for a number of aspects relevant for society at large, such as the controlled fusion [how people can continue to spend public funds in attempting controlled fusion via magnetic confinement if they do not resolve first the problem whether or not the intrinsic magnetic moments of nucleons change during the physical conditions of the controlled fusion?].

The papers submitted identified the problem of the deformation of the charge distribution of particles, submitted a general theory for the construction of the covering symmetries whenever the conventional ones are broken, and (paper II) constructed explictly the generalization of the rotational symmetry for deformed spheres. The specific applications to nucleons were indicated as forthcoming in the subsequent papers in my correspondence with your editorial office, beginning with my original letter of submission.

Point 4. Because of the above, the papers were conceived for and are manifestly well within the objectives of your journal, at least those officially stated.

Point 5. Whenever rejections of papers dealing with fundamentsl open problems occur via the total absence of constructively critical comments, as you and your associates have done, this inevitably implies the existence of underlying politics.

The issue opened by your letter is therefore the following:

WHICH ARE THE UNDERGROUND POLITICAL REASONS THAT HAVE FORCED YOU AND YOUR ASSOCIATES TO SUPPRESS ANY SUGGESTION FOR THE POSSIBLE IMPROVEMENT OF THE PAPERS AND FORMULATE A TERMINAL, TOTALLY UNMOTIVATED REJECTION?

The asnwer that I consider most probable is the following. The possibility that nucleons experience an alteration of their magnetic moments when under nuclear forces was fully identified in the early stages of the theory and limpidly presented in books in nuclear physics of this early period, such as those by Blatt-Weiskopff and by Segre. Subsequently, the hypothesis remained without consideration and passed to the current stage of silence in most of the contemporary literature [including papers in Nuclear Physics], except a few isolated instances [such as papers in the Hadronic Journal].

The reasons for the suppression of consideration of the hypothesis, despite its manifest plausibility and known implications, have been identified and are now well known. They are a manifestation of political-ethnic-academic interests due to the fact that, when the hypothesis is studied in any quantitative amount, it implies a violation of Einstein's special relativity, trivially, via the intermediate breaking of the rotational symmetry due to the deformation of shape.

Of course I do not have proof, but I suspect that the reason why you have implemented the suppression of any scientific process regarding the consideration of papers # 7275/7276 is due to an apparent opposition by you, your associates and your referees, against the conduction of quantitative studies on the limitations of Einstein's special relativity and on its generalization.

You should not forget that, as stated in the papers themselves, the subsequent paper III deals exactly with the isotopic lifting of the special relativity for nucleons experiencing alteration of their intrinsic characteristics, that is, deviations from an exact verification of the special relativity because of sufficiently intense, short range, external fields. This is the paper you intended to prevent to appear in your journal, as a prima facie interpretation of your behaviour!

Very Truly Yours

Ruggero Maria Santilli

RMS-mlw
cc. Professor K. JONES, Editor, Nuclear Physics

P.S. Papers # 7275/7276 are no longer available for your Journal. Their essential contents has now appeared in Lettere NC and other journals. The papers themselves have been completely rewritten twice thanks to a true scientific process provided by cooperative editors of another journal.

PART XVII:

JOURNAL

DE

PHYSIQUE

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

January 30, 1984

COMMISSION DES PUBLICATIONS FRANÇAISES DE PHYSIQUE
LABORATOIRE DE PHYSIQUE DES SOLIDES
UNIVERSITÉ DE PARIS SUD
F-91405 PARIS, FRANCE

Att.: Editors of LE JOURNAL DE PHYSIQUE

Dear Editors,

I here respectfully submit for publication in LE JOURNAL DE PHYSIQUE the enclosed article in three copies entitled

"COMMENTS ON POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE"

The paper has not been submitted elsewhere nor it will be submitted during your consideration. In case of acceptance, the copyrights are hereby granted to LES ÉDITIONS DE PHYSIQUE.

Please be reassured that I would be sincerely grateful for any constructive, critical comment aimed at the improvement of the paper. In case of any value, I enclose a list of experts in the fields of the papers that are not widely known [Lie-isotopies and Lie-admissible genotopies].

Finally, in case of interest by your Journal, I would be glad to submit to you the papers developing in detail some of the arguments [ref.s 24].

Thanking you for your consideration and time, I remain

Yours Very Truly

Ruggero M. Santilli

RMS-mlw
encls.

Secrétariat de la Commission des Publications Françaises de Physique
Bâtiment 510, Université Paris-Sud, F 91405 Orsay Cedex

Manuscript submitted for publication in *Journal de Physique* our ref. 4-1030

Author (s) R.M. Santilli
Title Comments on Polarization Experiments and the Isotropy of Space

REFEREE'S REPORT

The paper presented is nothing but a lengthy advertisement for preceding papers of the author and his followers, published in his samizdat "Hadronic Journal".

I have nothing against that kind of literature except reading it myself (Refs. 7 and 8 total more than 500 pages), but I consider that the conceptions of the author derive from a profoundly ill-conceived view of natural sciences, and of physics in particular.

To be specific, the author considers that the most general theory (non-associative, non-hamiltonian, anisotropic, and so on) is the most likely to adjust to reality. It is probably true, or at least it allows to push away indefinitely any conflict between theory and facts. This is another way of saying that such an extensive view of theory has no predictive power whatsoever, since it may be generalized enough to accommodate any fact.

Science proceeds otherwise, or at least, has been creative precisely by posing more and more stringent conditions on theories, instead of relaxing them. This, of course, leads to open conflict with the facts, sometimes, and it is precisely that kind of conflict which stimulates imagination towards better, more constrained, more predictive theories.

The authors misrepresents Ref. 1, which only discusses the possible experimental similarity between space anisotropy and parity violation ; he takes advantage of a 1.1 standard deviation experimental error taken from Ref. 29 ; with these weapons he declares war against hamiltonian Quantum mechanics, ignoring field theory, QCD, and all developments since ten years.

Is that an approach to the problem of nucleon structure ? No.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

May 24, 1984

Professor J. ZINN–JUSTIN, Editor
Journal de Physique
Commission des Publications Françaises de Physique
Bâtiment 510, Université Paris–Sud
F–91405 ORSAY CEDEX, FRANCE

Dear Professor Zinn–Justin,
I am respectfully re–submitting for publication in your Journal my paper entitled COMMENTS ON POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE. Three copies of the manuscript are enclosed. As you can see, the paper has been completely re–written following the report of your referee.

You will note that I have taken in all possible consideration the valuable part of the report (and duly thanked the referee in the Ancknowledgments for that). In fact, I have rewritten the paper in such a way to minimize as much as conceivably possible my own contributions in the field; I have eliminated references to un–essential Proceedings, to avoid the complaint of advertising (!?!); and restricted the presentation to the truly essential part: the deformation of the charge distribution of hadrons under external, sufficiently intense fields, with consequential alteration of the magnetic moments, and the available direct measures by Rauch favoring this setting.

In regard to the offensive part of the report, I beg your personal understanding. I decided long ago NOT to accept gracefully offensive language in scientific proceedings, and I regret being unable to make an exception here. At any rate, the offensive nature of the report goes beyond the contents of the paper submitted to your Journal, and invests all Editors, referees and authors of the HADRONIC JOURNAL. As seen by us, this situation is simply too grave to be accepted lightly. I have therefore enclosed a separate answer to your referee. The courtesy of sending this answer to the referee would be appreciated.

Permit me to recommend that a new referee be selcted for the further review of this paper. In fact, it is unlike that I can have a scientifically meaningful dialogue with the previous referee. Also, permit me to recommend that the review be done in Europe. I shall remain at your disposal for sending you a list of distinguished, senior experts in the field of the paper for your possible use as referees.

As far as I am concerned, I will sincerely appreciate ANY criticism on my paper (s), no matter how harsh they are, provided that they are scientifically constructive and non–offensive. Under these circumstances, you can count on my sincere collaboration and gratitude.

Very Truly Yours

Ruggero Maria Santilli
author
Efitor,
HADRONIC JOURNAL
RMS–mlw, encls.

May 24, 1984

Critical analysis
by
Ruggero Maria Santilli
on the
REFEREE REPORT RELEASED BY THE JOURNAL DE PHYSIQUE
regarding the paper
COMMENTS ON POLARIZATION EXPERIMENTS
AND THE ISOTROPY OF SPACE
(J. de Phys. ref. no. 4.1030 of 8 Feb., 1984)

A well established editorial rule is that the use of offensive language in the refereeing of technical papers is a mascara of scientific corruption, no matter how the papers are wr—ong. I present below the reasons why I suspect that this referee report is no exception. In case of evidence of the erroneous nature of my arguments, I am ready to present my most humble apologies. However, in case of insufficient evidence, the mere suspicion of dubious ethical standards should be sufficient for the termination of all future associations between the JOURNAL DE PHYSIQUE and this person.

The primary reasons why the paper was written are the recalling of certain manifestly fundamental, theoretical and experimental facts on the conventional rotational symme—try, such as: (A) the hystorical hypothesis of the deformation of the magnetic moments of hadrons under the nuclear conditions; (B) the recent interpretations by Eder et al of this alteration as due to the deformation of the charge distributions of hadrons under sufficiently intense external fields, and, last but not least, (C) the availability of direct interferometric measures by Rauch and his team (totally ignored in ref. 1 with too many others), which, in their current form, DISPROVE orthodox view in favor of the mani—festly plausible deformation/rotational asymmetry.

I have reasons to suspect that this referee intends to suppress the appearance of these manifestly plausible physical aspects in the JOURNAL DE PHYSIQUE. In fact, if the referee was seriously interested in the publication of the facts, he/she would have presen—ted a CONSTRUCTIVE report indicating all deficiencies of the paper (which are fully admitted here) and suggesting the suitable improvements. Instead, this referee has selec-ted a totally passive report, which is typical of the referee opposing the publication of the topic considered.

But, WHY THIS REFEREE IS SEEMINGLY OPPOSED TO THE PUBLICATION OF INCONTROVERTIBLE FACTS such as Rauch's experiments, and Eder's studies? A quite conceivable reason is the fact that these experimental and theoretical studies are manifestly against the vested, academic—financial—ethnic interests surrounding Ein—stein's theories. In fact, the experimental confirmation of the deformation/rotational asymmetry of hadrons would imply the irreconciliable invalidation of Einstein's special relativity for the physical conditions considered. The considerable damage to said vested interest is evident beyond any doubt.

But, above all, the primary reason that leaves this author dubious on the ethical standards of this referee, is the last passage of the report concerning the seemingly "declared war against hamiltonian Quantum mechanics, ignoring field theory, QCD, and all develop—ments since ten years." Since this referee has reached the status of refereeing for the JOURNAL DE PHYSIQUE, I must assume that he/she is fully aware of the following facts (otherwise he/she does not qualify for the review): (1) the "perpetual motion" does not exist in our macroscopic environment; (2) the physical trajectories in Newto-

nian mechanics are NONHAMILTONIAN—NONCANONICAL as a rule, and hamiltonian—canonical only as rare exceptions, as established by satellites during re—entry, damped giroscopes, all holonomic systems (because of the frictional force of the constraints), and too many additional cases; (3) the reduction of these experimentally established NONHAMILTONIAN—NONCANONICAL systems to a large collection of conjectured hamiltonian-unitary descriptions of particles constituents is manifestly inconsistent in an irreconciliable way. I must insist on the true technical knowledge of this referee (that beyond academic politics), and expect that he/she is capable of proving theorems establishing such an irreconciliable incompatibility between quantum mechanics and our real macroscopic world (that of decaying trajectories and not the preferred world of "perpetual motion" of beautiful hamiltonian-canonical character).

But then, how can this referee dream of being convincing in suppressing this incontrovertible incompatibility of quantum mechanics with the established nonhamiltonian character of the real world? How can this referee dream of suceeding with this author and his known LACK of alignment with vested interests in particle physics? How can this referee dream of succeeding via the mere mention of QCD and the litany of its unspoken problematic aspects and shear inconsistencies (such as the known, but carefully avoided in printed papers, finite, non-null probability of tunnel effects for free quarks in direct contradition with physical evidence, etc. etc.).

The reference to lack of predictive power of the generalization of quantum mechanics under construction (hadronic mechanics) is a rather clear manifestation of the typical ignorance that generally underlies offensive reports. The specific, detailed, quantitative predictions of deformation/rotational-asymmetry by Eder were reported clearly in the paper. Evidently, these detailed predictions are damaging vested interests on Einstein's ideas, but they are there, and they were there in the original version of the paper. There is no point for this author to list the number of additional predictions that are coming from a number of independent sources. The very claim of generality beyond computational capability is studiously erroneous and must be disclaimed here. Hadronic mechanics demands the knowledge of two operators, the Hamiltonian H and the isotopic operator g = I +"QM corrections", the latter one representing the nonhamiltonian forces due to contact among extended charge distributions. The new mechanics DOES NOT restrict the functional dependence of H and g in exactly the same measure as QM does not restrict the functional dependence of H. The strict implementation of this referee view would literally imply the abandonment of quantum mechanics because it implies an infinity of possible models, all those permitted by infinitely many H!!!.

The offensive reference to the "samizdat Hadronic Journal" demands a special comment inasmuch as it appears to be intended, or otherwise invests all editors, all referees, and all authors of the journal. The reason why the Hadronic Journal was founded is known in the trade and must be repeated here. It was due to the known deterioration of ethics in physics which has reached such an alarming level, to suffocate at birth the most vital aspect in the achievement of novel human knowledge, the publication of plausible conjectures. In fact, it is common knowledge that the possibility for Albert Einstein to became a scientist, would he have lived today, would have been so minute to be laughable.

At the HADRONIC JOURNAL we FIRST publish plausible physical conjectures with a sufficient technical maturity, and THEN talk about it.We do not suppress them at birth as done too often elsewhere. But this implies the publication of physical ideas that are manifestly against vested academic-financial-ethnic interests. In this sense the HADRONIC JOURNAL is definitely a "samizdat" journal. But then, this means that the journal pursues physics and not academic politics, thus resulting in a beautiful qualification of our efforts.

PREPRINT OF THE INSTITUTE FOR BASIC RESEARCH NUMBER IBR–DE–84–1

PAC NUMBERS 11.30.–j; 11.30.–Er; and 21. 03.65.–w

Journal de Physique
Manuscrit n° 4-1030
reçu le
8 FEB 1984

COMMENTS ON POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE

Ruggero Maria Santilli*
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Abstract

A valuable and courageous note by G. R. Goldstein and M. J. Moravcsik on possible tests of the rotational symmetry under strong interactions has been recently brought to our attention. In these comments we indicate possible additional tests, as well as references on the problem that were apparently unknown to the authors at the time of writing their note.

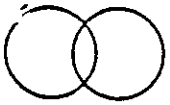
Submitted for publication

*Supported by the U. S. Department of Energy under contract number DE–AC02–80ER10651.A002.

PART XVIII:

MISCELLANEOUS

CORRESPONDENCE



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

JAN 16 1984

Office of the President

January 3, 1984

Professor J. A. WHEELER
Department of Physics
University of Texas
AUSTIN, Texas 78712

Dear Professor Wheeler,

I would gratefully appreciate your review of the enclosed paper

GENERAL RELATIVITY AND PLANETARY ORBITS, by Dr. H. YILMAZ

submitted for publication to the Hadronic Journal on December 15, 1983. As you can see, the paper contains an updated and novel presentation of the revision of the general theory of gravitation that Dr. Yilmaz has proposed since 1958.

To the best of our understanding, personally and via referee reports from preceding publications in our Journal, the situation is as follows.

1. Yilmaz generalization is compatible with all available experiments in gravitation, and, therefore, it cannot be ruled out on grounds of available experimental knowledge.

2. Einstein's equations are plagued by a number of problematic aspects, only some of which are presented or reviewed in the enclosed paper; and

3. There seems to be grounds for the initiation of a scientific process of resolution of the issue: whether Yilmaz stress—energy tensor should indeed be added to the gravitational equations for the exterior problem.

Nevertheless, we need advice by qualified experts in the field to verify the veridicity of these aspects, or at least their plausibility. We would therefore gratefully appreciate your advice on the above aspects.

Please keep in mind that the enclosed paper by Dr. Yilmaz does not contain an exhaustive presentation of all the problematic aspects of Einstein's equations identified in the literature, nor is it expected to do that. Nevertheless, a mature scientific judgment should be expressed by taking into account also these additional, at times important facets of this quite intriguing case.

As an example, I would like to bring to your attention an apparently unknown paper I wrote on the subject [Ann Phys. 83, 108 (1974)]. As you know, a fundamental assumption of Einstein's theory is that the gravitational

field in the exterior of a body with null total electromagnetic phenomenology [zero total charge, zero electric and magnetic dipoles] has no source, and the equations are $G_{\mu\nu} = 0$. The paper quoted above essentially shows that, as a result of these equations, Einstein's theory is incompatible with electromagnetism in an apparently irreconciliable way. In fact, despite the null character of the total electromagnetic quantities, the total electromagnetic field of the charged constituents of the body is far from being null and cannot be made null unless Maxwell's theory is abandomed. To put it bluntly, Einstein's theory only holds under the assumption that matter has no charge structure. Intriguingly, the electromagnetic fields resulting from the charged structure of matter has precisely the structure of Yilmaz stress—energy tensor. As a result, the paper quoted above, is in rather strong support of Yilmaz's theory.

Permit me to stress that I have no personal claim; that the scientific priority rests on Dr. Yilmaz (I merely presented an argument); and that I see no need to have Dr. Yilmaz quote the paper indicated above in his article. I brought it to your attention to indicate that the issue under consideration here is much more deep, involved, and delicate than that sometimes postured by nonscientific academic circles.

The analysis above is solely referred to the **exterior problem** of gravitation. To complement your judgment, you should also take into consideration the additional, perhaps even bigger problematic aspects of Einstein's theory of gravitation for the **interior problem**. For this purpose, it is sufficient to recall Cartan's point that the equations (actually, the Riemannian geometry itself) do not permit to recover at the Newtonian limit the equations of motion of the interior systems of our Earth, those with contact—nonpotential forces, say, of type of power series expansions in the velocities used by engineers (which have reached powers of the fourth and even fifth order in the velocity). It is evident that, until a theory of gravitation capable of admitting these systems at the Newtonian limit has been built, all current theories are and remain "provisoires".

Also, caution should be exercised in the old idea of by—passing these Newtonian forces via the reduction of the body to point—like constituents. In fact, this idea is plagued by a host of technical inconsistencies, such as the inability to reduce the experimentally established **noncanonical** time evolutions of interior trajectories of our Earth to a collection of **unitary** time evolutions of the trajectories of assumed point—like constituents.

Note that Dr. Yilmaz's paper is on the exterior problem only and, in our view, does not need to enter into the interior problem. Nevertheless, the latter should be considered for an overall judgment because the generalization of Einstein's relativity for the interior problem needed to represent the Newtonian systems of our enviroment is also expected to call for the addition of Yilmaz's stress—energy tensor when reduced to the exterior case.

In closing, permit me the liberty to suggest that a scientific process of comparative, constructively critical examination of Einstein's, Yilmaz's, and possibly other viewpoints be initiated via the presentation and examination of all views in the field. To achieve this objective in an effective way, our Institute would like to organize a Workshop and subsequently publish its proceedings in order to leave the necessary scientific record.

Kindly let me know whether your Institution might join the I.B.R. in the organization of this Workshop, and, in case this is not possible, whether you would be interested in contributing to this scientific process via your participation in the Organization Committee.

Thanking you for your time and consideration, and wishing you and your family a happy and prosperous 1984, I remain

Very truly yours,

Ruggero M. Santilli
Editor in Chief
HADRONIC JOURNAL

RMS/mlw

THE SAME LETTER WAS MAILED TO:
– A. PAIS, ROCKEFELLER UNIV.
– S. DESER, BRANDEIS UNIV.
– Y. NE'EMAN, TEL-AVIV UNIV., ISRAEL
– S. WEINBERG, UNIV. OF TEXAS AT AUSTIN

THE UNIVERSITY OF TEXAS AT AUSTIN
AUSTIN, TEXAS 78712

Center for Theoretical Physics
(512) 471-3751

January 27, 1984

Professor Ruggero M. Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This to acknowledge the receipt of Yilmaz's "General Relativity and Planetary Orbits", and your thoughtful letter about the same. Responsible colleagueship like yours is the foundation of sound science. I deeply regret that I cannot live up to your fine example because a truly major deadline is staring me in the face, forcing me to return these materials.

Best wishes for 1984.

Sincerely,

John Archibald Wheeler
Ashbel Smith Professor and
Blumberg Professor of Physics
and Center Director

Enclosures: Abstract
lch

BRANDEIS UNIVERSITY
WALTHAM, MASSACHUSETTS 02254

THE MARTIN FISHER
SCHOOL OF PHYSICS
617-647-2835

January 18, 1984

Dr. Ruggero M. Santilli, Editor in Chief
Hadronic Journal
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Thank you for your letter and the Yilmaz paper. I have, in the past, had discussions with the author, but always found it difficult to get my questions understood. I therefore feel everyone would be better served with a different referee—I especially suggest Professor C. Will at Washington University, Saint Louis, who is the expert on tests of gravity theories.

I am also afraid I cannot participate in the workshop you propose since my own interests are currently in quite different areas.

Sincerely,

Stanley Deser
Professor of Physics

SD/de

THE UNIVERSITY OF TEXAS AT AUSTIN
AUSTIN, TEXAS 78712

Department of Physics

January 17, 1984

Dr. Ruggero M. Santilli
Editor in Chief, HADRONIC JOURNAL
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

I am sorry that I will not be able to review the paper by Dr. Yilmaz, or help to organize your Workshop.

Very truly yours,

Steven Weinberg
Josey Regental Professor of Science

שר המדע והפתוח
MINISTER OF SCIENCE AND DEVELOPMENT

24 January 1984
YN/1236

Professor R M Santilli
Editor in Chief
Hadronic Journal
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge
Mass 02138
U S A

Dear Professor Santilli

I received your letter of the 3rd January but regret that due to my present duties, I find it impossible to devote the necessary time and attention required to study Yilmaz's theory.

I am sure you can find advice elsewhere in the GRG community.

With kind regards

Yours sincerely

Yuval Ne'eman

YN/bmr

ירושלים, משרדי הממשלה, הקריה הבורחית טל. 02-277046/7



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

March 31, 1983

Dr. D. L. NORDSTROM, Editor
Physical Review D
1 Research Road
RIDGE, N.Y. 11961

Dear Dr. Nordstrom,

I here submit for publication as "Rapid Communication" in PHYSICAL REVIEW D my enclosed paper in two copies entitled

COMMENTS ON THE NOTE "POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE" BY G.R.GOLDSTEIN AND M.J.MORAVCSIK

The PACS numbers are in the front page of the article; the copyright transfer letter is enclosed; and the publication costs will be paid by the I.B.R.

The reasons for submission of the comments as "Rapid Communication" are self-evident in this case. In fact, the note submitted presents a series of mathematical, theoretical, and experimental references on the problem of the isotropy of space (i.e., of the rotational symmetry) that were not quoted in the note by Goldstein and Moravcsik. A rapid correction of the occurrence is therefore recommendable to avoid the appearance of additional papers in the field with major deficiencies in the listed literature. Additional reasons for the "Rapid Communication" are due to the apparent increase of experimental interest in the field. It appears therefore recommendable to provide the community with additional tests that seems to be better understood, more effective, amd readily feasible with available technology (neutron interferometry).

Very regrettably, recent extremes of decay of scientific ethics in the U.S. physics, particularly in refereeing, force me to submit this paper under legal assistance from the very beginning.

Please do not feel offended by this unsual form of submission. In fact, I believe that you, as Editor, are a victim of the decaying ethics of our community much more than the authors. At any rate, I put in writing in the past my unconditional faith in you as a person, and I confirm it here.

Also, I believe that Goldstein and Moravcsik were in good faith when they published their paper in your journal, and, under no circumstance, the legal assistance is due to their persons. In fact, I know them personally; I consider them highly; and a significant scientific exchange of ideas has been lately initiated among us.

Furthermore, please be reassured that I shall be most receptive to any constructively critical suggestion for the improvement of the paper submitted. To put it explicitly, in case an orderly, respectful, and effective scientific process is implemented in the consideration of the paper hereby submitted, it would be a point of honor for me to respond in a way as respectful and cooperative as possible.

The possible activation of the legal assistance is therefore solely restricted to refereeing practices that, lately, have become not unfrequent, such as: use of offensive language in referee reports; use of refereeing authority to delay the consideration process for 6 months to one year (or even more in certain known cases) to favor other groups or for other nonscientific objectives; manifest manipulations and distorsions of scientific truths in the apparent attempt to suppress at birth undesired advances; etc. etc. etc.

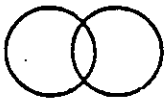
In the hope that we can unite forces to contain such ethical decays in the interest of America, I remain

Sincerely and Gratefully Yours

Ruggero Maria Santilli

cc. Mr. [redacted], Esquire
[redacted]

and
Drs. D. LAZARUS and G. TRIGG, Physical Reviews



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

March 31, 1983

Professors G. R. GOLDSTEIN, Tufts University and
M.J.MORAVCSIK, University of Oregon

Dear Professors Goldstein and Moravcsik,

I enclose copy of a paper presenting a few comments on your paper published in PR D25, 2934 (1982), which has been submitted to PRD as Rapid Communication.

Any constructively critical remark and/or advice you may have to achieve a better maturity of presentation would be gratefully appreciated.

Also, I would like to take this opportunity to inform you of the forthcoming I.B.R. meetings this summer (see enclosed announcements) from August 2 to 7, 1983 where our common interests on the tests of the rotational symmetry under strong interactions will be studied in all possible mathematical, theoretical, and experimental depth. In case you are interested to attend, you would be sincerely welcome.

Very Truly Yours,

Ruggero M. Santilli

cc: Dr. D. NORDSTROM, PRD

MICHAEL J. MORAVCSIK
• Theoretical Physics
• The Science of Science
• Science Policy and Development particularly in the Third World

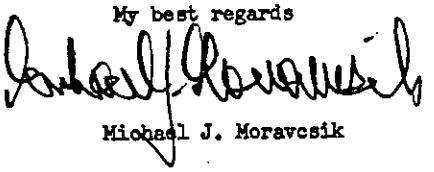
Professor Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street, Cambridge,
Mass 02138

April 6,1983

Dear Professor Santilli,

I want to thank you for sending me a copy of your paper :"Comments on the Note ..... Moravcsik". I admired your breadth of vision and coverage exhibited in that paper, and I really have nothing to add to it in response that would be worth printing. Perhaps the only comment I could make informally to you as the author is that it may be useful, at the end of the paper, to summarize the specific experiments you would urge. As the paper stands now, an experimentalist reading it would be awed but would probably be unable, on his own, to glean experimental guidance out of it.

My best regards



Michael J. Moravcsik

Copy: Professor Gary Goldstein

Mailing address: Institute of Theoretical Science, University of Oregon, Eugene, Oregon 97403 USA
Phone: (503) 686-5207 or (503) 344-1137. Messages at (503) 686-5204.
Telex: 510 597 0354

THE PHYSICAL REVIEW 719 D

PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES

1 RESEARCH ROAD [redacted] RIDGE, NEW YORK 11[redacted]

[redacted] 924-6533

21 June 1983

Dr. Ruggero Maria Santilli
The Institute For Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Re: Comments on "Polarization experiments and the isotropy of space"
By: Ruggero Maria Santilli

DDR231D

Dear Dr. Santilli:

The above manuscript has been reviewed by one of our referees.

Comments from the report are enclosed. We are returning the manuscript for your consideration of these comments.

Yours sincerely,

Stanley G. Brown
Editor
Physical Review D

F2

[redacted]ICAN PHYSICAL SOCIETY

Referee's Report on MS No. DDR231D, by R.M. Santilli.

This paper hardly qualifies as a "Comment" on the work of Goldstein and Moravcsik, Ref. 1, since the subject matters of the two papers have very little to do with each other, hence, it has to be viewed as a separate article.

The paper under review, however, contains no new material. Rather, it is a summary of some of Santilli's and collaborators' works, published elsewhere, on their modification of the rotation group. The author also claims that the experiments of Eder et al, also published elsewhere, support his theory of rotations; however, the claim is made without a detailed analysis of those experiments or without proposing new types of experiments to further test this claim.

Where the paper does contain specific comments regarding the work of Goldstein and Moravcsik, those are based on a misunderstanding of the framework in which the latter authors obtain their results. Contrary to Santilli's statement (see e.g. "Comments D"), the results of Goldstein and Moravcsik are not based on potential scattering or on the assumption of structureless target and projectile. The authors of Ref. 1 merely assume that a differential cross section is the modulus squared of a scattering amplitude; the latter, in turn, possesses all the invariance properties of the underlying theory. This approach is perfectly general and it is independent of the details of a theory as long as the theory in question is a quantum theory in which the principle of superposition holds.

In conclusion, this paper does not contain new results or comments relevant to the subject dealt with by Goldstein and Moravcsik, Ref. 1. Hence, it is not suitable for publication in Physical Review.

July 7, 1983

COMMENTS ON REFEREE'S REPORT ON THE PAPER NO.DDR231D, by R.M.Santilli, ACCEPTED AND RELEASED BY PHYSICAL REVIEW D

The statement by this referee
"....the subject matters of the two papers have very little to do with each other"
is manifestly false. Both papers (by Goldstein-Moravcsik and by Santilli) deal exactly with the same, single,issue: the tests of the rotational symmetry in particle physics.

The additional statement by this referee
"The paper under review, however, contains no new material."
is also manifestly false. The paper is the first to treat jointly and on a comparative way all (and not only some) possible tests of the rotational symmetry, as an essential pre-requisite for the future conduction of the tests themselves.Furthermore,the paper presents for the first time the main ideas of the Lie-isotopic lifting of the rotational symmetry and contains several other advances which need not to be identified here.

The additional statement by this referee
"...the claim is made ... without proposing new types of experiments..."
is also manifestly false. The paper proposes specifically and in all sufficient details three varieties of experiments, identified in page 8 and recalled in the final statements.

The additional statement
"... experiments by Eder et al.."
is also manifestly false. Eder is a theoretician. The experiments referred to (interferometric measures of the apparent, quite natural, deformation of the spherical charge distribution of neutrons in the intense fields of Mu-mtal nuclei, with consequential rotational-asymmetry) have been conducted since 1975 by H. Rauch et al, as repeatedly stated in the paper.

The additional statement by this referee
"This approach [by Goldstein-Moravcsik] is perfectly general and it is independent of the details of the theory in question..."
is also manifestly false. As explicitly stated in the paper, Goldstein-Moravcsik assume the conventional associative algebra with trivial associative product of matrices, functions, etc of type AB. But this is the SIMPLEST POSSIBLE (rather than the most general possible) realization of the associative product. The hadronic mechanics assumes instead the most general possible associative product of operators with realizations of the type $A*B = AgB$ where g is fixed (and verifies certain restrictions). In turn, it has been shown in the literature that an isotopic lifting of the enveloping algebra (with a parallel one for the Hilbert space) implies a generalization of the current"abstract"formulation of the scattering theory, including nontrivial deparures from the cross sections used by Goldstein-Moravcsik.

All these and other elements suggest rather strongly that the review is of nonscientific nature, that is, of the political character which is rendering the journals of the APS sadly known world wide. At any rate, the absolute, total, and complete lack of any constructive comment or suggestion to improve the paper establishes quite clearly the fact that this referee OPPOSES the experiments suggested in the paper and the appearance of the paper IRRESPECTIVE OF POSSIBLE IMPROVEMENTS. In short, we are evidently facing a situation of academic dances totally deprived of any scientific content whatsoever.

The basic motivation for the preparation of the paper and its submission to Phys. Rev. D must be recalled here. The submission resulted from the fact that the paper by Goldstein and Moravcsik failed to quote a rather massive literature in the topic pf their paper (test of the rotational symmetry), which, when combined with theoretical and mathematical efforts exceeds the 10,000 pages of published research!

It is evident that the leaving of this situation uncorrected will damage, first of all, Goldstein and Moravcsik. Second, it will damage the reputation of the PR at an international level, and last but not least, it will not serve the pursuit of novel physical knowledge.

It should also be stressed that the occurrence is PRIMARILY AN EDITORIAL PROBLEM, THAT IS, THE PRIMARY RESPONSIBILITY OF THE MASSIVE LACK OF REFERENCES RESTS IN THE EDITORS OF THE PHYSICAL REVIEW D.

Two possibilities can be foreseen for the solution of the problem.
ALTERNATIVE I. Goldstein and Moravcsik publish an Errata Corrige OR Addendum to their paper indicating the missed references. In this case the paper by Santilli will be withdrown, rewritten,and submitted to another (European) journal.
ALTERNATIVE II:Phys. Rev. D selects a true referee, that is, a referee interested in doing physics in the traditional way: submission of ideas and presentation of CONSTRUCTIVE criticism for their improvement. Reference is made here to the uncompromisable need that referee's reports indicate in all specific details the aspects that must be improved to reach maturity of publication. Complete silence on this point implies that the referee opposes the line of study of the paper. To be even more specific, the referee should indicate whether paper DDR231D should
- elaborate in more details the three varieties of experiments suggested;
- enlarge the novel parts on the isotopic lifting of O(3) and its capability to leave invariant all ellipsoidical deformations of the spherical charge distributions;
- modify in any desired/suggested way any other aspect.

On one point is is essential that Phys. Rev. reaches a clear understanding. Everybody is entitled to his/her own little politics. But there MUST be limits, even in the current absence of a Code of Ethics in Physics. In the case of the paper by Goldstein-Moravcsik, the missed quotations are simply too huge to be left unchallenged.

The continuation of the formal acceptance of nonscientific referees of the type accepted and released by Phys. Rev. D. will be taked for its face value: a provocation to turn the issue into a legal fight.

cc. Dr. Lazarus, Editor in Chief
Drs. Goldstein (Tufts Univ.) and Moravcsik (Univ. of Oregon)
Attorney J. Grassia, Boston

Encls.: Revised version of the paper.

July 14,1983

MICHAEL J. MORAVCSIK
• Theoretical Physics
• The Science of Science
• Science Policy and Development particularly in the Third World

Dr. Ruggero Santilli
Institute of Basic Research
96 Prescott Street
Cambr‡ dge, Mass 02138

Dear Dr. Santilli,

Thank you for sending me a copy of your comment on the referee's report on your paper that you were kind enough to send me a copy of earlier. I was sad to hear that you have had some difficulties with Phys. Rev. Let me know how things work out, but for the moment let me just make a few comments which may help to resolve the difficulties.

I see that one of the main points in the argument is the presumed lack of references in our original article. As you recall, that paper simply contained a rather simple point, pertaining to the experiments and their interpretation, and we did not feel it would be appropriate in that note to make a mountain out of a molehill and drag in the 10,000 pages of research on symmetries which are not really directly relevant to the article or contain results on which our note was built. We still think so, but of course this is a matter of opinion, and therefore we would by no means be opposed to submit an erratum or an addendum, containing a modest list of references supplied by you, and we would be happy to acknowledge your help in preparing that list.

It would, however, be preferable if your article, or some version of it, could be published in Phys. Rev. or some other journal, since it summarizes the background much more effectively than a list of references could in an addendum.

I have not had a chance to discuss the content of this letter with Gary Goldstein, who is at the moment at the Rutherford Laboratory, and in fact by the time he returns in late August, I will have left for Europe, to return only at the middle of September. So this letter is only my personal opinion, though I would expect Gary to concur with its content.

Sincerely

Michael J. Moravcsik

Copy: Gary Goldstein

Mailing address: Institute of Theoretical Science, University of Oregon, Eugene, Oregon 97403 USA
Phone: (503) 686-5207 or (503) 344-1137. Messages at (503) 686-5204.
Telex: 510 597 0354

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

July 19, 1983

Office of the President

Dr. M. J. MORAVCSIK
University of Oregon
EUGENE
Oregon 97403

Dear Dr. Moravcsik,

Please accept my appreciation for the courtesy of your letter of July 14, 1983. I am also grateful for your positive attitude. You can therefore trust on my own best possible attitude.

MISSING REFERENCES. They are the following.

FIRST PRIORITY REFERENCES: All the experimental papers by H. Rauch and his associates on the rotational symmetry of neutrons conducted since 1975. They are ref.s 25 through 29 of my note on your paper. These references (particularly the last one, ref. 29, on the latest results) refer specifically to the experiments you suggest. Their quotation is of utmost importance for all papers on the rotational symmetry, whether yours or mine.

SECOND PRIORITY REFERENCES. They are given by the theoretical studies by G. Eder on the apparent deformation of the spherical symmetry of the charge distribution of hadrons (ref.s 12 through 14 of my note on your paper). They provide a model of deformed nucleons for which SO(3) is manifestly broken. As such, they are directly related to your paper.

LAST PRIORITY REFERENCES. Are my own studies in the field, and you should not feel obliged to quote them. To put it explicitly, I have contacted Phys. Rev. on the issue as a representative of a scientific group, rather than for myself only. Perhaps, rather than quoting my papers (and those of additional researchers), you should consider quoting the Bibliography by M. L. Tomber (ref. 4 of my note on your paper), as well as the Proceedings of the Orléans International Conference (Ref. 5).

To avoid misunderstanding, none of us consider you and Dr. Goldstein directly responsible for the occurrence. In fact, we believe that you were in good faith, and that you simply were unaware of the amount of publications directly related to your paper. The entirety of the responsibility of the occurrence is seen to belong to the editors of Phys. Rev. D who were fully aware of the references.

POSSIBLE SOLUTIONS. Your indication of the possibility of publishing a brief Errata—Corrige or Addendum at Phys. Rev. D is seen as a confirmation of your good faith. In fact, a few lines would be sufficient, indicating that, following the publication of the paper, a number of references had been brought to your attention, and then quote the experimental papers first, followed by the theoretical ones. In case you publish these lines, I shall withdraw my own paper from Phys. Rev. D, rewrite it, and submit it to another journal as indicated in my recent letter to Phys. Rev. D.

There is another possibility you should consider. We can publish jointly a follow up paper. As you know well, following our meeting here at the I.B.R. and this correspondence, there are a number of technical aspects on your paper that need clarification, such as:

- The possibility that space is and remains isotropic even for a broken rotational symmetry. In fact, the breaking my indicate motion of extended particles within an isotropic hadronic medium without any connection whatsoever with the isotropy of space (as considered in your paper);
- The possibility that the breaking is due, quite simply and trivially, to a conceivable deformation of the spherical charge distribution of hadrons, as suggested since 1978;
- The possibility that, even in case the symmetry under conventional rotations is broken, the SO(3) symmetry is still exact. In fact, our isotopic lifting $\widehat{SO}(3)$ of SO(3) provides the invariance of all possible ellipsoidical deformations of the sphere, while being isomorphic to SO(3). Thus, the abstract rotational symmetry can be exact EVEN FOR UNISOTROPIC MEDIA AND DEFORMED SPHERES.

The purpose of my note on your paper submitted to Phys. Rev. D is to bring to the attention of the experimenter these and other facts. It is evident that, lacking their knowledge, the maturity of the formulation of the experiments you suggest is questionable. It is a question of scientific accountability.

Rather than publishing these comments on my own, I would be glad to join forces with you and publish them together. In this way, rather than appearing as a form of insufficiency of your work, the remarks acquire the meaning of further developments.

In case you are interested in this joint collaboration, simply rewrite and modify my note submitted to Phys. Rev. D in the way you wish, and let me have a copy. Additional papers on the isotopic lifting of rotations are enclosed. Additional information will be available at our summer workshops, where the issue will be discussed in considerable experimental, theoretical, and mathematical detail (a formal invitation for you and Dr. Goldstein to attend the workshops was mailed a time ago).

Sincerely,

Ruggero M. Santilli

RMS/mlw

cc: Dr. Goldstein and Phys. Rev. D

P.S. I shall leave soon after our workshops (on August 8) for a tour of lectures in Europe, and I contemplate to be back sometime in September 1983.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES
1 RESEARCH ROAD BOX 1000 RIDGE NEW YORK 11961
Telephone (516) 924-5533

12 September 1983

Dr. Ruggero Maria Santilli
The Institute For Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Re: Comments on ''Polarization experiments and the isotropy of space''
By: Ruggero Maria Santilli

DDR231D

Dear Dr. Santilli:

The above manuscript has been reviewed by one of our referees.

We are enclosing comments from the report, and are returning the manuscript.

Yours sincerely,

Stanley G. Brown
Editor
Physical Review D

F2-R

REPORT OF SECOND REFEREE

REPORT OF REFEREE

Manuscript Number: DDR231D

Author: Ruggero Maria Santilli

Title: "Comments on 'Polarization experiments and the isotropy of space'"

1. This manuscript is a comment on the paper of Goldstein and Moravcsik only in the limited sense that it points out literature citations apropos of the subject but omitted in the paper of Goldstein and Moravcsik.

2. Manuscript DDR231D is, for the most part (but not exclusively), an extended discussion of the work of the author and his group on theories of Lie-admissible extensions of mechanics and associated criticisms of rotational symmetry. This discussion is difficult to follow since the terms are not defined, and the treatment is not self-contained (referring to citations for key results). The material in this part of the manuscript is not suitable for publication.

Recommendation: Publication is <u>not</u> recommended.

September 27, 1983

Dr. S.G.BROWN, Editor, Physical Review D
P.O.Box 1,000, Ridge, New York 11961

RE: Comments on the second referee report of paper DDR231D entitled
"Comments on 'Polarization experiments and the isotropy of space'
by G.R.Goldstein and M.J.Moravcsik"

Dear Dr. Brown,

I believe that the dishonesty of the first referee report was beyond any reasonable doubt .

This second report must be praised for the use of clean language and arguments. Nevertheless, its end results raise the same doubts of the first: partisanship with established scientific interests; suppression of due scientific process; and insufficient scientific accountability of the journals of the APS vis-a-vis the American taxpayer.

The doubts on partisanship are evident. In fact, the second referee essentially rejects the paper because the basic aspects are defined elsewhere, by therefore preventing complete comprehension of the issue via only the paper submitted. IF the same editorial rule is applied to ALL papers submitted, it would lead to the suppression of the virtual entirety of papers published in PRD. In fact, NONE of the papers published (or, at best EXTREMELY FEW) are completely selfsufficient. Only review papers are conceived to be entirely self-sufficient, but then they are not published in PRD.

The doubts on lack of due scientific process are equally self-evident. In fact, the report is ONLY NEGATIVE, and FAILS TO INDICATE SPECIFICALLY THE IMPORVEMENTS UNDER WHICH THE PAPER MIGHT BE PUBLISHED. This is a quite widespread disease of the review process at the Journals of the APS, with the understanding that it is implemented only for papers of potential novelty, that is, papers potentially against established vested interests.

The doubts on insufficient scientific accountability are equally evident. The facts treated in the paper are incontrovertible and leave no room to academic dances.

1) Extended charge distributions (such as hadrons) are expected to be deformable under sufficiently intense external fields, as a consequence of which the magnetic moments of the particles are altered, and the conventional rotational symmetry is manifestly broken [results of ref.6 of paper].
2) Quantitative calculations of the effect have been conducted by Eder, leading to the expectation of about 1% rotational asymmetry for low energy (thermal) neutrons within the fields in the vicinity of Mu-metal (or similar) nuclei [ref. s12-14].
3) Direct experimental tests on the intrinsic rotational symmetry of neutrons have been conducted by Rauch and his associates since 1975 via neutron interferometry. Even though still preliminary, the latest and best available measures CONFIRM THE BREAKING OF THE CONVENTIONAL ROTATIONAL SYMMETRY EXACTLY IN THE 1% RANGE [25-29].

The APS has somehow managed to suppress the appearance of facts 1), 2), 3) in its journals. This has been achieved via referee reports of the type under consideration here (first and second). The creation of doubts on sufficient scientific accountability are then evident. In fact, how can topics of such fundamental nature be left without due scientific process, that is, without their PUBLICATION and subsequent critical examination, experimentally and theoretically, in other publications?

The implications are evident, not only for the entirety of the scientific and financial profile of basic research [evidently, because of the breaking of the rotational symmetry due to deformations of extended objects], but also for possible applications [evidently, because of the implications, say, for the attempts to reach magnetic confi-

nement of nucleons whose intrinsic magnetism can CHANGE with the approaching of the fusion conditions...].

I have repeatedly communicated these problems to the highest levels of the APS in other occasions. It is my opinion that, the later the existence of these problems is acknowledged, the bigger and more explosive will be an inhevitable crisis.

In fact, lacking any valuable scientific content in the referee reports, the only aspect left is the question: for how long can the suppression of facts 1), 2), and 3) be continued at the Journals of the APS?

Also, lacking any scientific content in the reports, the paper is resubmitted without modifications, jointly with a paper appearing elsewhere, in the flimsical hope that at least some members of the APS are indeed interested in due scientific process, e.g., to see better why mutation of shape and magnetic moment-and breaking of conventional rotational symmetry-may occur while hadrons conserve their conventional values of spin.

Very Truly Yours

Ruggero M. Santilli
96 Prescott Street
Cambridge, Massachusetts 02138

cc. Dr. D. Lazarus, APS
Dr. G.R.Goldstein, Tufts University
Dr. M.J.Moravsik, Oregon State University

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

Physical Review D
Editors
D. NORDSTROM
STANLEY G. BROWN

EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE, NEW YORK 11961
Telephone (516) 924-5533
Telex Number: 971599
Cable Address: PHYSREV RIDGENY

December 8, 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Manuscript No. DDR231D

Dear Dr. Santilli:

The above manuscript has been reviewed by Dr. Gordon L. Kane, in his capacity as a member of the Editorial Board of Physical Review D. We regret that in view of his comments (enclosed), we cannot accept the paper for publication. We are therefore returning the manuscript.

Sincerely yours,

Stanley G. Brown
Editor
Physical Review D

SGB/di
Enc.

Editorial Board Report on DDR 231D, Santilli

The reviewing of this manuscript seems to have been done in a responsible way by informed reviewers. I see no reason to modify their conclusions. One solution to the conflicting viewpoints seems to have been acceptable to all parties, and it solves the substantive problems, so I also recommend it-- namely, that Goldstein and Moravcsik publish an erratum listing a set of references; it would be suitable to cite several recent references, with a remark that earlier work can be traced from those.

John Kane

MAIL RECEIVED

DEC 0 6 1983

PHYS. REV. - P. R. L.

PHYSICAL REVIEW D VOLUME 25, NUMBER 11 1 JUNE 1982

Polarization experiments and the isotropy of space

Gary R. Goldstein
Department of Physics, Tufts University, Medford, Massachusetts 02155

Michael J. Moravcsik
Department of Physics and Institute of Theoretical Science, University of Oregon, Eugene, Oregon 97403

(Received 5 May 1981)

It is shown on an example that sensitive tests of the isotropy of space (i.e., of rotation invariance) in strong-interaction particle reactions are almost identical to tests of parity conservation, and hence the two can be confused without some additional experiments which we specify.

The test of various conservation laws connected with symmetries is a central concern in nuclear and particle physics both because of cosmological implications and because theories of particles themselves depend on such conservation laws. Rotation invariance (i.e., the isotropy of space) is a symmetry that is relatively rarely studied. Our present belief, for example, that space is isotropic with respect to strong interactions is not based on experimental information of very high precision.[1] The aim of this article is therefore to explore the type of particle reaction experiments which can test rotation invariance in strong interactions. The conclusions of the investigation can be summarized in three points:

(1) One can construct tests, by using polarization quantities that lend themselves to "null experiments," which can be performed to a reasonably high degree of accuracy, such as one part in 10^7.

(2) These tests are virtually identical with experiments which test parity conservation, and hence evidence for parity nonconservation can be easily mistaken for evidence for violation of rotaion invariance.

(3) There are feasible additional experiments which can distinguish between exidence for parity nonconservation and evidence for anisotropy of space.

It would be quite feasible to discuss this problem in the framework of a general formalism of polarization phenomena. For didactic reasons, however, it might be much preferable to select instead a simple reaction as an example. The nature of the discussion will be such that it should be evident to the reader that nothing essential hinges on the specific properties of the example reaction and that therefore the generalization to any other reaction is straightforward.

The reaction we choose as an example is $0+\frac{1}{2}\rightarrow 0+\frac{1}{2}$, where the 0 and $\frac{1}{2}$ denote particles with spins 0 and $\frac{1}{2}$, respectively. A specific instance of such a reaction may be elastic pion-nucleon scattering, but there are many other instances also throughout particle and nuclear physics. We will first discuss this reaction in the case when rotation invariance holds.

In that case, the M matrix can be written in the following form:

$$M=a_0+a_1\vec{\sigma}\cdot\vec{q}_1+a_2\vec{\sigma}\cdot\vec{q}_1\times\vec{q}_2+a_3\vec{\sigma}\cdot\vec{q}_2\ , \tag{1}$$

where q_1 and q_2 are the initial and final center-of-mass momenta, the a's are the reaction amplitudes which are complex numbers depending on kinematic factors, and $\vec{\sigma}$ is the usual Pauli spin matrix. This is one of the multiply infinite number of ways of writing the M matrix. From the point of view of our discussion, it makes no difference which of the ways of writing the M matrix we consider, and hence this one is used since it may be familiar to many of the readers.

The amplitudes a_i are functions of the rank-zero tensors one can construct from the vectors that determine the kinematics. In the present case these vectors are $\vec{q}_1$ and $\vec{q}_2$, and hence the rank-zero tensors are q_1^2, q_2^2, and $\vec{q}_1\cdot\vec{q}_2$. The fact that these three are not independent of each other is of no concern to us in the present discussion. It is important to note, however, that all three of these rank-zero tensors are scalars and not pseudoscalars.

Now let us impose, in addition to rotation in-

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RAPID COMMUNICATIONS
DDR231D
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PREPRINT OF THE INSTITUTE FOR BASIC RESEARCH NUMBER IBR-DE-83-4

PAC NUMBERS 11.30.-j; 11.30.-Er; and 21. 03.65.-w

quotes

COMMENTS ON THE NOTE "POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE" BY G. R. GOLDSTEIN AND M. J. MORAVCSIK

Ruggero Maria Santilli*
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

(RECEIVED APRIL 1983)

Abstract

A valuable and courageous note by G. R. Goldstein and M. J. Moravcsik on possible tests of the rotational symmetry under strong interactions has been recently brought to our attention. In these comments we indicate possible additional tests, as well as references on the problem that were apparently unknown to the authors at the time of writing their note.

NOTE OF JULY 7, 1983: THIS IS AN IMPROVED VERSION.

*Supported by the U. S. Department of Energy under contract number DE-AC02-80ER10651.A002.

PART XIX:

PHYSICS

LETTERS

(CORRESPONDENCE

WITH HOVARD

GEORGI)

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 22, 1983

Professor R. GATTO
Editor, Physics Letters B
CERN
CH-1211 GENEVA23, Switzerland

Dear Professor Gatto,

I submit the enclosed note entitled
"Use of hadronic mechanics for the regaining of the exact space-reflection symmetry in weak interactions"
for publications in your Journal.

The paper has not been submitted to other Journals nor will be submitted during your consideration. The copyrights on the note are assigned to North-Holland Publishing Company in case of publication.

The note complies with the restrictions on length set forth by your Journal, to my understanding. If this is not the case, Physics Letters is authorized to eliminate entirely footnote 11.

For your convenience, I enclose copies of the galleys of ref.s 1b and 1c that might not be available in Geneva at this time.

Any critical remark for the improvement of the presentation would be gratefully appreciated.

I am currently working on two additional notes:
- one on the use of hadronic mechanics in Kalnay's realization to achieve a "strict confinement" of quarks (identically null probability of tunnel effects for free quarks), while leaving current quark theories virtually unchanged; and
-one on the use of hadronic mechanics to achieve convergent perturbative series when divergent at the quantum level.

In case of interest by your Journal on these efforts, it would be a sincere pleasure to submit them to you.

Very Truly Yours

Ruggero Maria Santilli

PHYSICS LETTERS B

HOWARD GEORGI
Physics Department
Harvard University
Cambridge, MA 02138
U.S.A.
Tel: 617-495-3908

December 13, 1983

Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

This paper draws so heavily on your earlier works (which are not widely known) that it cannot be made sufficiently self contained to warrant publication in the letter format. It is not suitable for Physics Letters B.

Sincerely,

Howard Georgi
Editor

HG:pcc

enclosure

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Cables: PHYSLET Amsterdam — *Telex:* 18582 espa nl — *Telephone:* 020-762013

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

December 15, 1983

Dr. H. Georgi, Editor
Physics Letters B
Department of Physics
Harvard University
Cambridge, Ma 02138

RE: manuscript # 1117

Dear Dr. Georgi,

Absolutely none of the papers you have accepted for Physics Letters B is "sufficiently selfcontained" to be understandable without a knowledge of the quoted literature, nor any letter can reach such a status these days. As a result, there exists absolutely no difference between the papers you routinely accept, and the paper submitted. In actuality, the latter paper requires the knowledge only of the literature quoted in ref. 1 [the papers printed in Lettere Nuovo Cimento], copies of which were enclosed with the original submission. Your rejection therefore has absolutely no visible scientific-editorial grounds.

Most regrettable are the implications of your rejection for a number of developments dependent on the paper submitted, such as the achievement of a strict form of quark confinement [identically null probability of tunnel effects for free quarks] via the use of Kalnay's quantization of Nambu's mechanics for the triplet case, that is emerged as being a particular realization of hadronic mechanics.

As communicated in the original letter of submission mailed to CERN, these latter developments were contemplated for submission to your journal. They are expected to constitute a primary topic of study at the forthcoming Second Workshop on Hadronic Mechanics [see copy of the announcement here enclosed]. In particular, they constitute one of the primary motivations for which the Hadronic Journal was founded.

A rejection of the paper without scientific-editorial grounds would imply a necessary revision of all these programs, for which you must assume the responsibility. Before doing that I want to give you a second, final chance of re-examining the paper and submitting it to a due scientific process. On my part I shall be glad to cooperate for all scientifically warranted revisions.

Very Truky Yours

R.M.Santilli

encls.

PHYSICS LETTERS B

HOWARD GEORGI

Physics Department
Harvard University
Cambridge, MA 02138
U.S.A.

Tel: 617-495-3908

February 2, 1984

Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Ruggero,

I have looked at your paper again, but I really don't know where to start trying to fix it. There are two problems, not unrelated. The first is the jargon. You have invented your own, which you have the right to do. But you have not tried hard enough to make connections to more conventional ideas. This makes the paper locally very hard to follow. The second problem is that even when the paper makes sense locally, it is not clear what is your overall plan. Unless the purpose and conclusions of the paper can be stated without reference your other works, it is not suitable as a letter.

Now let me write frankly, as a friend. I do not know whether your whole program makes any sense because I have not studied it deeply enough (although people I respect have studied it and claim that it doesn't). But I do know that if you really believe in it, then you are going about trying to convince others that it makes sense in the wrong way. Instead of basing your work on large papers full of jargon, you should start over completely from scratch. You should write a short self-contained introductory paper, completely free of jargon, historical references, etc.--concentrating on the physics which you are trying to address.

If you continue writing papers such as this one, you won't get anywhere. To any reader who did not already share your point of view, this paper would look like an elaborate mathematical ediface constructed out of random definitions. Of course, lots of things look like that at first which turn out to be interesting. Your paper may be one of them. But in its present form, it will only encourage readers to think that you are hiding behind jargon because you don't really have any thing to say. That doesn't do you or the readers or Physics Letters any good at all.

Sorry that I can't be of more substantive help, but I hope you will take my suggestions in the right spirit. They are well meant.

Sincerely,

Howard Georgi
Editor

HG:pcc

enclosure

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

February 7, 1984

Howard Georgi, Editor
Physics Letters
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Howard,

Please accept the sentiments of my sincere gratitude for your constructively critical comments regarding my note "Use of hadronic mechanics for the ...". Please be reassured that, when constructive and therefore performing a scientific process, I am sincerely grateful for any critical comment, no matter how harsh.

I am in full agreement with you that the letter is not suitable for publication in its current form, and needs rethinking and re-writing. However, I share only in part your view. In particular, I have difficulty is seen lack of discrimination between our scientific current [which is, by now, fully established no matter what other people say], and conventional trends when referring to the self-containing character of the letter and the absence of prior reference. If I have to do it, then exactly the same rule must be applied to, say, a paper on SU(5)!

Nevertheless, you are perfectly correct in asking that the physics to be addressed must be identified as clearly as possible. It is in this point where you can contribute significantly for a due scientific process. You are familiar with our objectives, but let's review them.

We believe that hadronic mechanics can:
(A) provide a strict confinement of quarks, that is, a theory with an identically null probability of tunnel effect for free quarks [see announcement of our second Workshop at Villa Olmo nest August];
(B) permit the identification of the quark constituents with ordinary electrons and positrons, although obeyind a generalized mechanics because of the generalized forces occurring from conditions of deep mutual penetration of their wave-packets [see the Hadronic J. Vol. 1 number 2, 1978]; and, last but not least;
(C) provide realistic hopes of re-establishing the exact character of space-time symmetries when quantum mechanically broken, via their more general Lie-isotopic formulation. Similar results are expected for internal symmetries. In particular, the conventional and isotopic symmetries result to be locally isomorphic as established for the rotational, Lorentz and parity in 1982-1983, and for SU(N) symmetry by Mignani very recently.

I believe that the paper submitted to Physics Letters should be restricted to its physical objective, as specifically identified beginning from its title. In fact, it is a mere individual link in our program. Its enlargment to include topics (A) and (B) would be inappropriate, in my view, although I might be wrong in such thinking. At any rate, an indication of aspects (A) and (B) as possibilities, prior to their actual achievement could be inappropriate.

By keeping these various aspects into consideration, I would like to re-write the letter along the following main lines

(1) Eliminate all past references, with the sole exception of ref. 3 on the Proceedings of the First Workshop on Hadronic mechanics, where the existence

and self-consistency of hadronic mechanics have been established, of course on formal grounds only, but beyond reasonable doubts;

(2) reduce the jargon to the truly, absolutely essential parts, which are three notions, those of isoenvelope, isofield and isohilbert space, by providing in footnotes information for their speedy identification in the current ref. 3; and

(3) elaborate in more detail the achievement of states with the right mixture of conventional parity nonconserving states, which is only indicated as possible in the current version via the use of the "isotopic element" of hilbert product [how can you call it with an old jargon if it does not exist?]. This last point would render truly visible the regaining of the exact P-symmetry, evidently, because the conventional and hadronic formulations would be equivalent for all practical computational needs. I might add comments on our future hopes to achieve objectives (A) and (B), but only if you advise me so.

But above all, the objective of the note is to focus the attention on the role of the unit operator which, in turn, is the true, ultimate basis for (A) and (B).

Kindly advice me whether a reworking of the note along points (1), (2) and (3) would make sense, or you would still disagree on the general lines. This would save me considerable time, and I would have additional reasons to be grateful to you.

Please feel free to call me, if you so desire. I could brief you on our progress in objectives (A) and (B).

Sincerely,

Ruggero M. Santilli

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

February 7, 1984

Professor T.D.LEE
Columbia University
Department of Physics
NEW YORK, N.Y. 10027

Dear Professor Lee,

I would gratefully appreciate your comments for the improvement of the enclosed note entitled "Use of hadronic mechanics for the possible regaining of the exact space-reflection symmetry in weak interactions".

The note has been submitted to Howard Georgi as editor of Physics Letters. Howard has rejected the note for insufficient maturity due to the use of excessive new jargon that is specialized in our line of inquiry, as well as insufficient focusing of the physical problem to be addressed. I agree with Howard that the note is immature in its current version and I have written him a note of sincere thanks for his constructively critical comments.

Nevertheless, I have difficulties in rewriting the note without our terminology and reference. It would be the same as asking the author of a letter in SU(5) to write it without any reference to past contributions in the field ! Similarly, I believe that the problem is fully identified in the note beginning with its title.

I was planning to rewrite the paper: (A) by eliminating virtually all references to our studies, except ref. 3 [on the Proceedings of the First Workshop on Hadronic Mechanics, where the formal, theoretical existence and consistency of hadronic mechanics has been established, I believe, beyond any reasonable doubt]; (B) by providing footnotes for the speedy identification in ref. 3 of all essential definitions [which are basically three, those of isoenvelope, isofield and isohilbert space]; and (C) working out the problem left open in page 4, to the effect that the states are indeed of the right mixture of conventional parity-nonconserving ones.

Do you think that such revisions make sense? Could you kindly express any criticism that has escaped both Howard and myself?

For your information, this note is a oreliminary steps toward a subsequent ongoing step, the proof that hadronic mechanics provides a strict confinement of quarks, that is, a true, identically null probability of tunnel effect of free quarks, while leaving the quark theory essentially unchanged.

Studies to this effect are in progress. Nevertheless, you can anticipate them from the enclosed note. In fact, the strict confinement is expected from the incoherence of the Hilbert spaces for the interior and the exterior problem, when the former is realized according to hadronic mechanics, and the latter is realized as in conventional quantum mechanics. The preservation of the quark theory as currently known is expected from the isomorphism of the conventional SU(3) and its image under isotopy.

In case you are interested in inspecting any of the existing literature, please let me know. I would be glad to let you have a complimentary copy of the Proceedings of our recent workshop.

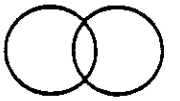
Sincerely Yours,

Ruggero M. Santilli

THIS LETTER WAS NEVER ACKNOWLEDGED.

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859



June 18, 1984

Dr. H. GEORGI
Editor for the U.S.A.
PHYSICS LETTERS
Harvard University
Department of Physics
Cambridge, Massachusetts 02138

Dear Dr. Georgi,

I must have a record of my doubts regarding the [illegible] of your recent refereeing for your journal.

YOUR FIRST REJECTION. Your rejection of my paper [1] is not credible. The paper presented a conjecture regarding a possible pulsating structure of the Coulomb law for electron pairs whose consistency has been proved beyond reasonable doubt for the nonrelativistic case. The motivation for your rejection was that the theory is not extendable to relativistic setting, in your view. This is not credible on a number of counts, such as, for instance, the fact that all known theories which are consistent nonrelativistically, have been proved sooner or later to admit a consistent relativistic extention. Besides, the job is under way. How can you claim it cannot be done before doing it? Perhaps, the true motivation of your rejection must be searched outside the pursuit of novel physical knowledge. At any rate, the paper you rejected was routinely accepted and published by a European letter journal.

YOUR SECOND REJECTION. Your second rejection is truly incredible by all standards. In substance, your letters of rejections of December 13, 1983 and February 2, 1984 state that you have rejected my paper because you have heard around in academic corridors that the hadronic generalization of quantum mechanics has no physical value. This is a sentence stated by senior physicist at your department since 1978, as you are well aware and know well from the extreme occurrences regarding my visit there in 1977-1980. The pertinent question here is the following: have you appraised the ethical standards of the colleagues you heard in academic corridors on the soundedness of the new mechanics? I do not believe you did, and there are reasons to expect you did not do it, particularly if you are financially affiliated with them on grants and other matters.

The additional thing you ask is truly incredible. I am referring to your request that the paper be completely self-contained without any quotation of preceding work. It is evident that absolutely no paper you have passed for your journal has met these requirements even minimally. You therefore practice a selective kind of refereeing,

with manifest leniency for certain types of conjectures aligned with your line of vested interests, and a different type of refereeing for conjectures and/or their authors outside said circle of interests. But then, under these premises, you are compelling even your best friends to enter into a severe judgment of your editorial work.

HARVARD'S APPARENT CONTROL OF PHYSICS LETTERS FOR THE U.S.A. In the name of our former friendship and associatiation, permit me to convey to you most candidly, primarily in your own interests, that the premises for your editorial post at Physics Letters are wrong. They are wrong for you in the long run. They will inevitably be wrong for Harvard, and they are definitely wrong for the printing house of your journal. I am referring to your totalitarial control of ALL publications in your journal originating in the U.S.A.

This situation is becaming more and more known in the trade, and is creating an increasing concern. It is established beyond a reasc nable doubt in my case, as well as in numerous others. In fact, I did not want my second paper be refereed by you and therefore mail it to the editorial office of your journal in Geneva and, in particular, to the European editor Dr. GATTO. My failure to have the second paper considered by ANOTHER editor of Physics Letters OUTSIDE HARVARD UNIVERSITY establishes your absolute control of U.S. submissions to your journal.

This is wrong. It cannot be otherwise.

Best Regards and Good Luck!

Ruggero M. Santilli
RMS-mlw

PACS NUMBERS 03.65.–w; 036.65.Bz; 11.30.–j

USE OF HADRONIC MECHANICS FOR THE POSSIBLE REGAINING OF THE EXACT SPACE–REFLECTION SYMMETRY IN WEAK INTERACTIONS

Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street, Cambridge, Massachusetts 02138

Abstract

It is shown that the isotopic lifting of the enveloping associative operator algebra, of the field and of the Hilbert space of quantum mechanics into those of the covering hadronic mechanics offers realistic hopes of regaining the exact space–reflection symmetry when quantum mechanically broken by weak interactions.

PART XX:

LETTERS

IN

MATHEMATICAL

PHYSICS

LMP LETTERS IN MATHEMATICAL PHYSICS

A Journal for the Rapid Dissemination of Short Contributions in the Field of Mathematical Physics

Editors:
M. FLATO, *Dijon*
M. GUENIN, *Geneva*
R. RĄCZKA, *Warsaw*
J. SIMON, *Dijon*
S. ULAM, *Boulder*

Postal address:
Physique Mathématique
Université de Dijon, B.P. 138
F-21004 Dijon, Cédex (France)

M. GASPERINI
Istituto di Fisica Teorica
Universita di Torino
Corso M. D'Azeglio 46
10125 TORINO ITALY

Dijon, March 19, 1984

Dear author,

Your paper, entitled : "Lie osotopic lifting of general relativity" has been examined by one of our referees, who made the following remarks :
"This paper should be submitted to the Hadronic Journal because, it is based on the idea of "Lie isotopic generalization of Lie theory" developed in that Journal and incomprehensible to those who do not study the papers of R.M. Santilli".

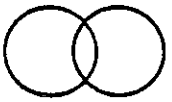
Unfortunately, in view of these remarks, we cannot accept your paper for publication in LMP.

Sincerely Yours,

J.C.CORTET

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859



May 23, 1984

Dr. J.C.CORTET,
Editor
LETTERS IN MATHEMATICAL PHYSICS
Physique Mathematique
Universite de Dijon
DIJON, France

Dear Dr. Cortet,

I hereby respectfully submit for consideration by your journal the enclosed letter in three copies entitled

"Use of hadronic mechanics for the possible regaining of the exact space-reflection symmetry in weak interactions".

The note is not under consideration at other journals, nor it will be submitted to other journals during your consideration process. The copyrights of the letter, if published, will be granted to your journal.

Very Truly Yours,

Ruggero M. Santilli

RMS-mlw

encls.

LMP LETTERS IN MATHEMATICAL PHYSICS

A Journal for the Rapid Dissemination of Short Contributions in the Field of Mathematical Physics

Editors:
M. FLATO, *Dijon*
M. GUENIN, *Geneva*
R. RĄCZKA, *Warsaw*
J. SIMON, *Dijon*
S. ULAM, *Boulder*

Postal address:
Physique Mathématique
Université de Dijon, B.P. 138
F-21004 Dijon, Cédex (France)

Professor R.M. SANTILLI, Editor in Chief
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
CAMBRIDGE, Mass. 02138 USA

Dijon, June 22, 1984

Dear Professor Santilli,

Thank you for your letter dated May 23. I am able to assure that the competency and the integrity of the referee are not suspicious. I submitted your comments and your paper to the editorial staff of LMP.

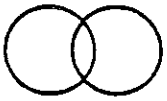
Unfortunately his decision is to not consider it for publication in our journal.

Sincerely Yours,

J.C.CORTET

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859



July 18, 1984

Dr. J. C. CORTET
Letters in Mathematical Physics
Physique Mathematique
Universite de Dijon, B.P. 138
F-21004 DIJON CEDEX, FRANCE

Dear Dr. Cortet,

I acknowledge receipt of your letter of June 22 declining the consideration of my paper "Use of the hadronic mechanics for the possible regaining of the exact space-reflection symmetry in weak interactions."

Unfortunately, facts speak for themselves:
1) the paper was particularly suited for your letter journal;
2) you declined consideration of the note; and
3) your declination was done via an absolute and total lack of any scientific content.

These facts point quite clearly toward mumbo-jambo academic politics as the most plausible explanation of the occurrence.

I shall reserve the option to disclose publicly and internationally all the correspondence on this case at the time I consider it most appropriate.

Very Truly Yours

Ruggero M. Santilli
RMS-mlw

ALPHA PUBLISHING
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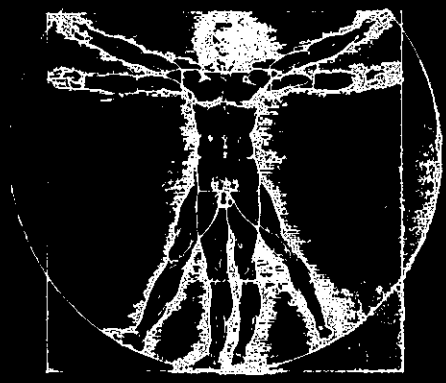
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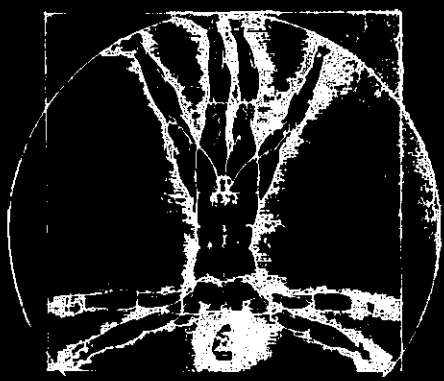
III

DOCUMENTATION

OF

IL GRANDE GRIDO





Ruggero Maria Santilli

DOCUMENTATION

OF

IL GRANDE GRIDO

Volume III

Ruggero Maria Santilli

— 1984 —
Alpha Associates
Rome, Italy

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DOCUMENTATION
OF
IL GRANDE GRIDO
VOLUME III

by

Ruggero Maria Santilli

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PART XXI:

REJECTIONS BY THE NATIONAL SCIENCE FOUNDATION DURING THE PERIOD 1972–1978

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

SEP 221972

Dr. Ruggero M. Santilli
Department of Physics
Boston University
Boston, Massachusetts 02215

Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Investigations on a New Analytic Extension of the Scattering Amplitude."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

Andrew W. Swago

Andrew W. Swago
Acting Division Director for
Mathematical and Physical Sciences

Copy to:
Dr. Robert F. Slechta
Associate Dean
Graduate School

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUL 16 1975

Dr. Ruggero M. Santilli
Department of Physics
Boston University
Boston, Massachusetts 02215

Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Investigation of Generalized Analytic, Algebraic and Statistical Formulations for Interacting Systems."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

Andrew W. Swago

Andrew W. Swago
Acting Division Director for
Mathematical and Physical Sciences

Copy to:
Mr. Charles W. Smith
Vice President for Finance

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 28 1976

Dr. Ruggero M. Santilli
Department of Physics
Boston University
Boston, Massachusetts 02215

Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Investigations on the Origin of the Gravitational Field."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other proposals which you might wish to submit.

Sincerely yours,

William E. Wright

William E. Wright
Division Director
for Physics

Copy to:
Mr. Henry T. Spiers
Comptroller

October 28, 1976

Dr. Boris J. Kayser
Division Director For Theoretical Physics
National Science Foundation
Washington, D.C. 20550

Dear Dr. Kayser:

I hereby submit for consideration by NSF my research grant proposal entitled "Necessary and sufficient conditions for the existence of a Lagrangian in Newtonian Mechanics and in Field Theories".

I also enclose a list of scientists who have been exposed to my current research interests hoping that it might be of some value in your referee selection.

Finally, I enclose samples of my papers on the subject of the proposal, which will appear in Annals of Physics to indicate the status of my research. These papers were done during my visit at the Center of Theoretical Physics of the Massachusetts Institute of Technology thanks to the kind hospitality by Professor F.E. Low.

Sincerely yours,

Ruggero Maria Santilli
Associate Professor

RMS/cc

cc: Dr. A. Isaacson
Encl.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139
Center for Theoretical Physics

Dr. Boris Kayser,
Program Director for Theoretical Physics
National Science Foundation
Washington, D.C. 20550

December 22, 1976

Dear Dr. Kayser,

I am contacting you to provide additional materials and informations in relation to my research grant proposal

"Necessary and sufficient conditions for the existence of a Lagrangian in Newtonian Mechanics and in Field Theories"
NSF No. 7703963

Since the last several years, I have been involved in a long term and laborious study of certain methodological aspects of theoretical physics which I hope to bring in due time up to the level of practical applications, particularly in high energy physics.

In line with my proposal, my studies consist of the following three phases:

1: The Inverse Problem in Newtonian Mechanics.

This problem basically consists of: a) the identification of the necessary and sufficient conditions for the existence of a Lagrangian for the representation of systems with arbitrary Newtonian forces; b) the methods for the construction of a Lagrangian from the given equations of motion; and c) an analysis of the significance of the underlying methodology for other aspects of the theory, e.g. the transformation theory.

I have been deeply involved in writing a monograph on this subject. This project is at a rather advanced stage as a result of several redraftings following the advice of my referees (P.Dedecker, R. Hermann, P.Huddleston, A.C.Hurst, H. Rund, S. Shanmugadhasan, A. Shimony and two of my graduate students). A copy of the currently available version of the manuscript is enclosed for your inspection.

The MIT Press has expressed interest in publishing my manuscript upon its finalization. Due to the poor mastering of the English language I still have, the manuscript needs a severe editorial control. I am pleased to report to you that Dr. Denis Nordstrom, Acting Editor of the Physical Review (after Pasternack departure) has accepted and initiated such editorial control. This project seems therefore proceeding along promising lines.

I called Kayser on March 16, 197 , to visit him at his office during my next trip to Washington. I said to be too busy to receive me

On historical grounds, you might be interested to know that, to the best knowledge of several experts contacted by me as well of myself, there is no recent account of this problem in both the mathematical and physical literature. I therefore initiated a detailed and laborious search of the prior state of the art which I conducted in all the science libraries of the Boston area as well as in the Library of Congress, by moving backward in time up to the beginning of the past century. All my findings will be reported in the forthcoming monograph. Basically, I discovered that the problem had been rigorously formulated on fashinating intuitional grounds by H. Helmholtz in 1887 and subsequently virtually solved within the context of the calculus of variations in specialized mathematical journals of the first part of this century. Regrettably, however, since that time the problem had remained virtually ignored.

On pedagogical grounds, the monograph appears to be potentially significant for the intended audience of first or second year graduate students. This is so because the Inverse Problem constitutes one of the best arenas for an in-depth study of the fundamental analytic equations, namely, the Lagrange's and Hamilton's equations. As one referee put it "... after Santilli work, no student will be able to claim a knowledge of the Lagrange and Hamilton equations without a knowledge of the necessary and sufficient conditions for their existence". Additional referee's reports are enclosed for your consideration. For any additional information please feel free to contact Mr. A.B.Evans of The MIT Press.

On technical grounds, the monograph apparently solves one of the central and vexing problems of Newtonian Mechanics. The conventional Lagrangian representations of Newtonian systems are virtually restricted to only conservative systems due to lack of knowledge on how to construct the Lagrangian for the case of more general Newtonian forces. This, however, often represents only a crude approximation of the Newtonian reality. To put it quite candidly, while I was teaching a graduate course in Classical Mechanics according to the conventional patterns I felt like the inventor of the machine for the perpetual motion. As a matter of fact, one of the primary motivation to undertake this laborious task was precisely my uneasiness with currently available methods. By looking in retrospective, I am now satisfied of my efforts. Indeed, I am now in a position to compute the Lagrangian for Newtonian systems as they actually are in the physical reality and not only in their conservative approximation.

In line with my application, I must add that this analysis demands, for completeness, the study of its extension to the case of Newtonian systems with generally non-integrable subsidiary constraints. This is part of my contemplated subsequent research.

2: The Inverse Problem in Field Theory

My Newtonian studies attracted the attention of the MIT in 1975 where I was then invited to be a visiting scientist since Jan. 5, 1976 thanks to the interest by Professors H. Feshback and F. E. Low.

During this calendar year here at MIT I have worked on a series of seven papers on the extension of the Inverse Problem to classical relativistic field theories. The first three papers will appear in Annals of physics; papers IV and V are currently under inspection by leading scientists prior to their submission to Annals of Physics; and papers VI and VII are under finalization.

Copies of these papers are enclosed for your consideration. There is a possibility that the MIT Press might be interested to reprint them in due time as a follow up to the first volume on the Newtonian aspect of the problem. If this project will materialize, I intend to dedicate the volume to my teacher and friend Professor Paul Roman. It is in this spirit that they are presented to you as a collection.

The papers are intended for a broader audience, rather than for few experts, in view of their potential technical as well as pedagogical significance. This is reflected in the adopted stile of presentation. After all, the Inverse Problem is again the best arena for an in-depth study of the fundamental analytic equations of all our field theoretical models. As the referee of Annals of Physics put it in his official report, "Santilli has performed a real service in reviving beautiful old ideas and extending them to field theories. Such scholarly virtue is rare these days and is very important".

The technical content is here perhaps multifold. First of all there is the intrinsically significant Lagrangian representation of Lorentz covariant systems of field equations with arbitrary couplings. Secondly, as you can see from papers I and II, by matching my field theoretical and Newtonian analyses, the unified gauge theories of weak and electromagnetic interactions emerge with a new light because their Newtonian limit results to be precisely of non-conservative type as it is after all self-evident from the velocity dependence of their couplings. In my opinion this indicates that forces which are not derivable from a potential have a precise physical role at both a Newtonian as well as a field theoretical level. If one searches for further generalizations of the couplingsaiming at the inclusion of the strong interactions, then the Inverse Problem emerges with a self-evident potential significace.

Some of the most intriguing implications of the Inverse Problem appear to be within the context of the transformation theory. See in this respect

paper V. I should indicate in this context that I purposedly avoided any elaboration and application of these results in these introductory papers.

3: Applications to High Energy Physics

A part from the few technical points I indicated in my proposal, it would be simply presumptuous for me to ventilate at this time possible results prior to their appearance. This is ultimately the fickle nature of a research proposal where often the author, for professional attitude or unpredictable turns of events, either cannot fully disclose his ultimate objectives or cannot predict, more often, its outcome.

I think however that it is appropriate for me to indicate to you that the ultimate motivation for my undertaking this long term, time consuming and laborious program, is precesely my personal conviction of the possible significance of the Inverse Problem in high energy physics.

In any case, I am now very close to the completion of my "homeworks" 1 and 2 and I will soon dedicate myself entirely to this third phase of my studies.

In closing, I would like to recall to you that I have already applied to the NSF for research support in the past but unfortunately the NSF was not in a position to fund my proposals.

More specifically, I applied for the first time in 1972 with a proposal on the study of the analyticity properties of the scattering amplitude. These studies resulted in my paper in Phys. Rev. D10, 3396 (1974) as well as several others, in which I reached the generalization of the PCT theorem to all discrete space-time symmetries. I understand that this paper, which several collegues consider rather highly, is currently used in various fields ranging from the discrete symmetry violations to the analyticity properties of the S matrix.

I then applied for the second time in 1974 with two proposals. The first one was related to a feasibility study to ascertain whether with the present technology it is possible to experimentally verify or disprove the central prediction of the Eistein-Maxwell theory according to which any distribution of electromagnetic fields generates a gravitational field. In subsequent correspondence with NSF I stressed the need of support for the continuation of these studies because, unlike my application of 1972, they required the set up of a team of experts in various disciplines. And indeed, various experts had agreed to rather enthusiastically participate. But on June 1976 I received a letter from Mr. W. E. Wright to the effect that the NSF was unable to fund my proposal. Regrettably, I had to abandon this project.

My second proposal of 1974 was closely related to my recently submitted proposal although at that time the presentation was predictably embrionic in nature.

Almost needless to say, I have recalled the above proposals for the sole intent of assisting you in your identification of my previous contacts with NSF.

Thrusting in your understanding, I would like to disclose to you that I am currently considered for a position here at MIT and at Berkeley. I understand that a faculty decision will be reached sometimes in February-March 1977. I would be sincerely grateful to you if any decision can be reached on my application by that time. If this is too early, I would appreciate the courtesy of an indication of the anticipated time of the decision.

Thanking you for your consideration and with my most sincere best wishes for the coming holidays, I remain

Very Truly Yours

Ruggero Maria Santilli

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE. MASSACHUSETTS 02139
Center for Theoretical Physics

March 14, 1977

Dr. B. Kayser,
Division of Theoretical Physics
NSF
Washington, D.C.

Dear Dr. Kayser,

following my phone call of March 7, 1977 and according to your suggestion, I am indicating in a letter the reasons for my request of a meeting to discuss my pending application No. NSF7703963.

By separate parcel post, I have mailed to you copies of my three monographs on the Inverse Problem, MIT-CTP publication Numbers 606, 607 and 608.

I would appreciate whether you can return to me the copies of the previous drafts, because now obsolete, which I mailed to you on December 22, 1976 (although I do not know whether you received them).

In my pending application I indicated the appearance of these monographs. As a matter of fact, the application was for financial support primarily in the writing of these monographs. I would appreciate the courtesy of your mailing a copy of these manuscripts to the referees of my application. Just let me know how many you need and I shall send them to you by return mail.

The reason for such a request is that, understandably, I have exposed myself to the physics community with such an announcement. Your mailing of the copies of my manuscripts would give the opportunity to the referees to inspect my results.

Secondly, I would appreciate your advice as to whether I should rewrite the proposal or leave it as it is. In essence, the basic research aspect of the pending proposal is by now completed, while the part of the proposal related to possible applications of the Inverse Problem to High Energy Physics remains in full effect. I personally prefer leaving the proposal as is, although I considered advisable to bring to your attention this new situation. Ultimately, I shall follow your advice "ad litteram".

I enclose on confidential grounds copy of the report to the CTP here at MIT on my trip to Washington of March 9 through 12. Hopefully, this report should provide you with an indication of the possibilities of the Inverse Problem.

Please let me know whether it would be appropriate for me to submit to your division the program (b), namely, that discussed with NASA, or some other of the contemplated applications of the Inverse Problem.

In closing, I would like to recall a phone conversation with Dr. R. Isaacson of some two years ago in which I stressed my full confidence on your capabilities, indicated my understanding of the difficult situation in which you operate and at the same time I indicated my reservation as to whether the current rules and regulations in which you are forced to operate are actually the best for the best interest of the Country.

In case your division will be involved sometime in the future in any revision of the current rules and regulations aiming at a more democratic dispersal of the available funds, please keep me in mind. You will have my unconditional support.

Sincerely Yours

Ruggero Maria Santilli

c.c. Dr. R. Isaacson

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139
Center for Theoretical Physics

March 14, 1977

Dr. E. Creutz
Division of Mathematical Science
NSF
Washington, D.C.

Dear Dr. Creutz,

I would like to express my appreciation for your kind reception during my visit at your Division on March 11, 1977.

I am enclosing, on fully confidential grounds, copy of the report to the Center here at MIT on my trip to Washington hoping that it might indicate to you the range of applicability of the Inverse Problem.

In particular, I would like to bring to your attention the fact that possible practical utilizations of the Inverse Problem vitally depend on its application to the Optimal Control Theory, which was the topic of my informal presentation. And indeed, both the USAF and NASA have strongly recommended this line of study.

I contemplate to apply to your Division for studies along these lines in the near future. Let me however candidly confess that, unless properly supported, I will simply be unable to conduct the contemplated extension of the Inverse Problem to the Optimal Control Theory.

My applications for federal research support on the Inverse Problem of few years ago were not supported because, according to my best reconstruction, the so-called experts in analytic mechanics had considered the problem to be vacuous. I did the job on a completely unsupported basis resulting on three monographs for some 1500 pages, the third volume of which is a collection of papers appearing in Annals of Physics. This is the result of some five redraftings. The related expenses for typing, xeroxing and mailing to my referees have completely exhausted my personal financial resources. As a result, despite my best intention, unless properly supported, I simply am not in a position to conduct research to any significant dept.

Very Truly Yours

Ruggero Maria Santilli

c.c.: Dr. B. R. Agins

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE. MASSACHUSETTS 02139
Center for Theoretical Physics

March 14, 1977

Dr. B. R. AGINS,
Division of Mathematical Soiences
NSF
Washington, D.C.

Dear Dr. Agins,

I simple have no words to express my appreciation and gratitude for your kind reception during my recent visit and for your several suggestions.

As soon as my plans are finalized, I shall take the liberty of contacting you again.

Sincerely Yours

Ruggero Maria Santilli

Encl.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

March 28, 1977

Dr. Ruggero Maria Santilli
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Dr. Santilli:

In reply to your letter of March 14, 1977, I suggest you send us six copies of the material you wish the reviewers to see. I am afraid this material is needed immediately if the reviewers are to see it.

With regard to revising your proposal, I suggest you do not do so, since any revision at this time would make it impossible for the Foundation to consider possible funding before the fall.

Sincerely yours,

B Kayser

Boris Kayser
Program Director for
Theoretical Physics

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE. MASSACHUSETTS 02139
Center for Theoretical Physics

Dr. B. Kayser, Director
Division of Theoretical Physics
NSF
Washington, D.C.

April 21, 1977

Dear Dr. Kayser,

I enclose for your attention the first four of a series of nine articles entitled "A hadronic model for the nonapplicability of Pauli principle". This series is the result of some twelve years of preparatory studies for a primary objective which I disclose only now. My studies on the Inverse Problem from 1970 until recently were part of this preparatory program. The series of monographs and articles in your possession on this topic where primarily conceived for this new hadronic model. The remaining half of the needed methodology falls in the so-called Lie-admissible Problem which I worked out from 1964 until 1969 in a series of articles.

In essence, I construct a new model on the structure of the hadrons by using the old idea that the strong interactions are not derivable from a potential. This idea, however, is subjected to direct analysis rather than the customary approximati in terms of couplings derivable from a potential. You can now see the vital need of the Inverse Problem as a methodological tool.

The results of this series of articles are of grave physical, methodological and emotional nature. When the strong couplings are taken "ad litteram" as not derivable from a potential, they became so powerful to literally distroy our entire knowledge. Fundamental disciplines such as the special theory of relativity, quantum mechanics and quantum field theory simply became nonapplicable within the hadron, even though the analysis confirms their unequivocal validity for the arena in which they have been experimentally tested until now, electromagnetic interaction In particular, the SU(3) model on the structure of the hadron is invalidated at all levels, from its recent color implementation, to the same central idea of multiplet. In particular, the concept of quark as the elemental constituent of the hadrons becames vacuous because the strong interactions, under said assumption, imply the nonconservation of the charge, spin and magnetic moment of the constituents even though the total characteristics are of course conserved and quantized according to conventional rules. According to the opinion of all collegues I have consulted until now, there is simply no way conceivable at this moment that the SU(3) can be even partially salvaged under the assumption that the strong interactions are not derivable from a potential. I should add that the results of my analysis indicate that the SU(3) models do have a clear physical significance, but only when interpret as describing the chemical external behaviour of the hadrons and as producing their classification of Mendeleyév type . However, the moment the same models are

interpreted as characterizing the actual structure of the hadrons they result to be invalidated at all level. It is in essence the same situation which occured in atomic physics. The Mendeleyév classification has a precise role in the theory. The Bohr model has an equally precise role but profoundly different than the former. The interpretation of the SU(3) models as structure model would be the same as constructing a model on the structure of the hydrogen atom whereby the valence play a dominant role.

I am now deeply involved in completing this series of articles. My central duty is to indicate that the needed generalization of known disciplines to treat forces not derivable from a potential are not only conceivable, but actually possible by using my preparatory methodological studies on the Lie-admissible problem and the inverse problem. After studying this problem for over a decade, I can assure you that the emerging new methodology exhibits a unique beauty, simplicity and physical effectiveness. In much the same way as quantum mechanics was specifically conceived for the atomic structure, this emerging new methodology results to be specifically conceived for the hadronic structure, generalizes the known disciplines according to a physically clear pattern and recover these disciplines under a limit procedure of clear physical significance, the limit of null values of the couplings not derivable from a potential. In the final analysis, this limit appears to characterize the transition from the hadronic to the atomic structure.

Unfortunately, my research program must be truncated by June 1, 1977. The reason is that Boston University, despite the sincere support of my collegues, is not financially capable of extending my contract without a federal research grant. I have applied to all U.S. Physics Departments with a graduate school during this academic year (without disclosing my research program on the hadronic structure) without one single offer until now. My contract at B.U. expires on June 1, 1977. I have a family of four to feed. I must therefore take a full time job in the industry or leave the U.S.A.

As indicated to you and Dr. Isaacson, I have no faith in the current referee system. I discourage you from submitting my enclosed studies to any expert in current hadronic physics for self-evident reasons: my results may invalidate the very motivation for their grants. As also indicate earlier, my entire faith is in your personal vision, professional qualifications and human integrity.

In your judgement you should also take into account that the potential impact of my studies goes considerable beyond hadronic physics. Again I am not in a position to disclose studies prior to their achievement, but I am sure you realize that my studies may have a profound impact on a problem of central social significance: the controlled fusion.

Please reach either a positive or a negative decision on my grant application no later than the last week of May 1977.

Sincerely

Ruggero Maria Santilli
Visiting scientist

c.c.: Dr. R. Isaacson

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

June 21, 1977

Dr. Ruggero Maria Santilli
Visiting Scientist
Center for Theoretical Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Dr. Santilli:

Thank you for your letter of June 1 advising us of your current situation. I hope that the Foundation will be able to advise you of the status of your proposal reasonably soon. I also hope that something satisfactory turns up for you in your career plans.

Sincerely yours,

B Kayser

Boris Kayser
Program Director for
Theoretical Physics

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 30 1977

Dr. Ruggero Maria Santilli
Department of Physics
Boston University
111 Cummington Street
Boston, Massachusetts 02215

Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Necessary and Sufficient Conditions for the Existence of a Lagrangian in Newtonian Mechanics and in Field Theory," PHY77-03963.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

Marcel Bardon
Acting Division Director
for Physics

Copy to:

Dr. Charles W. Smith, Vice President
Financial and Business Affairs

BORIS KAYSER
Program Director
for Theoretical
Physics

July 21, 1977

Dr. Ruggero Maria Santilli
International Center for
Theoretical Physics
Post Office Box 586
34100 Trieste, ITALY

Dear Dr. Santilli:

In response to your request of July 13 to Dr. Marcel Bardon, I enclose the attached verbatim reviews of your proposal. I hope that they will be of use to you in your future plans.

Sincerely yours,

Boris Kayser
Program Director for
Theoretical Physics

Enclosures

FORMAL REFEREE REPORT ON SANTILLI'S MONOGRAPHS "FOUNDATIONS OF THEORETICAL MECHANICS", VOLS I, AND II, SPRINGER-VERLAG, IN PRESS, ACCEPTED AND RELEASED BY NSF OFFICERS.

I have examined the proposal by Dr. Ruggero M. Santilli PHY7703963 (returned under separate cover). My reaction to it is rather negative. I also thought that Santilli was on the borderline between being a third rate scientist and a crack pot and I do not think that the monumental work can change substantially my opinion. The idea of reading it thoroughly produces in me an incoercible revulsion and if you insist on it I am going to resign as a reviewer. The book is written in a pompous, immodest, self-glorifying style which I detest given also the absolute lack of physical content. In view of this criticism I find the total figure asked for the project quite extraordinary.

OVERALL RATING
- ☐ EXCELLENT
- ☐ VERY GOOD
- ☐ GOOD
- ☐ FAIR
- ☒ POOR

NSF Form 173, Jan

Ruggero Maria Santilli

Foundations of Theoretical Mechanics I:

The Inverse Problem in Newtonian Mechanics

Texts and Monographs in Physics

W. Beiglböck
L. H. Loeb
Series Editors



Springer-Verlag
New York Heidelberg Berlin

It is doubtful that this proposal should be given a high priority. The problem that the author has decided to devote so much work on is worthwhile, but I have a strong impression that not much will be accomplished. This impression is based on a careful reading of the proposal and 100 pages of Volume 1 of the voluminous treatise included with it. Here are some specific points that, though not individually of great importance, have led me to think that the author may not be up to the task.

Proposal, p 6. The discussion of O(2) turning into O(1.1). This is probably not so interesting as the author thinks it is — probably just a case of many-to-one mapping. The discussion is naive.

Vol 1, p 33. The definition stinks: "... Lagrangian or the Hamiltonian ...", does he really mean "or"? How can the rest of the definition be applied to the Hamiltonian? Kinetic energy is undefined and so is the concept of a "nontrivial additive interaction term". The footnote introduces additional undefined concepts.

Vol 1, p 46. The third paragraph: Very bad; "arbitrary functional dependence" is meaningless and is not a property.

Vol 1, p 51. That definition of interaction again! What does "nontrivial" mean?

Vol 1, p 57. The discussion on pp. 47 to 56 is very unclear and reaches absurdity in the summary 1.2.3 on p. 56 (spilling over into p. 57). What in the world is the meaning of (3) (top, p. 57). The rest of p. 57 is also unclear.

Vol 1, p 98 Nota Bene. This is an appropriately headlined remark. Here it becomes very clear that the author does not understand the meaning of the problem that he is working on. How can one investigate existence of a Lagrangian when everything is regarded as an approximation? Approximate existence? Certainly it is possible to develop physical ideas without mathematical rigor, but not existence theorems.

OVERALL RATING
- ☐ EXCELLENT
- ☐ VERY GOOD
- ☐ GOOD
- ☐ FAIR
- ☒ POOR

NSF Form 173, Jan 1976

The author proposes essentially a scholarly study on classical mechanics and continium physics; a main objection being the completion of a series of books. Although he should be encouraged if he wants to pursue this kind of work, I would rate the proposal in the category of research work only as "good", or below, for the following reasons. The foundations of mechanics and field theory is a very old subject and much has already been written about exhaustively. Many applied mathematicians and rational mechanists, civil and mechanical engineers, have developed considerable traditions and a new discipline. The author does not mention for example many people around "Arch. Rational Mechanics & Analysis", the Russian and European schools. It does not seem that the specific problem posed, namely, "the necessary and sufficient conditions for the existance of a Lagrangian", is either new, or exciting, or could lead to major advances in knowledge, or a difficult undertaking even if it were not completely solved.

OVERALL RATING
- ☐ EXCELLENT
- ☐ VERY GOOD
- ☑ GOOD
- ☐ FAIR
- ☐ POOR

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

November 28, 1977

Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of September 4, 1977. By now you will have received the verbatim copies of the reviews of your proposal, and you will have seen that they contained strongly negative comments. These reviews resulted in the Foundation's inability to support your proposal. If, considering your reviews, you feel that you would like to appeal the Foundation's decision, you may follow the appeals procedure described in Important Notice #61.

We hope that it will be possible for you to continue with your work even in the absence of NSF help.

Sincerely yours,

Marcel Bardon
Director, Division of Physics

Enclosure
Important Notice #61

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

November 28, 1977

Mr. MARCEL BARDON, Director,
Division of Physics
National Science Foundations
Washington, D.C. 20550

CERTIFIED

Dear Mr. Bardon,

I acknowledge receipt of your letter dated November 28, 1977, received on the same date. I must express a profound dissatisfaction for my many years of totally unrewarding relationship with your division and, in particular, for the following occurrences.

1) My last (of a series of) application No. PHY77-03963 was for the study of the inverse problem in Newtonian Mechanics and field theory. Specifically, the proposal was for the study of : (a) the integrability conditions for the existence of a Lagrangian, or, independently, of a Hamiltonian for the representation of systems of ordynary or partial differential equations with arbitrary couplings, (b) the methods for the computation of these functions from the given equations of motion when their existence is guaranteed by the integrability conditions and (c) the application of the underlying methodology to other aspects of analytic mechanics (such as the transformation theory, symmetry and first integrals of systems with arbitrary couplings) as well as the identification of its significance for applied physics problems, such as nonlinear nonconservative plasma equations, missile trajectory problems and engineering problems (e.g. circuit design inclusive of internal losses) treatable with the optimal control theory. The research was expected to result, as stated in my application of the fall 1976, in three monographs (suggested by the total silence of contemporary theoretical physics on the inverse problem) as well as in a series of papers.

2) On May 1977 I organized a trip to Washington to discuss the intriguing possibiliti of the inverse problem with federal agencies. I was cordially received by several governmental agencies (such as ERDA, USAFOSR, as well as another division of NSF). The case with your division of NSF was different. My phone request from MIT for an appointment to present my latest results in relation with the then pending application PHY77-03963 met with Mr. Boris Kayser's answer: "we do not have time to receive all our applicants".

3) At the specific request by Mr. Kayser, I then did a follow up by letters on my way back from Washingtons according to the letters which should be in your file. This resulted in the official enclosure to my proposal of the three monographs (MIT-CTP publication numbers 606, 607 and 608) which, since the time of my applicatic of one year earlier, were then ready in a preliminary form. Six copies of these monographs (for some 7200 pages all at my personal expenses) were mailed to your office upon formal assurance by Mr. Kayser that they would, in turn, be mailed to the referees selected by NSF for the finalization of their personal opinions on my proposal.

3) Your letter of Nov. 30 communicated to me that my proposal PHY77-03963, as it had been the case for all my preceding proposals beginning from 1972, was unfunded because, as you put it in your recent letter, of "strongly negative comments". The questionable nature of these comments, as well as the NSF responsability in the selection of their authors, is easily established by the following facts: (A) my studies on the inverse problem for partial differential equations had resulted in a series of articles in Annals of Physics which, in turn, resulted in over 500 requests of preprints from all over the world to the Center of Theoretical Physics of the Massachusetts Institute of Technology (most of which in my possession and none of which evaded due to lack of funds). Most impressive was in particular the differentiated nature of the unsolicited intended applications. (B) My three monographs were subsequently accepted for publication by one of the most selective publishers, Springer-Verlag of Heidelberg(WG), in their series "Monographs in Physics", under the title "Foundation of Theoretical Physics". This was the results of enthusiastic referees reports, as officially acknowledged by Springer-Verlag, on the novelty and significance of my studied for theoretical and applied physics by numerous experts in Europe, USSR and USA. (C) The significance of my studies is such to have motivated the preparation of independent previews of the contents of my monographs by other authors which will eventually appear in the specialized press for broad physical audience. The NSF responsability in the selected referees can be best expressed with a comment I was told this summer during my trip of invited lectures in Europe:" the fact that you, with your scientific achievements, are unsupported is a disgrace for the USA".

4) Jointly with the finalization of my studies on the methodology of the inverse problem I also worked on what is, in my opinion, its most significant application: the study of the old idea (e.g., Enrico Fermi) that strong interactions are due to local couplings not derivable from a potential with particular reference to the problem of the hadronic structure. A tentative and highly confidential (at that time) series of papers in their first version was rushed to your division as an informal collateral element of my application PHY77-03963. The most visible implication is the need of subjecting the validity of established relativity and quantum mechanical laws within a hadron to an experimental verification (rather than the tacit acceptance of currently supported research). This disclosure, in my opinion, provides additional indications on the questionable nature of the reports by the NSF selected referees, as indicated by the following facts: (A) after predictable numerous revisions, my studies have been approved for publication by a US publisher as a monograph under the title "Lie-admissible approach to the hadronic structure" . I believe that the referees reports(in my possession) are the clearest illustration of the highly questionable selection of referees by your division. (B) I have written a series of summary papers which will appear in print, again, with additional referees backing totally contrary to your statement of "stronggly negative comments". And, last but not least, (C) I have delivered a series of invited lectures and collected a number of written opinions by leading scientists on the need of conducting my line of studies on the problem of the hadronic structure (jointly with the currently supported trends) to render any different opinion solely motivated by financial interests of established groups of scientific power.

5) My request of July 10, 1977 made directly to you to disclose copies of the negative referee reports had not been honored, contrary to your statement in your letter of November 28 and contrary to the current rules and regulations of NSF. And indeed I simply have not recived these reports at this or at any of my previous addresses.

6) My request of disclosure of the rules to file an appeal has clearly not been honored in time. And indeed, following my written request of July 11, 1977, it took you the months of July, August, September, October and November to answer with your latest letter finally disclosing the "Important Notice No. 61". The point is that, as this notice clearly states, your disclosure occurred after the deadline of 180 days to be counted from your letter of June 30, 1977 of lack of support. According to the opinion of all contacted peaople, this has also been in violation of the NSF rules.

7) My request to reconsider application PHY77-03963 for a reduced amount of $20K to $25K to be granted to me as an individual, has again not been honored and it is ignored by your letter of November 28, 1977.

I am under the impression that you do not realize the fact that all these years of completely unsuccessful research grant applications to your division have resulted in an enormous moral, scientific and academic damage to me up to the the point that I cannot any more take them lightly. In particular, this lack of research support resulted in the impossibility by Boston University of considering me for tenure at the seventh year of my service.

I am also under the impression that you do not realize the extreme unrest in the U.S. community of basic studies toward the current criteria of dispersal of federal funds by your division. To have an idea I suggest you to secure copy of the recently circulated report

R. Hermann, "ELEMENTARY PARTICLE PHYSICS: THE SCIENTIFIC FRAUD OF THE CENTURY"

Finally, I am also under the impression that you do not realize the legal implications of the occurrences 1) through 6). I therefore suggest you to consult an NSF attorney, e.g., on the legal implications whether a fully tenured, fully salaried and fully supported (by NSF) physicist conducts active research in any of the several applications of my methods, while their initiator, despite a fully documented application (inclusive of monographs for 1200 pages) has been unable to receive any support whatsoever.

There is still a residual possibility that I can initiate a scientifically productive association with your division, but, for several reasons which is inappropriate here to disclose, the time for you to reach a positive decision is very small.

Through my several years of applications you have all the necessarily elements of decision and, therefore, I do not consider necessary any further mailing of material, such as the monographs and papers of my studies on the hadronic structure.

In the event that your division is positively inclined toward the support of my studies, I would recommend you to consult additional referees, such as

[redacted]

These scientists are aware of my studies and there is no need for you of mailing material. The selection of additional scientists only motiv ated by a genuine interest in basic studies is left to your capabilities.

I must strongly recommend you not to suggest the resubmission of another proposal because, quite frankly, I would consider it offensive.

The only possibility which I foresee for NSF supporting my research is to honor my request of July 11, 1977, namely, to reconsider my application PHY77-03963 under the reduced budget of $ 20K to $ 25K to be granted to me as an individual.

In case of lack of action on this request, you should not expect any further communication on my behalf.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Honorary Research Fellow Without Stipend

P.S. In the extremely remote possibility that your are truly serious in supporting my proposal (which, in the opinion of many, clearly surpasses by far most of the other proposals you have jointly considered with mind and, unlike mine, funded) you should keed into account that I am not in a position to accept support unless it initiates from December 1977 or, at the very latest, January 1978.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

December 22, 1977

Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

CERTIFIED MAIL

Dear Dr. Santilli:

It is unfortunate that you did not receive copies of the reviewers' comments. They were sent to you on July 21. Another set and a copy of Dr. Kayser's letter of July 21 are enclosed.

In view of the regrettable delays induced by this and other problems, the NSF is taking the position that the deadline discussed in the appeals procedure will be counted from my November 28, 1977 letter, and not from June 30. You therefore have plenty of time to appeal the declination of your proposal if that is what you wish to do.

Your request for reconsideration of application PHY77-03963 for a reduced amount can not be accepted since that application was already declined. You may, if you wish, submit a revised proposal at the lower level, but it would be best also to take into account the reviewers' comments. I am not suggesting the resubmission of another proposal. You have made clear you would find this offensive.

It is indeed regrettable that you have had several years of unrewarding efforts in attempting to obtain NSF funding. We are very limited in what we are able to support. Many worthwhile projects are in the same situation as yours. We and our review process are surely not perfect, and we must constantly be alert to possible errors, but I must conclude that your proposal has been appropriately reviewed and fairly handled. Of course, the appeals procedure is available to you if you find that appropriate.

Sincerely yours,

Marcel Bardon
Director, Division of Physics

Enclosures

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

December 29, 1977

Mr. M. BARDON,
Director, Division of Physics
National Science Foundation
Washington, D.C. 20550

CERTIFIED MAIL

Dear Mr. Bardon,

I acknowledge receipt of your letter of December 22, 1977 and (finally!) of the copies of the verbatim reports by the NSF referees on my proposal.

Upon inspection of these reports I hereby formally request the reconsideration of my proposal PHY77-03963 according to section 5 of the NSF Notice No. 61, January 27, 1976. In accordance with the same section, I expect that "within 30 days following the date of the request, the Assistance Director(or other official designated by the Assistant Director) shall furnish to the PI in writing the results of the reconsideration."

The reasons for requesting this reconsideration are the following.

- The referees have been erroneously selected. In my opinion, their reports clearly indicate that none of them is an expert in the Inverse Problem of the Calculus of Variations (the central topic of my proposal). Therefore, none of them was in a position to objectively evaluate the technical aspects of my proposal, as well as the physical and mathematical relevance of my research.
- NSF should have returned these reports to their authors because of lack of technical qualifications. For instance, the second referee (in the order of your mailing) states "How can one investigate existence of a Lagrangian when everything is regarded as an approximation?" The fact is that the word "approximation" is absent throughout the entire content of my three volumes on the Inverse Problem (MIT-CTP Nos. 606, 607 and 608). The analysis is devoted to the integrability conditions for the existence of a Lagrangian (or, independently, of a Hamiltonian) within the context of the calculus of differential forms and the converse of the Poincaré lemma in particular. The mere mention of the word "approximation" in relation to these techniques indicates the complete lack of technical qualifications or the pursuit of nonscientific objectives through a referee process. The technical content of the first report is simply entirely absent. The third report merely expresses some vague personal feelings which are completely unsubstatiated. For instance, after having spent some three years of laborious library search, having consulted virtually all experts on the Inverse Problem in Europe, USSR and (the few) in USA, and after having visited and lectured at several of the best institutions in analytic mechanics, this third referee has the courage to state "the author does not mention for example many people around "Arch. "ational Mechanics & Analysis", the Russian and European schools."

- You and Mr. Kayser should have rejected these reports on etical grounds. An incontrovertible aspect of these reports is their language. Such a language is justifyable, say, for a frustrated mine worker. For a referee procedure involving the delicate financial issue of the allocation of tax payers money for research programs, languages of the type of the reports you have accepted and mailed to me can only have a dubious interpretation. It is common practice among reputable journals to return to their authors either nontechnical reviews of technical material or reviews containing questionable language. For a referee procedure involving the indicated delicate financial issues, the rejection of reviews of the type you have mailed to me should be simply mandatory.

To give you an idea of the difference between the reports of the referees selected and accepted by NSF and independent scientists, I enclosed a number of reviews on my studies. Additional reviews, perhaps more enthusiastic, have not been include

The material which I want to be included in the reconsideration is the following.

(A) My proposal PHY77-03963 as is. It is by now largely obsolete because most of the indicated research objectives have already been achieved. Neverthless, it is my opinion that the proposal, as is, is sufficient for a review process which is qualified on both technical as well as ethical grounds. The objection by one referee that its language is naive simply remainds me of the objection by a physicist on Yang-Mills paper soon after its appearance that the presentation was naive.

(B) My three monographs on the Inverse Problem in Newtonian Mechanics and Field Theory (MIT-CTP publ. nos. 606,607 and 608), because they were officially attached to my proposal PHY77-03963. These monographs are totally obsolete at this time. In essence, they were a draft rushed to your division to provide more evaluational material. I do not intent to release the new versions which have been accepted for publication by one of the most selective publishers, Springer-Verlag of Heidelberg, WG. The reason is that I have found simply preposterous the pretention by one of your referees that these manuscripts should be perfect. If these manuscripts had reached full maturity, I simply do not see the reasons why to apply for a research grant. I am here formally asking that the subsequent highly tentative series of papers on the application of my studies for the construction of a new model of the hadronic structure (which I mailed to your division in April 1977) should not be included in the reconsideration because they were intended to be a confidential disclosure. In any case, after many redraftings, implementations and expansions, these papers have resulted in a series of preprints of the Lyman Laboratory of Physics of Harvard University and in a series of monographs which have been accepted for publication by Hadronic Press, Inc., as you can see from the enclosed reviews. I am, however, formally asking that the reconsideration

must take into account the central reason why I entered into such a laborious research program on the Inverse Problem: these new techniques are centered on the study of systems with couplings not derivable from a potential; as such, they are significant for the study of the old idea that the strong hadronic forces are precisely of this type. It is my opinion that this remark alone is sufficient to complement the material of my MIT-CTP monographs and of my proposal. I do not see the necessity of mailing to you my Harvard preprints and my monographs on the study of this physical application. In any case, my totally unsupported studies have by now resulted in over 5,000 research pages. I simply do not see how your division can effectively and objectively review all this material in 30 days.

(C)This letter and its enclosure of independent reviewers.

If I can be of any assistance in the reconsideration, please do not hesitate to contact me. I am formally asking that any additional review by NSF referees be promptly mailed to me.

In closing, I must stress my complete disagreement with your statement, in the letter of December 22, 1977, that

"Your proposal has been appropriately reviewed and fairly handled".

You should be also informed of my intent of disclosing our correspondence, at any time I consider appropriate, to a number of observers currently monitoring the operations of the Theoretical Physics Division of the U.S. National Science Foundations.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli

encl.: Verbatim review by independent scientists.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

January 9, 1978

Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

I have your letter of December 29, 1977, in which you request reconsideration of your proposal, PHY77-03963. You will note from NSF Important Notice #61 that such a request must be addressed to the appropriate Assistant Director. Accordingly I have forwarded your letter to Dr. James Krumhansl for his consideration.

Sincerely yours,

Marcel Bardon

Marcel Bardon
Director, Division of Physics

MEMORANDUM

DATE: January 9, 1978

To : Dr. J. Krumhansl, Assistant Director, MPE

Via : Dr. Ronald E. Kagarise, Deputy Assistant Director, MPE

From : Director, Division of Physics

Subject: Correspondence from Ruggero Maria Santilli

I have received the attached letter from R. Santilli, requesting reconsideration of his proposal to the National Science Foundation, which was declined by the Physics Division. I am forwarding it to you for appropriate action. Also attached is the file for his proposal, PHY77-03963.

Marcel Bardon

Attachments

Copy to:
Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

January 31, 1978

Dr. Wayne GRUNER,
National Science Foundation
Room 305
1800 G Street N.W.
Washington, D.C. 20550

Dear Dr. Gruner,

I would like to express my appreciation for the courtesy of your phone call of this afternoon and take the liberty of providing you with some additional information in relation to the reconsideration of my grant application PHY77-03963.

As you know from my application of reconsideration mailed to the National Science Foundation on December 29, 1977, the material which I have asked to be reconsidered is the application itself, plus the preliminary versions of my three monographs on the Inverse Problem, MIT-CTP publication numbers 606, 607 and 608. I have also asked that my letter of December 29 and its enclosures be considered as part of the proceedings.

I am fully aware of the difficulties of your job, essentially due to the limited period of time allowed by NSF Important Notice No. 61 of Dec. 27, 1976 for the reconsideration proceedings as well as the fact that true experts on the methodology of the Inverse Problem are extremely few and known also to researchers in the fields. The following suggestions are provided on grounds of my desire to assist you, but they are left to your discretion. More specifically, I am not expecting nor requiring that you should take into account the following content of this letter.

Current status of the material under reconsideration. As stressed in my letter of application for reconsideration, the complete material is by now obsolete. In particular, the central part of the application, the monographs on the Inverse Problem, has been subjected to a profound revision which resulted from: (a) a number of invited talks in U.S. and European Institutions (the list is at your disposal), (b) critical comments by several collegues in USA, Europe and USSR and (c) an informal seminar course which I have delivered here at Harvard during the past term to a group of graduate students and researchers of the Boston Area (see the enclosed outline of the course). Neverthless, it is my opinion that the material of the grant application PHY77-03963 should be considered as available at the time of the decision. As a result, I do not intend to disclose improved versions of the material. To be specific, I consider absurd the idea that, for a grant application, the research topic should be already worked out to utmost maturity.

Applications of the methodology of the Inverse Problem. The methodology which is the subject of my application PHY77-03963 deals with the integrability conditions for arbitrary systems of ordinary and partial differential equations to admit an analytic representation in terms of conventional Lagrange's and Hamilton's equations. Owing to the elemental nature of these equations in physics, engineering and mathematics, the methodology under consideration is expected to have a number of diversified and significant applications to both, conservative and nonconservative systems, such as, nonlinear nonconservative plasma equations, electric circuits inclusive of internal losses, trajectory problems of missile motion in atmosphere, ecc. In essence, the knowledge of a Lagrangian or a Hamiltonian renders applicable established, rigorous analytic techniques for systems which are today ofteh treated with semiempirical approximation techniques. Also, the methods of the Inverse Problem appear to be computerizable with selfevident possible significance for, say, the in board solution of trajectory problems or optimal flight paths. Informal conversations with NASA and USAFOSR officials have confirmed the possibility of both civilian and military applications.

These possibilities are, of course, indicated in my monographs. However, they are diluted in some 1500 research pages. As a result, I considered advisable to prepare for NSF a very brief outline of these possibilities. A copy of this document, mailed to NSF sometime in March 1977 (if my recollection is correct) is enclosed for your consideration.

Again, this document is by now obsolete. The reason is that a number of applied physicists, engineerings and applied mathematicians are apparently working on some of the indicated aspects and I could therefore provide much more specific data. I do not intend to disclose the names of these collegues. The reason is that some of them are apparently with NSF support, that is, they are working with NSF grants on the methodology which I have laboriously identified and for which NSF has refused support year after year, year after year.

The indicated applications of the methodology of the Inverse Problem are those which I consider of transparent nature. In addition they are in areas outside my current research interest. In essence, my intervention is that of assistance to collegues, when needed, in the proper use of the methodology under consideration.

The application of the methodology in which I am currently interested is of nontrasparent nature. It concerns what I consider the truly fundamental problem of contemporary experimental and theoretical high energy physics: the validity or invalidity for the hadronic constituents of those relativity and quantum mechanical laws which have proved to be so effective for the atomic (as well as nuclear) costituents.

I enclose an outline of three forthcoming monographs I am currently finilizing on this topic (after many years of laborious and solitary studies) which will be published by Hadronic Press. The manuscripts are available (as well as my Harvard papers summarizing their content). Nevertheless, I do not intend to disclose them for the proceedings of reconsideration of my grant. The reason is that this part of my research proposal was only indicated, but not formally

accluded to the proposal.

In line with my application of reconsideration, I am simply asking that in your proceedings you also take into account the fact that the methods of the Inverse Problems are conceived for systems with forces not derivable from a potential, and that this is precisely the old idea that strong interactions are of this type. In different terms, the methods appear to be of some significance for the vexing problem of the nature of the strong interactions which, according to a mounting evidence, do not appear to be treatable with the semplicistic idea of a potential function (that is the same as the electromagnetic interactions).

Qualified referees. As indicated earlier, I am fully aware of the difficulties of identifying "experts" in a discipline which has remained virtually ignored in both specialized mathematical and physical literature for over half a century.

At your discretion, please feel free to directly contact the following persons.

[redacted]

As you know, my monographs on the Inverse Problem have been accepted for publication by Springer-Verlag (the contract was signed by both parties through the respective attorneys in December 1977). It is significative that the acceptance was based on th obsolete copies in your possession. Of course, the improved copies are also in [redacted] possession. Predictably, Springer-Verlag has consulted a rather significant number of professional referees solely interested in the pursuit of knowledge, rather than entangled in the financial machinery of NSF grant allocation. I am confident that, if asked, Springer-Verlag will release the file of their reports or a summary of them. Please feel free to contact either [redacted] in Heidelberg or [redacted] in New York.

[redacted]

The Instituut voor Theoretische Mechanica is one of the oldest and most prestigiuous institutes entirely devoted to Theoretical Mechanics. I had the honor of receiving invaluable assistance from several of its member over an extended period of time. In particular, this summer I had the opportunity of presenting an invited talk, with several days of detailed discussions with various experts in some of the aspects of the Inverse Problem. The Director of the Institute, Professor [redacted] is fully aware of my laborious search for maturity and I

am confident that, if asked, he will provide you with his independent assessement. For a formal stetement on my study by Professor Mertens released for the press, see the enclosed brochure by the Hadronic Press. Notice that, to my knowledge, Springer-Verlag has not contacted Professor Mertens and his associates. As a result, this is an additional independent source of evaluation. Also, one of Professor [redacted] associates, [redacted] will likely spend next year at Harvard with me to work on certain methodological aspects of the program (see the enclosed copy of formal application).

[redacted]

I have been associated to Professors [redacted] and [redacted] for several years when I was at [redacted] and our scientific contacts have persisted after the termination of my appointment with this university. I am sure that they will cooperate with you in any form you desire. Notice that they have already released a formal statement on my studies for press distribution. See also the enclosed brochure by the Hadronic Press. Professor [redacted] is currently supported by NSF.

[redacted]

The [redacted] has also been interested in the publication of my monographs. I decided to publish them with Springer-Verlag for a number of reasons of nontechnical nature. It is appropriate here to acknowledge that the assistance I have received by The [redacted] for the finalization of the manuscripts has been invaluable(this was during my stay at MIT in 1976-1977). The reason is quite simple: the [redacted] again, had selected highly qualified professional referees for my earlier versions. They did a detailed technical review of the analysis and they provided a number of criticisms on several technical aspects which have simply been invaluable for my efforts. I am sure that [redacted] will provide you with his referee file or with a summary of the reports. If you contact [redacted] please inform him of the existence of revised versions of my manuscripts (which I did not release to The [redacted] and indicate the expression of appreciation I have for the role of his referees in my achieving these improved versions.

[redacted]

The Hadronic Press is the publisher of my additional monographs on the hadronic structure. Prior to committing a rather sizable portion of the company's resources

in my research monographs, the company, of course, entered into a laborious referee process. The speculative nature of my studies called for a particular effort which resulted in the submission of the manuscripts to professional, genuine scientists in more than one continent. The results of this review have been beyond my best expectation. A formal statement released by the president of the [redacted] in enclosed in the separate summary of statement. I prohibited the printing of this statement in the formal brochure for press and promotional distribution because excessively positive. In any case, I am sure that the president of Hadronic Press, Inc. will be fully cooperative with you for the disclosure of the referee reports or for a summary.

You should be aware that the above persons constitute only a minor part of possible sources of qualified informations on my research. Several additional sources are at your disposal, with the exclusion of physicists currently working with NSF support on the applications of those methods which have seen NSF refusal of support year after year, year after year, for their originator. You should be also aware that I have in my possession copies of all the verbatim referee reports originating from the indicated sources. Finally, I should confirm what I verbally indicated to you: by no mean I claim that I have achieved maturity in my studies. I am simply working on my laborious search for the best I can personally do. The achieving of maturity on the methodology of the Inverse Problem will likely take more than one generation, owing to implications in several disciplines, such as Nonconservative Mechanics, Nonlinear Mechanics, Optimal Control Theory, Differential Geometry, Functional Analysis, Field Theory, Continuum Mechanics, not to mention quantum mechanical and quantum field theoretical aspects.

Legal implications of NSF refereeing. I intend, of course, to avoid the expression of my personal opinion of the verbatim referee reports I have received on my grant application from NSF officials. The reason is that the best place to achieve a valuable judgment of these referees, their reports and their responsabilities, as well as the NSF responsability in their selection and in their acceptance, is in court. You should be aware that, according to my attorneys, these reports are such to warrant a law suit on a number of independent counts and to more than one individual. The sole reason way this law suit has not been filed until now is to pay an undisclosed and tacit form of appreciation for the hospitality I am currently receiving from Harvard University. To be specific, I have not filed a law suit while being a guest at Harvard University because it is contrary to my ethical code. But, my visit here at Harvard will soon be completed and then I will be free to act according to what is, in my opinion, the best interest of this Country, as well as mine. I only hope that, in the meantime, the responsible authorities will give evidence of implementing rules for the dispersal of tax payer's money for research grants in theoretical physics in a form which is genuinely effective and, thus, truly in the interest of the Country, rather than isolated groups of scientific power. Quite frankly, I do not believe that this Country will prosper (or even survive as is) on a long range basis without the seeds of a well balanced basic research.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Honorary Research Fellow Without Stipend

c.c.: [redacted]

Encl.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 7, 1978

Dr. W.GRUNER,
Special Assistant to the Director
Division of Applied Mathematics
1800 G Street
National Science Foundation
Washington, D.C. 20550

Dear Dr. Gruner,

I am contacting you to ask for the courtesy of a suspension of the reconsideration of my research grant application to NSF of 1976.

Following my application for reconsideration, I have submitted a research grant proposal to the Department of Energy with Professor Shlomo Sternberg as Principal Investigator.

Lately, we have received communication that the Division of high energy physics of the Department of Energy has approved the proposal and recommended it to the DE administration for funding. It is my understanding that a research contract is in the process of being executed between Harvard University and DE.

It is my decision to formally ask for the waiving of the process of reconsideration as soon as such a contract is executed and I hope to be able to contact you soon in this respect.

In the meantime, I would like to express the sentiments of my sincere appreciation for the genuine interest you have indicated for my case.

Very Truly Yours

Ruggero Maria Santilli
Honorary Research Fellow

c.c.: Professor S. Sternberg

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

nsf

April 10, 1978

OFFICE OF THE
ASSISTANT DIRECTOR
FOR MATHEMATICAL AND
PHYSICAL SCIENCES,
AND ENGINEERING

Dr. Ruggero M. Santilli
Harvard University
Lyman Laboratory of Physics
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

This is in response to your letters of March 6 and March 7. Let me thank you first of all for your courtesy in notifying us promptly of these developments.

Second, let me express my pleasure and the pleasure of the Foundation upon learning that you have a good prospect of receiving support from the Department of Energy.

Finally, let me note that, according to your request, we intend to take no further action concerning your proposal No. PHY77-03963 unless requested in writing by you to do so. Once again let me thank you for your courtesy in notifying us of the state of your negotiations with the Department of Energy.

Very sincerely,

Wayne R. Gruner

Wayne R. Gruner
Special Assistant to the
Assistant Director

cc:
Dr. J.A. Krumhansl AD/MPE
Dr. R.E. Kagarise DAD/MPE
Dr. M. Bardon DD/Physics
Dr. Boris Kayser PD/Physics

Ruggero Maria Santilli
367 Linwood Avenue
Newtonville, Ma. 02138

July 20, 1978

Dr. JAMES A. KRUMHANSL, Assistant Director for
the Mathematical and Physical sciences and Engineering
National Science Foundations
WASHINGTON, D.C.

Dear Dr. Krumhansl,

I would like to express my congratulations for your new post and my support for the active campaign you have initiated. I also would like to take the liberty of presenting my view on the current situation of basic, theoretical research in high energy physics. Permit me the use of a candid, nonacademic language. The situation is so grave, that the identification of the current problems in a way as clear as possible can only be beneficial. I am confident in your mature and receptive attitude.

You will probably recall me because I have been at the very edge of filing law suits against Mr. BARDON and Mr. KAYSER, both as individuals and as NSF officers, on a number of counts. One of these counts was the fact that these officers had accepted and released a referee report of clearly offensive language on my technical manuscripts which had been accepted for publication by one of the most prestigious publishers (Springer-Verlag) in one of the most prestigious series of research monographs in physics. For your convenience, I enclose copy of the front page of my first volume and of one of the NSF referee reports.

These laws suits were not filed to pay an undisclosed and tacit form of respect for the hospitality that Harvard University was providing me. As I put it in my correspondence with Mr. CARTER, the filing of these laws suits while being a guest at Harvard was contrary to my ethical code.

Subsequently, I became recipient of a research grant from the Department of Energy. I therefore instructed my Boston based and Washington based attorneys to delay indefinitely the filing of these laws suits. I also took all the necessary precautions to prohibit these attorneys from releasing the material they had collected.

I would like to stress that I do not have and never had any animosity against the indicated NSF officers. My contemplated, and intended to be, highly publicized laws suits were solely intended to draw national attention on the grave situation (in my perhaps erronous view) of funding, promotion and support at NSF for creativity in basic research. I am never tired of repeating that this is a technologically oriented Country with a cloudy future. Such a long range future vitally depends on the capabilities of the Governmental Agencies of implementing now the seeds of a well balanced community of basic studies, in the genuine pursuit of knowledge.

I have dedicated my life to basic research. Unlike other collegues, I have always put the pursuit of academic power subordinate to that of the pursuit of knowledge. I feel no shame in disclosing to you, as an indication of my determination, that I have put this attitude in practice to the point of being unemployed for a considerable period of time with a family of four and my wife at the graduate school.

Permit me to confess that I had lost all hopes that an improvement of the operations at the division of basic research of NSF could be achieved without grave gestures, such as laws suits, Senate Hearings, etc. This was simply due to the fact that my gentle initial attempts had met with the costomary academic tool for unwanted lines: complete ignorance.

This letter is motivated by my hope that, perhaps, the objective considered can be achieved in an orderly fashion, without the grave gestures indicated. The remarks below are presented in this spirit.

I enclose a courtesy copy of a letter I wrote to Professors PANOFSKY (SLAC), WILSON (FERMILAB) and VENEYARD (BROOKHAVEN). As you can see, it is a passionate appeal that we simply cannot continue on the current basis of complete monopolistic control of basic research by the quark conjectu It is time to implement a well balanced condition and conduction of research in which efforts along the quark models are indeed continued. But jointly we implement fundamentally different approaches to hadron structure for a comparative confrontation of physical reality.

My first appeal to you is that NSF initiates the support, even in a minimal fraction of available funds, of studies on the hadron structure which are strictly quark-non-oriented. I believe that it is virtually impossible to achieve the much needed well balanced conduction of research on the fundamental proble of contemporary physics without a well balanced policy of research grants by Governmental Agencies.

To be quite frank, I believe that an increase of funds available to NSF will be entirely ineffective, unle such a revision of policy is implemented. Indeed, if these additional funds are dispersed according to current criteria, they will results in noting more than a further proliferation of minute incremental contributions which only in the most optimistic circumstances can hope for a future status of scientific curiosity for curious historians.

My second appeal to you is that a mere formulation of policy to achieve a well balanced conduction of research in hadron physics will be entirely ineffective, unless a profound revision of the current operations of the NSF division of basic studies is implemented.

This is the true problem. NSF operates in a complete symbiosis with academicians currently in contr of the scientific power. All these fellows are academically and financially committed and dependent on the quark conjecture, as you can easily identify in all grants issued by NSF during the last decade specifically devoted to the study of hadron structure. I simply have no faith whatsoever that one of these high standing academicians financially committed to quarks will release a positive referee report on a proposal which is strictly quark-non-oriented.

You should not be surprised at this statement, nor you should read in it my intention of accusing my fellow researchers of scientific corruption. As a matter of fact, I believe that they are convinced of being in the right track. They are simply not consciuous that their action is, in my view, strictly antiscientific. Hadron physics is not a science, that is, the manifestation of an experimentally established truth. Instead, it is the mere expression of mere opinions by groups of researchers, such as the opinion that the quarks are the physical constituents of hadrons, complemented by the opinion that they confine, etc. etc. In filing their negative reports on a quark-non-oriented proposals, these referee simply express another mere opinion that it is not the right way to go.

I have recalled my case, and the fact that it brought me so close to filing quite delicate laws suits, because we can learn by analysing it. I have been told that the referees of my proposal were "truly outstanding physicists", that is, in my candid language, physicists currently in control of the scientific power with a vital dependence on the quark conjecture for the preservation of such power. The net result is that their high academic standing did not prevent them from filing not only a negative report but one in the language which could be only justified for a frustrate mine worker. The truth of the matter is that my proposal was inspired as strictly against quark conjectures. This inspiration created such negative reaction to render these "truly outstanding physicists" blind on the physical relevance of the methods I had laboriously worked out in years of solitary and completely unsupported work, as presented in three research monographs on the so-called Inverse Problem of mechanics (MIT-CTP publications 606, 607 and 608) formally reviewed. For your informations, these methods are now applied by numerous physicists and mathematicians in differentiated problems such as circuit design inclusive of internal losses via the optimal control theory, nonlinear nonconservative plasma equations missile trajectory problems and high energy physics.

The fact remains that, in my view, NSF had funded during the last decade on hadron structure only personal opinions on an ever increasing plurality of different unknown quarks. We simply cannot continue indefinitely along these funding lines.

My specific proposals for an improvements of the operations of the basic research division of NSF and, consequently, of the dispersal of available funds are the following.

I - FORMULATION OF THE ETHICAL CODE FOR NSF REFEREES. In January 1978 I received the pleasant duty to organize a new journal in hadron physics (the HADRONIC JOURNAL). This journal is now acquiring momentum and I am very pleased of its initial results (see the enclosed letters to Professors Panofsky, Wilson and Vineyard). My very first gesture after acquiring the post of Editor in Chief was the setting of the ethical code for referees which I enclose with each request of review. It essentially states that, even in case the submitted paper is completely nonsentical, offensive language in the report must be categorically avoided. It then enters into the request that the referee conducts a selfcritical examination of the physical laws and knowledge he uses in reaching his conclusions, to the effect of ascertaining whether they are experimentally established, or merely believed to be true.

This ethical code was conceived and implemented for an editorial process without any financial aspect. I am sure you will agree with me that it becomes mandatory when delicate money aspects are involved, such as the NSF funding or lack of funding of proposals.

I have no words to stress the need that you implement such ethical code in the form you consider most appropriate. It is simply imperative, in the NSF interest, that all necessary precautions are taken to the effect that referees reports such as those I have received be categorically excluded by NSF operations. The risk for the lack of implementation of this request is clear: laws suits and Senate Hearings for alleged scientific corruption.

II - FORMULATION OF THE NSF POLICY FOR REFEREING PROPOSALS. The HADRONIC JOURNAL is dedicated to plausible studies on fundamental issues (minute incremental paper are rerouted to other journals). In particular, my primary objective is to avoid a monopolistic presentation of research. I therefore dedicate exactly the same attention to either quark-oriented or quark-non-oriented papers. The second step I implemented after acquiring the post of Editor in Chief is that the submission of any paper to referees of ony one belief be categorically prohibited. Specifically, I considered vital for objectivity that each paper, whether quark-oriented or quark-non-oriented, be submitted to referees who are quark-believers as well as most importantly quark-non- believers.

This refereing policy was implemented, again, only for editorial purposes. I am sure you will agree with me that it becomes mandatory when delicate money aspects are involved, such as in NSF funding of research. To be specific in this truly vital aspect, if a quark-non-oriented proposal is submitted only to physicists financially committed to quarks via existing grants, this is literally equivalent, in my view, to the decision not to fund the proposal at its very reception.

I must stress that the academic status of the referee is of purely secondary consideration (unless you believe that the outstanding referees of my proposal did act properly). What must be of utmost priority in the consideration is whether the referee is or is not recipient of research grants on quarks and whether he is a believer or non believer in quarks. I can assure you that there exist physicists of proved ethical substance who are quark-non-believers. Some of them are indeed outstanding and they have simply absteined from active publications in hadron structure to avoid their association with games of scientific curiosity such as truth, beauty, up and down etc. (in the current quark language funded by NSF).

I have no word to stress to you the need to identify and strictly implement a new policy for the proper selection of the referees of each individual proposal. It is vital that each proposal, irrespective of whether quark-oriented or quark-nonoriented, is submitted to a group of referees which satisfy precise, uncompromisable criteria of differentiation in their personal beliefs and committments. It is vital that NSF gives proof of truly effective refereing proposals which are strictly quark-non-oriented and which are nowadays, by and large, considered outside the "scientific establishment" and thus of no scientific value by physicists academically and financially committed to the quark conjecture. It is vital that NSF begins the submission of proposals by quark believers to quark-non-believers (I would be happy to serve NSF in this latter function, and so are other more qualified collegues, but we have all been excluded by the NSF referee process until now). The risk for the lack of

implementation of this request is clear: the monopolistic continuation of funding for studies on the fundamental problem of contemporary physics along only the quark conjecture. With full candor, unless this situation is avoided, a crisis in basic research of unpredictable proportions will be unavoidable.

III - FORMULATION OF THE NSF PRIORITIES FOR FUNDING. The enclosed letter to Professors Panofsky, Wilson and Vineyard, as you can see, is a passionate appeal to reestablish the traditional priorities of basic research which have contributed so much to human knowledge up to the first part of this century and lately abandoned. What NSF has funded in hadron physics during the last decade is essentially a sea of minute incremental contributions deprived of any contribution in basic research which is even partially comparable to the great achievements of the first part of this century.

I have a scientific duty to inform you that a series of papers in the HADRONIC JOURNAL present clear criticisms of the fundamental physical laws used for strong interactions and ask for their direct experimental verification.

I have no words to stress to you the need to keep this new orizon in due consideration. In my view, it literally creates a new situation for funding research proposals and the need to establish precise priority for fund allocations. I am sure you realize that NSF simply cannot continue to fund on minute incremental contributions in hadron physics along established trends, when the basic physical laws used in these studies are in question.

My recommentation to you is that I have made to Professors Panofsky, Wilson and Vineyard. I suggest that utmost priority be given for funding proposals in the study of truly fundamental physical problems beginning and most importantly from the fundamental physical problem whether the experimentally established knowledge for the electromagnetic interactions is applicable or inapplicable to the strong interactions in their currently known form. The funding of studies of minute secondary aspects should receive a minute secondary priority.

Whatever the priorities you select, it is essential that they are fully disclosed and advertised at the time you consider it appropriates. I am sure you will agree with me that their criptic containment within NSF files would be uneffective. Instead, to reach the necessary effectiveness, I consider essential that our entire community of basic research is fully and adequately informed of the selected priorities.

You have a possibility of giving an invaluable contribution to basic research. But, it demands clear ideas, firm implementation agaist predictable opposition, and courage. The identification, release and full disclosure of clear priorities for fundings in basic research appears to be essential for a genuine contribution to the pursuit of human knowledge.

IV - INCREASE OF NSF FUNDING TO RESEARCHERS AS INDIVIDUALS RATHER THAN TO THEIR INSTITUTIONS.

I believe that a number of potentially crucial applications on truly fundamental physical problems will never reach you as a formal grant application via Institutions. The understanding of this occurrence again, demands an open language. Truly new ideas generally see their inception during graduate studies. When the researcher then reaches sufficient maturity for their treatment, he is at the level of research associate or assistant professor. The filing of any proposal at this step of the academic layer via Institutions generally demands approval by the senior collegues in the department. I know of a number of cases in which truly promising proposals were killed at the departmental level and they never reached a Governmental Agency. I can also personally testify that the filing of my own research proposals during my past academic life via Institutions demanded the overcome of such academic entanglements to go beyond the wildest imagination.

If NSF is genuinely interested in a comprehensive program for the support, promotion and assistance of creativity in basic research, I believe that it is vital to increase the number of grants to individual

and correspondingly decrease the grants to Institutions. Again, it is vital that NSF fully discloses the statutory possibility that researchers can apply as individuals, rather than via Institutions, in such a way that each researcher has full information to reach the best decision under his own circumstances. As it is now, this is a criptic information which, even myself, I finally knew by word of mouth and after numerous years of research activity.

You must realize that this action is essential on a number of counts. The first is that a researcher with valuable ideas on fundamental problems should be free to pursuit them, and not be subject to the predictable opposition of his senior collegues with opposing vested interests. But there are other reasons in our changing community of basic studies which support the increase of grants to individuals (now virtually nonexistent at NSF, and only written in a nonimplemented statute). As you know, theoretical physicists nowadays, at the peak of their maturity and productivity (the level of associate, nontenured professor) find themselves unemployed because of lack of tenure. Other truly valuable young researchers either abandon the field, or are forced to do minute calculations as research associate to senior physicists with NSF (or other) support. A duly advertized information that grants to individuals are permitted by NSF statute and actually granted would be simply invaluable to this most productive and energetic segment of our community of basic studies.

But there is still another reason which strongly suggest the increase of grants to individuals. It is of mere arithmetic nature. Owing to the now prohibitive overheads, one grant to one institution can litterally supports twice as many researchers as individuals. In essence, a theoretical physicist studying fundamental physical problems does not need huge amount of money. He simply needs money for food and shelter for his family and himself. If the funds available to NSF for basic research were properly distributed between grants to individuals and grants to Institutions in a proper proportion (at least 50 o/o in my view, money wise), this would immediately imply a substantial increase of supported research without any increase of funds whatsoever.

Of course, Institutions will strongly oppose such an approach. But, is the duty of NSF to support the financial condition of U.S. campuses, or to support the pursuit of human knowledge?

V - GOOD LUCK TO YOU IN YOUR DIFFICULT DUTIES.

If I can be of any assistance to you, please do not hesitate to contact me. You can trust in my utmost confidentiality.

I occasionally visit the Washington area. If you are interested in my paying you a visit to discuss in more details these issues*, please let me know.

Very Truly Yours

Ruggero Maria Santilli

RMS|cgg

P.S. In case academic entanglements will prohibit my continuation of research under DOE support (which, by statute, cannot give grants to individuals), I intend to submit a research proposal to NSF which is (a) of strictly non-quark orientation, (b) on fundamental problems (the physical laws for strong interactions) and, most importantly, (c) submitted as an individual.

* There are aspects and recent events which go beyond a letter, even written in candid language.

c.c: Mr. M. BARDON, NSF .

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

August 17, 1978

OFFICE OF THE
ASSISTANT DIRECTOR
FOR MATHEMATICAL AND
PHYSICAL SCIENCES,
AND ENGINEERING

Dr. Ruggero Maria Santilli
367 Linwood Avenue
Newtonville, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of July 20, 1978, commenting on NSF's procedures for handling proposals and the general trend of the research we have supported in physics in the past years. I have given this matter my personal attention and have also discussed it with a number of members of my staff.

First let me respond to your comments in my role as a Federal administrator. In this capacity I feel that it is part of my responsibility to be sure that proposals received and grants made by the Foundation are handled strictly in accordance with the Foundation's policy which I believe represents a highly ethical manner of proceeding. I am, however, sympathetic to your being perturbed by intemperate comments in reviews. I should remind you in this context that comments made by referees are confidential and can be shared with the principal investigator only, following on a specific request from him. In brief, I believe that NSF's handling of proposals is carried out in an exemplary manner, and in this, I can speak from personal detailed knowledge of various proposals including your most recent one to us. My former Assistant, Mr. Wayne R. Gruner, has briefed me fully on the details of your recent dealings with the NSF.

Secondly, as a physicist I feel that while I have some passing acquaintance with the topics you mention, any comments that I might make would be somewhat uninformed in view of the limitations of my familiarity with hadron physics. Therefore, I feel it would be professionally irresponsible of me to enter into this specialized area. To guide the Foundation in this and other areas that are highly specialized, the Foundation has an Advisory Committee for each Division. Recently the

NSF Advisory Committee for Physics performed an in-depth review of NSF support of Theoretical Physics, and a copy of their report is enclosed. I believe the members of this Committee would be interested in your recent letter to me including its enclosures, and I would like to pass them on for their information. However, as one enclosure is marked "Confidential Copy", I would like your explicit permission before doing this.

Finally, I want to thank you for your letter. I know you feel strongly about these matters, and I can only admire your tenacity and dedication to your views.

Sincerely yours,

J. A. Krumhansl
Assistant Director

Enclosure

Ruggero Maria Santilli
367 Linwood Ave
Newtonville, Ma 02160
August 24, 1978

Dr. J. A. KRUMHANSL
Assistant Director for Mathematical and Physical Sciences
National Science Foundation
WASHINGTON, D.C. 20550

Dear Dr. Krumhansl,

I would like to express my appreciation for your letter of August 17, 1978. Following your request, I am glad to give you full authorization for sending a copy of my letter to you of July 20, 1978 and of all its enclosures to the members of the NSF Advisory Committee for Physics. However, I woul be grateful whether you contact Professor W.PANOFSKY in respect to the release of a copy of my letter to him of July 19, 1978 prior to any decision. Indeed, this letter was intended to be restricte to the persons indicated and, in any case, Professor Panofsky should be consulted in this respect. I would also appreciate whether you include a copy of this letter elaborating certain aspects and the circumstances which lead to my letter to Professor Panofsky. Also, my letter to you of July 20 was intended for your personal amusement and not conceived to be reviewed by an NSF Advisor committee. Neverthless, I have no objection for its release, provided that the matter remains confidential and does not become available to persons outside NSF.

The following points might have some complementary value for my letter of July 20, 1978 to you and for its enclosures.

(1) At the risk of being considered a visionary, permit me to restress that the ultimate reason behind my letters to you and to Professor Panofsky is my belief that the Division of Theoretical Physics of NSF is in a highly delicate moment. I am sure you are aware of the malcontent of one segment of our community of basic studies in regards to quark oriented studies and the amount of funds they receive. This malcontent is increased through the years, rather than decreased, becat the problematic aspects of the quark models have increased, rather than decreased, through the ye I do not know whether you are aware of the fact that lately this malconten t has reached alarming proportions. I must stress that this is my personal view, based on all the informations available to me and, as such, it could be enti rely erroneous. Neverthless, I am firmly convinced that this is the case. This conviction has created my moral obligation to inform you of these impressions in my letter of July 20, 1978 for whatever their value.

(2) Besides a sincere esteem for the known scientific stature of Professor PANOFSKY, I have a sincere gratitude for the courtesy, time and assistance he has provided for me on a number of occasions. My letter to him was not the result of a one day decision. It was the climax of a series of events which left me no other conceivable alternative to serve the interest of our community. Professor PANOFSKY is entirely noninformed of this background which lead to my letter to him. In essence, certain quite valuable, but truly malcontent physicists were in the process of impleme gestures which, in my view, would be highly detrimental to the U.S. community of basic research In my view (which, again, could be entirely erroneous) a preventive action was needed. I was in a unique position because, as you know, I am the recipient of what appears to be the first federal research grant of non-quark inspiration and, also, I am the editor of a Journal which is already emerged as dedicated to the sole pursuit of physical truth, whether of quark or nonquark inspirati My letter to Professor PANOFSKY was conceived, intended and used as a preventive tool. But, ag it was for me reason of considerable personal regret. This is the part of my letter to you of July 1978 which I referred to in the footnote of page 5 as going "beyond a letter, even written in candid language". Of course, I am glad to release the end results of this action. But under no circumstar whatsoever I intend to disclose names. At whatever price.

(3) All this commotion, so to say, boils down to a very simple argument. These highly malcontent physicists are, in my view, valuable and responsible scientists. Their requests are, also in my v

quite reasonable. In essence, they ask that federal research support for quark-oriented studies must continue. However, jointly, NSF must initiate support of studies along fundamentally different lines to achieve a well balanced conduction of research on this fundamental problem. I must acknowledge that this request is entirely reasonable and I must endorse it in its entirety. The area which has been a primary reason of irritation (my personal case is known to you, but apparently there are others, of course, of different nature) is the current refereeing of research proposals on hadron physics by NSF. In essence, to my perhaps erroneous view, NSF sends proposals of this nature to leading physicists in the field who are experts in quark models. The criticisms is that NSF should not identify quark experts with experts in hadron physics. This is the reasons behind my recommendation to you of July 20, 1978 in relation to the operations for refereeing.

My personal interpretation of this occurrence is the following. It appears that NSF is not yet aware of the fact that the physics community is becoming more and more divided on the issues of hadron structure into two opposite groups : quark-believers and quark-non-believers. Physicists in the first group are known to NSF. Those of the second not yet. Thus, there has simply been the lack of sufficient information to NSF to prevent malcontent and also misunderstandings.

The point, however, remains. To understand the occurrence, you should be aware that, in essence, statements of technical criticisms by a quark-supporter on non-quark-oriented proposals are entirely and completely distrusted by quark-non-believers. A specific example might be useful. Here I am glad to expose myself in the hope that it might be of some value for our community. After studying and conducting research on hadron physics for over a decade I must honestly confess that I simply do not believe in quarks. This implies that I do not believe in the technical statements or criticisms moved by physicists when based on quark arguments. For instance, when a quark expert tells me that Einstein's special relativity is valid within a hadron because of such and such argument I consider this a mere expression of his personal opinion. Any different view would imply that the assumption that the quarks are the constituents of hadrons, complemented by the assumption that they confine, complemented by the assumption that ....etc. etc., produce an unequivocal, incontrovertible, scientific truth.

It is vital, in my personal view, that NSF acquires full consciousness of this occurrence and takes all the necessary steps to prevent delicate situations which could lead to an unpredictable outcome. This essentially demands the consciousness that any critical statement of an NSF proposal based on quark arguments might be fundamentally inconclusive because the quark models are fundamentally inconclusive and they will remain so until the problem of the identification of quarks with physical particles is accomplished in an incontrovertible form. Confinement has created an area of further potential danger for NSF. The reason is that even assuming that a genuine mechanism of confinement will be achieved sometimes in the future, this will leave the problem of the experimental identification of quarks totally unchanged. It would simply shift such identification while within hadrons. In other world, confinement, if (and IF) achieved, will still leave quark models fundamentally unresolved. They will remain so until a new technology emerge capable of experimentally proving that, say, the π^0 has precisely a quark and an antiquark as constituents with precisely such and such data, etc. You can now understand the profound irritation of certain physicists when criticism on their work by quark believers based on quark arguments is taken seriously and used as a decisional tool. It is here where NSF, in my view, must exercise extreme scientific caution.

To conclude this presentation of my personal views, I am happy to report that I did achieve indeed my objective, that of quieting down excessively malcontent physicists and delaing their intended quite delicate action. This was the result of my letter to Professor PANOFSKY, as well as the fact

that I subsequently received from Governmental Agencies other than NSF for my refereeing research proposals which could lead to non-quark approaches to hadron structure (this was indeed invaluable for my action). However, this simply resulted for NSF in gaining some time for studyi the situation and adjusting to a fast changing situation of research in the sector. As for the future is concerned, permit me to stress that I do not intend to make a second intervention and that from here on I would like to abstein from partecipating in disputes ultimately related to the controversial topic of funding of quark hypothesis.

I should add that you are receiving a warm and sincere support by both quark believers and quark-non-believers. As a matter of fact, a number of physicists are relying their hopes in you. Almost needless to say, this is invaluable for how NSF is seen from outside researchers.

Speaking on personal grounds, you have my sincere esteem and my unconditional support. If I can be of any help in your rather difficult task at any time, please do not hesitate to contact me.

Very Truly Yours

Ruggero Maria Santilli

rms|cgg

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 14. 1980

Dr. L. F. BAUTZ
Deputy Director
Division of Physics
NATIONAL SCIENCE FOUNDATION
Washington, D.C. 20550

Dear Dr. Bautz,

Following your letter of February 4, 1980, I am here respectfully submitting a proposal for support of the

THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS.

The original of the proposal is enclosed to this letter jointly with two copies.

Ten additional copies have been separately mailed to you.

In addition, we have separately mailed to you one complimentary copy of the

PROCEEDINGS OF THE SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (two volumes)

Finally, I enclose a list of mathematician and physicists, experts in the Lie-admissible formulations, in case of any assistance for the selection of qualified referees.

I remain at your disposal for any additional information or assistance you might need.

Your consideration and time has been appreciated.

Very Truly Yours

Ruggero Maria Santilli
Chairman, Organization and
Admission Committee
THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS

RMS/ml
encls.

PART XXII:

REJECTION OF THE PRIMARY I.B.R. APPLICATION BY THE DEPARTMENT OF ENERGY IN 1981–1982

Ruggero Maria Santilli

Editor in Chief
Hadronic Journal

Home Address:
28 Cross Street
West Newton, Massachusetts 02165
Telephone (617) 964-1684

October 27, 1980

Drs. B. HILDEBRAND, D. PEASLEE, and W. WALLENMEYER
DOE, Division of High Energy Physics
Washington, D.C. 20545

Dear Bernard, David, and William,

I am happy to report to you that on October 25 (Saturday), 1980, my wife Carla and I have signed the Purchase and sale agreement for the acquisition of an 18 rooms Victorian house located inside Harvard University, one block from the Old Yard. As you can see from the enclosed copy of the purchase and sale agreement, the price is a knee trembling $[redacted]

The primary purpose for our embarking in such a venture is our firm determination to organize a new center of research called

THE INSTITUTE FOR BASIC RESEARCH

The objective of the new Institute is to gather and coordinate the best possible brains in experimental physics, theoretical physics and mathematics to pursue fundamental physical knowledge of primary energy-related orientation (strong interactions and the controlled fusion in particular). Officially the new Institute will be presented as a new research facility which "complements" the facilities already existing in the area. Unofficially and confidentially, the idea to organize the new Institute is an expression of a growing concern, nation wide, on the rather clear monopolistic restriction of research on strong interactions in leading Institutions along only quark oriented lines, and the need of a more balanced use of public funds. In fact, several precautions have been taken to ensure the genuine freedom of the researchers of the new Institute, and the consideration of all valuable or otherwise promising lines.

Beginning from this morning (Monday, October 27), I have initiated the application to local lending institutions for a first mortgage of $ [redacted] (for about 70 % of the value). A formal bank committment is needed on or before November 25, 1980. If everything goes as planned, the new Institute may initiate the operations on January 1, 1981. In particular, we feel confident to be self-sufficient for the purchase of the building of the Institute.

However, in regard to the operations of the new Institute we are currently facing a predictable negative cash flow for the first two years. This is due to several factors, including the need of administrative-accountant personnell (A DIRECTOR for the new Institute will be needed, but at some later time -- personally, I do not intend to take this post because I am primarily interested in conducting research). I am afraid that this negative cash flow could be detrimental for our capability to obtain a first mortgage. More explicitly, we are trying to identify revenue shources capable of providing financial selfsufficiency for the first two years of operations. However, we fear that our solutions may not result to be truly convincing to the conservative New England Banker. The entire project might therefore be jeopardized because of this aspect.

The funds needed for the first two years of operation are of the order of $ 100,000 ($ 45K for calendar 1981 and $ 55K for calendar 1982, including administrative personnell, but excluding the Director). A detailed itemization is at your disposal upon request. Please see whether DOE can support the initiation of this new research facility. Also, please take into consideration that the time factor is rather crucial in this instance. In fact, we need a bank committment on or before November 25 of this year. After that date, the property might still be available, but its price will be definitely higher, and even double (the property was located because of personal contacts with the owners, it was never on the market, and, once its availability is known, its price becomes a function of personal needs owing to the quite hot location).

On more specific grounds, please consider the possibility of adding $ 100,000 to my existing contract ($ 45K to that for 1980-1981 and $ 55K to its second year). A formal decision prior to November 25, 1980 on this matter would be determinant for the entire project. If this is not possible, a letter of interest would be also welcome. Alternatively, I would appreciate the authorization of releasing the name of one of you to selected lending Institutions. In this way you could verbally indicate the existence of an interest. However, please keep in mind that New England Lenders are traditionally conservative (they refuse to fund even the Polaroid for the initial operations). To be truly effective, a formal resolution on the availability of the funds, and the date appears to be needed.

As I have done in the past three years, I would like to rely entirely on your judgment and vision. The payback could be quite intriguing. In fact, a new Institute specifically organized for the genuine pursuit of fundamental knowledge in energy-related problems outside the current mumbo-jambo of academic dances could likely achieve breakthroughs of fundamental character.

Best Personal Regards

Ruggero

Ruggero Maria Santilli
Chairman of the Board of Trustees
and Acting Director
THE INSTITUTE FOR BASIC RESEARCH

P.S. I am sorry that I did not have the time to make you a detailed report on the THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS. It was a true success. We had some selected 30 participants, half pure mathematicians, and the rest theoretical and experimental physicists. Most determining was the participation of an experimentalist in neutron interferometry from Europe, Prof. Rauch. He clearly indicated that the experimental information is such to warrant doubts on conventional laws for the strong interactions. It was a quite emotional moment for all. The Proceedings look like a genuine contribution to knowledge. We shall have three volumes (rather than two for the meeting of 1979). Also, the Proceedings will be typeset. The Journal has purchased all the equipments and trained the personnell. I am typing this letter to you on a special IBM Composer to give you a first hand feeling of the selected style (I have been told that it is better than that of other Journals .....).

Research Grant Application

Submitted to the

U.S. DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street

Cambridge, Massachusetts 02138

Tel. (617) 864-9859

entitled

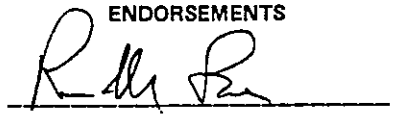
THEORETICAL STUDIES ON LIE–ADMISSIBLE FORMULATIONS

Principal Investigator

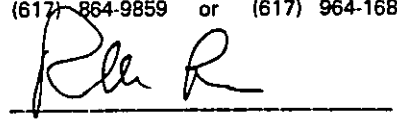
Ruggero Maria Santilli
Soc. Sec. No. 032–46–3855

Proposed Starting Date:	Proposed Duration:	Amount Requested:
June 1, 1982	24 Months	$339,975

ENDORSEMENTS



R.M. Santilli
Principal Investigator
(617) 864-9859 or (617) 964-1684



R.M. Santilli
President, The Institute for Basic Research
(617) 864-9859 or (617) 964-1684

Accounting Firm of the Institute
Vaccaro and Alkon PC, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
Att: Mr. R. Alkon, President
Tel. (617) 969-6630

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
Att: Mr. J.R. Grassia, Senior Partner
Tel. (617) 956-1700

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ABSTRACT

The present application is for the continuation of research initiated by the Principal Investigator (Professor Ruggero Maria Santilli) in 1978/1979 under DOE contract ER–78–S–02–4742.A000, and continued in 1979/1980 under contract AS02–78ER–4742, in 1980/1981 under contract DE–AC02–80ER10651 and in 1981/1982 under contract DE–AC02–80ER10651.A001.

The objective of the research is to achieve experimental, theoretical, and mathematical knowledge of whether intrinsic characteristics of particles (magnetic moment, spin, etc.), as currently measured under long range electromagnetic interactions, are preserved or altered in the transition to the different physical conditions of the strong interactions.

The relevance of the research can be seen in physics, mathematics, and engineering. Particularly important is the relevance for controlled fusion. In fact, clear knowledge of the intrinsic characteristics of nucleons under strong interactions is important to achieve controlled fusion (e.g., the value of the magnetic moments of nucleons under very high pressure, densities, and temperatures is important to achieve magnetic confinement).

The available experimental basis is primarily of nuclear character, and initiates with old evidence (presented in well written treatises) that the magnetic moments of nucleons change under nuclear conditions, as apparently necessary to interpret the total nuclear magnetic moments. This hypothesis was subsequently abandoned, until its coordinated study was resumed by the Principal Investigator under DOE support. Additional experiments via netron interferometers measure the spin precession of neutrons under joint electromagnetic and strong interactions. Available experimental data show unexplained clusters of points outside the curve predicted by conventional electromagnetic quantities and are unable to recover the 720° needed to establish the exact validity of the SU(2)–spin symmetry under strong interactions. This proposal contemplates the formulation and study of a series of experiments to achieve the resolution of the problem in due time.

The proposed organization is as follows. Two Senior Research Associates (an experimentalist and a theoretician) are recommended beside the Principal Investigator, owing to the complexity and diversification of the project. The proposal also includes the organization of the Fifth (1982), and Sixth (1983) Workshop in Lie–admissible Formulations, as well as of the Second International Conference in Nonpotential Interactions to be held in 1984.

The project is expected to result in a number of articles, monographs, and conference proceedings.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

August 19, 1981

Dr. DAVID C. PEASLEE
Physics Research Branch
Division of High Energy Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545

Dear Dr. Peaslee,

I hereby respectfully submit the enclosed original of the research grant application entitled:
THEORETICAL STUDIES ON LIE-ADMISSIBLE FORMULATIONS
under administration of The Institute for Basic Research. A number of copies of the application have been separately mailed to you.

The initiation date of the contract has been suggested at June 1, 1982 in order to preserve the continuity of research at the expiration of the existing contract number DE-AC02-80ER10651.A001 on May 31, 1982. The proposed duration is 24 months. The amount requested is $ 339,975.

During the consideration of the proposal, as well as of the amount requested, I would appreciate the courtesy of keeping into account that this is the first proposal of our Institute. Adequate funding will therefore permit the growth of a new research facility with a considerable potential in the free pursue of fundamental scientific knowledge.

I hope that the application is sufficiently informative on the continuation of research under DOE support, including general information on our Institute (as Appendix A), while I remain at your disposal for any additional information you may desire.

Sincerely Yours

Ruggero Maria Santilli
President
THE INSTITUTE FOR BASIC RESEARCH and
Principal Investigator

cc.: Drs. B. HILDEBRAND, and W.A. WALLENMEYER, DOE

RMS, ml

encl.

Department of Energy
Washington, D.C. 20545

NOV 6 1981

Dr. Ruggero Maria Santilli
President
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Your research proposal entitled, "Theoretical Studies on Lie-Admissible Formulations," submitted by the Institute for Basic Research has been received.

Your proposal is now under review in the Division of High Energy Physics and as soon as a decision with respect to support can be reached, you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him.

We appreciate your interest in submitting this proposal to DOE, and we will be pleased to give it review and consideration for support.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics
Office of High Energy and Nuclear Physics

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

October 22, 1981

Dr. DAVID C. PEASLEE
Division of High Energy Physics
Physics Research Branch
DEPARTMENT OF ENERGY
Mail Station J. 309
WASHINGTON, D.C. 20545

CERTIFIED MAIL

RE: Research Grant Proposal entitled
"Theoretical Studies on Lie-admissible Formulations"
Principal Investigator: R.M.Santilli
Submitted on August 19, 1981

Dear Dr. Peaslee,

As requested by you, I have separately mailed to you three parcel posts (certified) containing a collection of articles for the refereeing of the proposal above. A sample of the collection, entitled
"Primary bibliography on the problem of the exact or approximate validity of the SU(2)-spin symmetry under strong interactions"
has been enclosed to this letter for your consideration.

Please keep in mind that the Research Grant Application under consideration by your Office deals with a difersification of applications ranging from classical mechanics (trajectory problems in atmosphere) to particle physics (the open nature of the structure of the strong interactions). The selected articles for refereeing has been restricted to only one profile of the application, that regarding the intriguing situation of spin. At your discretion, we remain at your disposal to send you additional selections of articles in other aspects of the application.

Owing to this diversification of applications, we would like that the application above be considered independently from other applications by our Institute. Also, my current DOE support expires on June 1, 1981, and I would be truly grateful whether the consideration of the proposal can be expedite within reason, of course.

Very Truly Yours,

Ruggero Maria Santilli
President
RMS-pm
encls.
cc.: Drs. Wallenmeyer and Hildebrand, DOE

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 12, 1981

Drs. B. HILDEBRAND and W.A. WALLENMEYER
Division of High Energy Physics
Physics Research Branch
Department of Energy
Mail Station J-309
WASHINGTON, D.C. 20545

Dear Bernie and William,

Without a doubt, this is the most important letter I have written to you until now. As a result of your consideration of the content of this letter, the entire financing policy and planning of our Institute will be set for years to come.

Stated quite simply, the purpose of this letter is to recommend that the research initiated under your support in 1978 shall continue under your support in a way as smooth and harmoniously as possible with the scientific objectives of the Department of Energy, as well as with the various academic and national institutions supported by DOE.

I am confident of your sincere and best intention to study this possibility. Nevertheless, permit me to indicate that, from the viewpoint of our Institute, a major problem is time. In essence, a number of rather important decisions regarding the financing of our Institute are being delayed, and will be delayed to give to you the necessary time to reach a decision. However, we can delay our decisions only until mid-January 1982. Further delays beyond that date may imply excessive risks for our scientific programs. Therefore, after mid-January 1982, you should feel free to continue the investigation of the case and/or of individual proposals without any need of rush. However, you should expect the possible existence at our Institute of scientific policies which are not necessarily compatible with those of the DOE.

The purpose of this letter is precisely that of preventing this possible occurrence. On more specific terms, my proposal is the following:

1. **Election of the Director of our Institute.** As you know, we have an opening for the Director of general operations. We would appreciate your advice in the selection of the appropriate person as well as in the finalization of his functions. As far as I can see, the person should be well received by our neighbors as well as by DOE so that he can do a good job in smoothing out situations and resolving possible discrepancies. We are not expecting, of course, the appointment of this person by mid-January 1982. Nevertheless, we would appreciate whether you can communicate your thoughts on the opening by that time.

2. **Possible support available at the expiration of my contract on May 31.** As you know, my existing contract expires on May 31, 1982. The first application of our Institute to your Office has been that of the continuation of this support from June 1, 1982 on. As communicated separately to Dr. THEWS of your Office, it is of the utmost importance that a decision regarding this application be reached as soon as possible. Also important for our program is that we have the capability of supporting at least one additional physicist besides myself (as well as the Director, if appointed). I would appreciate whether by mid-January a decision (even informal) can be reached on the application.

3. **Possible available support for the future growth of our Institute.** We finally need some input in regard to future growth so that we can plan for an orderly expansion of our Institute in a way compatible with your more general plans and objectives. I would, therefore, appreciate whether by mid-January you could give us some indicative and orientational figures of ceiling for the applications we will meaningfully submit to your division. Please keep in mind that the demand for administration of grants by our Institute is great for several reasons (location, minimal overheads, genuine scientific freedom, etc.). It is, therefore, important for us that we set guidelines for growth, say for the next 4-5 years, not later than mid-January 1982.

I hope that in this letter I succeeded in communicating our sincere best intention to organize our Institute in a form compatible with more general plans at DOE. Most of all, I hope you will see in this offering the expression of my sincere gratitude for your scientifically and humanly invaluable, past support.

I shall remain at your disposal for any additional assistance you may need.

Very Truly Yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS/pm

cc: Board of Governors and Officers, I.B.R.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 11, 1981

Dr. ROBERT THEWS
Division of High Energy Physics
Physics Research Branch
Department of Energy
Washington, D.C. 20545

RE: Research Grant Applications entitled
"Theoretical Studies on Lie-admissible algebras"
submitted on August 19, 1981

Dear Dr. Thews,

Following our phone conversation of November 5, I am taking the liberty of confirming or elaborating the following aspects, while I remain at your disposal for additional assistance you may need.

1. SUBMISSION OF PROPOSAL. I would like to confirm that the application has not been submitted to other Governmental or Private Agencies for funding, nor do we contemplate such a submission at this time.

As indicated in the submission letter addressed to Dr. D.C. PEASLEE of August 19, 1981, this is the first application of our Institute. Numerous scientific initiatives and activities are dependent on this application. We would, therefore, welcome any suggestion as to whether the application should be jointly submitted elsewhere. At any rate, we shall abstain from such submission unless recommended by your Office, or following consultation with your Office.

2. REFEREE PROCESS. We are concerned that, at the time of our conversation of November 5, the referee process had not been implemented since the submission in late August. We are also concerned with the fact that the experts in the physical applications of the Lie-admissible algebras are very few at this moment, on a world wide basis, while the existing specialized literature in the field has now surpassed the mark of 5,000 published pages. We are concerned that scientists in other fields, even though in good faith, may be tempted to pass judgments without a technical knowledge of the topic. Finally, we are concerned of the possibility that the reaching of a final decision on the application might take excessive time (e.g., if the referees are not selected in the field, and have to study a voluminous literature), to the detriment of all.

As you know, we are currently spending large public funds in strong interactions. Most of these funds are spent under the assumption of the validity for the strong interactions of the basic laws established for the electromagnetic ones. Our grant application, to our

knowledge, is the only application specifically devoted to mathematical, theoretical, and experimental studies to verify the basic laws via direct and specific experiments. Within such administrative-scientific setting, we believe that it is in the best interests of all to reach a decision on the application as soon as possible.

In view of these (and other) reasons, we are respectfully submitting for consideration by your Office the following alternatives.

Alternative I: action on the proposal without external referees. The relevance of our research, I believe, is out of the question. Our capabilities to perform have been established by the preceding four years of support. Finally, your Office has been kept fully informed of all advances. In view of these aspects, I would, therefore, gratefully appreciate the consideration of reaching a decision without external refereeing, as it was done in preceding cases of our contracts.

Alternative II: action following referee process. In this case, I would appreciate whether experts in the field of the proposal are consulted. To facilitate your task, I enclose a list of experts in Lie-admissible formulations. Finally, and most importantly, **please mail to us all referee reports as soon as available.**

3. REFEREEING MATERIAL. Three copies of the application were mailed (via certified parcel post) to Dr. Peaslee. Additional copies are at your disposal on request. We also mailed three sets of a collection of papers related to the problem of spin under strong interactions. The understanding specified in a letter to Dr. Peaslee was that this material relates to only one aspect of the grant application. In fact, our research applies to a considerable number of fields ranging from Newtonian mechanics, to classical field theory, to statistical mechanics, and to quantum mechanics, while the selected papers were only in the problem of spin.

All references listed in the application are available in research libraries. Nevertheless, to facilitate the referees, we would be glad to mail you additional copies.

4. INFORMATION ON OUR INSTITUTE. Following your request, I have instructed our attorney to mail you all pertinent legal data, such as the names of the Officers and of the Governors (see enclosed letter).

In regard to the internal organization, such as that of the Board of Trustees-Advisors as well as the internal operational chart, they are still under finalization at this time. You can rest assured that they will be mailed to you as soon as available.

5. EVALUATION OF OUR INSTITUTE. Permit me the liberty of suggesting that an evaluation of our Institute during the referee process be avoided as much as possible. In fact, I am concerned that such an evaluation may raise and create unnecessary problems.

(a) Legally, our Institute has the same status as that of Harvard and MIT, and we see no point in entering into this aspect at the referee level.

(b) Administratively, we can administer a limited amount of federal grants at a fraction of the cost of our neighbors (specifically, our overheads are of the order of 30% while those of other institutions range from 65% to 75%, to my knowledge.) The advantage in favor of our Institute is self-evident. However, the raising of the issue during the formal referee process may be unnecessarily detrimental to our neighboring friends.

(c) Scientifically, our Institute was born for the conduction of research which is currently not conducted in other local Institutions, such as the experimental verification of Einstein's special relativity under strong interactions or, more specifically, under the conditions of the controlled fusion. An appraisal of this program on a comparative basis with those of other local Institutions could only create a host of unnecessary problems. In fact, the same research should be conducted at those institutions, owing to the considerable amount of public funds spent there in strong interactions under the assumption of conventional electromagnetical law. An evaluation of our scientific program would, therefore, inevitably raise the problem (whether now or in the future) of ascertaining the reasons why the same research has been rather vigorously precluded in other Institutions until now.

Owing to these and other aspects, I would like to suggest that the referee process be restricted to the scientific merit of each individual proposal, without any consideration in regard to our Institute.

However, we would much welcome a visit by you as well as any other member of the DOE. In actuality, this would be the best way to reach an evaluation of our Institute because one direct view is better than one thousand words spoken far away. In fact, only via a direct visit one can see our building, the facility that it offers, its location inside Harvard, and the possibility that it permits to each member to have continuous scientific interactions with individual Harvard scientists.

In closing, permit me to recall the achievements permitted by the DOE support during the preceding four years (such as, the publication of three research monographs and a considerable number of papers by several authors, the support of over fifty mathematicians and physicists, the organization and conduction of four international Workshops on Lie-admissible Formulations, and more recently the organization of the First International Conference on Nonpotential Interactions, as presented in detail in the application). Also, permit me to express the hope that these scientific programs can indeed be continued under DOE support. In case I can be of any assistance, please do not hesitate to contact me.

Sincerely,

Ruggero Maria Santilli
Principal Investigator of the Application
and President

RMS/pm

cc: J.R. Grassia, Esquire
Boston, MA

NOVEMBER 12, 1981 VERY CONFIDENTIAL MEMO

TO: Bernie and William
FROM: Ruggero
SUBJECT: comments on formal letter of same date

I am receiving rather preoccupying rumors regarding apparent pressures on you by Cambridge academicians against our Institute. I am fully aware of the difficulties of your situation. I would like therefore provide you with some background information so that you can be in a better position to reach mature decisions.

I belive I have given you proof of loyalty during the past several years. The proof I consider the best is my silence in regard to the uncountable academic dances which have occurred on my studies under your support at Harvard University and the Massachusetts Institute of Technology beginning with fall 1977 (*). This silence is due to my view that Governmental Officers should not be un-necessarily involved with vulgar academic greed. However, you should be aware that some of these episodes have been particularly serious, because they apparently imply abuses of scientific power in direct conflict with national interests, as well as the pursuit of scientfic knowledge, not to mention the human profile.

I am sure you are aware that the perception of the educated society at large on the conduction of science by academic institutions is changing rapidly. The terms "academic corruption", while virtually absent only a few years ago, are now heard more and more frequently. I am not referring here to corruption in the sense of stealing money. No. I am referring to the use of academic power to prevent the pursuit or jeopardize the establishing of undesired novel scientific knowledge, which is much more damaging to society than ordinary corruption as defined in the current code of laws. A number of educated persons are now under the feeling that this type of corruption has reached such a level, to represent a serious threat to National Interests. Also, the virtually unanimous feeling is that this is something happening at the academic, and not the governmental level. You are therefore completely out of the problem, to my understanding. However, you should be fully informed of its existence, and urged not to underestimate it, so that we can initiate coordinated, preventive and containing actions. The formal letter of this date is inspired by the hope that these latter objectives are achieved in a way as smooth and orderly as possible.

However, the background scientific issues should not be ignored. Actually, they are the central aspect of the situation. Permit me, therefore, to review them as they are perceived by scientists of proved ethical standards (This, I am sure, is not reported to you by other people).

1. The experimental verification of the validity or invalidity under strong interactions of Einstein's special relativity and other basic laws must be conducted. Period. There are two ways to do it. We can achieve the objective in a scientific orderly fashion, or following a crushing scandal. Accademicians who oppose these fundamental tests, either openly or in a criptic way, are clearly corrupt in my view. In fact, the tests, in the final analysis, can also confirm the validity of the atomic laws under strong interactions. By comparison, all other experiments currently under way or under consideration, even though definitely valuable on scientific grounds, have a comparatively minute scientific importance. Also, these tests are clearly essential for future technological advances of truly fundamental character. The opposition to these tests by

(*) The sole exception I have made is the recent episode of stall by MIT of the fundamental experiment on SU(2)-spin. The reason for the exception is, first of all, that the sad episode has a quite international character involving Governmental Agencies in three different Nations; and, second, because the episode, unless monitored and controlled, has all the ingredients of reaching front pages of dayly newspapers in France, Austria, and, inevitably, the U.S.A.

corrupt academicians, therefore, is a threat to National interests. At any rate, the existing direct tests DO NOT confirm the validity of the special relativity, and the need of repeating the tests is clearly unquestionable. I am referring here to the fact that the experiments available on the spinor symmetry under strong interactions do not reproduce the angle of precession predicted by Einstein's theory; the experiments on optical activity of neutron within matter are far from being convincingly in agreement with Einstein's relativity; additional experiments under nuclear forces clearly and grossly violate the T-symmetrt predicted by Einstein's relativity, etc. etc. Last, but not least, we are currently spending truly large amounts of tax-payers money in strong interactions. A majority of these public funds are spent under the assumption (very often, tacit) that the basic laws are valid. You can therefore understand the concern of educated persons to a situation of this type.

Also, you should know that the scientific power of corrupt academicians, after beeing unchallenged for decades, has now reached blinding dimension of genuine irresponsibility. In fact, several of these "scientists" have a BIG MOUTH. They talk openly of their opposition to visitors during their known "Cambridge lunch breaks" and other occasions, with statements such as "Santilli writes long and useless letters" following a kind, respectful and detailed proposal to initiate tests. The point is that the quests smile in their presence, but subsequently, they call either me, or eventually their representatives expressing concern for the alleged existence of scientific corruption in major academic institutions. The prestige of these institutions is still mostly intact now. However, my fear is that one big scandal, and their prestige would be tarnished for generations, with a consequential major damage to the Country and science at large. But, unless these people are brought to a responsible behaviour, and their big mouth closed, the risks are real.

2. The studies on the generalization of the atomic mechanics into a form suitable for the smaller nuclear and hadronic structures must continue. I am referring to the studies initiated under your support from 1978, which have resulted into so many international initiatives and results, and whose status will be reviewed at the forthcoming First International Conference on Nonpotential Interactions to be held in France in January 1982 under financial support by the French Government, and with the participation of virtually all developed Nations, including teams from China and the U.S.S.R.

These studies are also considered by a number of observers as being important for National interests, and future technological advances involving strong interactions. For instance, the achievement of a meaningful controlled fusion will likely demand the use of a generalized mechanics (including its statistical and plasma theories) for the conditions of hadrons under very small mutual distances, high temperatures, and large energies, in exactly the same way according to which the design of a nuclear reactor via Newtonian mechanics is pure fantasy.

Academic corruption, interests, and greed are against these studies more often than you may believe. But then, the threat to national interests, in my view, is there and real. It boils down to attempt the prevention of the achievement of new knowledge which can affect directly the life of all our children.

3. The Institute is a reality. The primary reason for the organization of our Institute is the fact that the conduction of the research of aspects (1) and (2) above in other academic institutions in Cambridge has been demonstratedly impossible beyond any conceivable doubt. Now that the Institute exists, I believe that it can be valuable, scientifically and administratively. In fact, we can complement the research done at other institutions, by therefore avoiding a public confrontation, while we can administer governmental funds at a fraction of the costs of other Institutions. In short, the Institute was born because it has a natural place, function, and future. It is here to stay. Academicials who oppose its existence and funding are only looking for troubles.

I though it is important you are fully informed of these things, as unpleasant as they may be, The hope is that your full knowledge of the delicate scientific moment may result to be valuable in your own, difficult administrative-scientistif function. But, whatever the future will bear, please rest assured that my loyalty and gratitude to you will not change.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

ary 11, 1982

CERTIFIED LETTER
RETURN RECEIPT
REQUESTED

OBERT THEWS
ion of High Energy Physics
:s Research Branch
TMENT OF ENERGY
NGTON, D.C. 20545

Dear Dr. Thews,

In comformity with the existing regulations at the Department of Energy, I am hereby asking that you mail me promptly and in their entirety, all the referees reports on our application: "Studies on Lie-admissible Formulations".

Very Truly Yours

Ruggero Maria Santilli
President

RMS-mlw

cc.: [redacted]

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

February 11, 1982

Honorable JAMES B. EDWARDS,
Secretary
Department of Energy
Washington, D.C. 20585

VERY URGENT

Dear Mister Secretary,

I feel obliged to bring to your personal attention a recent preliminary decision by Dr. B. HILDEBRAND of your Division of High Energy Physics to terminate all funding of our research.

Our studies deal with a truly fundamental problem, the validity or only approximate character for the strong interactions of the electromagnetic basic physics laws. Our studies are therefore directly relevant for the totality of research on strong interactions supported by DOE and, in particular, for the controlled fusion. As you can see from the enclosed letter to Dr. W. A. WALLENMEYER, Director of the Division of High Energy Physics, the termination of our research (which is the only theoretical one in the field), may create a rather substantial administrative problem of scientific accountability vis-a-vis to the tax payer, because billions of public funds would be invested in strong interactions under the mere assumption of the belief of the basic laws.

I believe that the risks of such sizable administrative implications are inappropriate, particularly at this delicate moment for DOE of which we are all aware. You should know that, besides the lack of experimental resolution of the basic laws of strong interactions in a scientifically credible way, we have continued to see for years the spending of large public funds in theoretical research, such as those along quark conjectures, which can be at best qualified as academic exercises of curiosity without any conceivable practical value.

The situation in our community is therefore truly tense. The continuation of financial support for quarks conjectures and other esoteric studies, if matched with the truncation of funds for more serious work on the fundations, may have such implications, that even a "class action" cannot be excluded. The survival of DOE, let alone its effective continuation, could be at stake because of administrative unbalances.

Owing to these and other reasons, permit me the liberty of urging your personal intervention in the case, to prevent the official implementation of Dr. Hildebrand's personal views, and to ensure that public funds are truly dispersed in a more scientifically and humanly equitable way.

In case I can be of more detailed assistance to you, please do not hesitate to let me know.

Very Truly Yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President
RMS-mlw
encls.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

February 11, 1982

Dr. ALVIN W. PRIVELPIECE, Director
Office of Energy Research
Department of Energy
Washington, D.C. 20585

VERY URGENT

Dear Dr. Privelpiece,

I feel obliged to bring to your personal attention a recent preliminary decision by Dr. HILDEBRAND of your Division of High Energy Physics, to terminate all funding of our research.

Our studies deal with a truly fundamental problem, the validity or only approximate character for the strong interactions of the basic electromagnetic physical laws. Our studies are therefore directly relevant for the totality of research on strong interactions supported by DOE, with particular reference to controlled fusion. As you can see from the enclosed letter to Dr. W. A. WALLENMEYER, Director of your Division of High Energy Physics, our research is the only theoretical one in the problem of the basis law. Its termination would therefore create a rather substantial problem of scientific accountability vis-a-vis the tax payer, becuase billions of public funds would be invested in strong interactions under the mere assumption of the belief of the validity of the basic laws.

The risks of such sizable administrative implications are inappropriate in my view, particularly at this delicate moment for DOE of which we are all aware. You should know that, besides the lack of resolution of the basic physical laws for the strong interactions in a scientifically credible way, we have seen for years the dispersal of huge public funds on theoretical research, such as those along quark conjectures, which can be at best qualified as academic exercises of curiosity.

The situation in our community is therefore truly tense. Hildebrand's decision, if formally implemented, ultimately implies the continuation of large support for esoteric research, joint with the truncation of support for more serious studies on the foundations. The emerging unbalance may have such implications, that even a "class action" cannot be excluded. The survival of DOE, let alone its effective functioning, could be at stake.

Owing to these and other reasons, I am taking the liberty of urging your personal intervention in the case, to prevent the official implementation of Dr. Hildebrand's views, and to ensure that public funds are truly dispersed in a more scientifically and humanly equitable way.

In case I can be of more assistance for detailed technical implications, particularly in regard to the implications for controlled fusion, please let me know.

Very Truly Yours

Ruggero Maria Santilli
President

RMS-mlw
encls.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

February 11, 1982

Professor Ruggero Maria Santilli, President

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
U.S.Department of Energy
WASHINGTON, D.C. 20545

EXPRESS MAIL
CERTIFIED

Dear Willy,

Robert Thews informed me yesterday of a preliminary negative decision on my grant application. After talking with you, I am under the impression that the decision was reached by Bernie Hildebrand without your knowledge, and, perhaps, by following questionable advice he may have received at MIT and/or other compuses.

Prima facie, Bernie's decision means zero funds for our new Institute, and the continuation of millions of dollars of theoretical support to MIT and other local institutions all of us know too well. This is an extreme disparity which can only promote comparatively extreme reactions, none of which have been apparently appraised by Bernie in sufficient depth.

Permit me to recommend most warmly that you reverse Bernie's decision, establish an administratively more balanced situation in the area, and fund our Institute with small, yet sufficient means. Permit me also to recommend that, owing to the delicate implications of Bernie's position, a final, formal decision be reached as soon as possible.

The human profile. As you know, I have two children and a family to support. On June 1, 1982 my current contract terminates, and I will be without any income. The mere idea of applying for an academic (or other) job is laughable. Bernie has apparently decided to implement this situation on my part, while continuing the supply of large public funds to tenured and highly salaried faculty at MIT and at other campuses. It appears that Bernie has totally ignored the extreme exacerbation of the human conditions of our community which are inevitable from unbalances of this type.

The scientific profile.Let there be no doubt that the scientific values, results, and achievements of our grant have absolutely no match in any other line of theoretical studies under your support during the same period. We all know that millions of dollars of public funds in the hands of other theoreticians have resulted in minute advancements in established lines along quark conjectures and related fields. A comparatively minute investment of an average of $ 60K per year has permitted our group a list of achievements too long to be repeated here. At any rate, the achievement of the generalizati-n of the Hamiltonian Mechanics into a covering Birkhoffian form for contact interactions, or the achievement of the foundations of the hadronic generalization of quantum mechanics speak for themselves. They are substantial scientific events, no matter what other physicists say. Apparently, Bernie has decided to truncate these efforts for the generalization of the foundation of contemporary physical knowledge, in favor of huge funds invested in incremental advancements by other physicists. I am under the impression that Bernie has substantially ignored the transparent differentiation in our favor. Why?

The administrative profile. But the aspect which concerns me most is the administrative implication of Bernie's decision. We are in times of mounting pressures for scientific accountability, particularly in these periods of considerable social difficulties. The U.S. Department of Energy is spending truly immense public funds in strong interactions. All these funds are generally invested under the belief of the validity for the strong interactions of the basic physical laws of the electromagnetic ones. Even though not admitted by possibly corrupt academicians, the technical reasons for doubt are truly substantial, and they are presented by numerous experimentalists in the Proceedings

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

February 22, 1982

EXPRESS MAIL
CERTIFIED

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
U.S.Department of Energy
WASHINGTON, D.C. 20545

RE: Primary research grant application for the funding of our Institute filed to your Office on August 19, 1981 under the title
THEORETICAL STUDIES ON LIE-ADMISSIBLE FORMULATIONS.

Dear Dr. Wallenmeyer,

I have been asked to submit to you this letter and the enclosed material with the request that
- it is formally included as an integral part of the application; and
- it is informally considered for the general administrative practices of your Office.

I remain at your disposal for any additional assistance you may need.

(1) The open problem of the basic physical laws. The virtual totality of experiments on strong interactions currently funded by your Office is based on the assumption of the validity of the basic physical laws. I am referring here to truly fundamental aspects of contemporary particle physics, such as:
- the rotational symmetry, with related Pauli's exclusion principle;
- the time-reversal symmetry, with related reversible dynamics;
- the Hamiltonina-unitary character of the time evolution, and related relativities;etc.

The validity of these laws was established for the electromagnetic interactions beyond any reasonable doubt. The same laws were subsequently assumed as valid for the different physical arenas of the strong interactions without any direct tests until recently.

It is public knowledge that your Office is in the process of funding truly expensive additional experiments on strong interactions, again based on the belief of the validity of the basic laws.

This situation is reason of sincere concern to us as well as to numerous observers throughout the Country. In fact, it has been an historical pattern to test first the basic physical laws, and then considers applications and secondary particularizations. The preservation of this sound administration of Science for the strong interactions appears recommendable, particularly when the experiments under consideration for support imply truly large public funds.

(2) Some administrative implications. It has been established in the technical literatur beyond any reasonable doubt that, in case the basic laws needs even a small revision, the actual, final experimental results are different. Modifications of the basic laws therefore have clear and substantial administrative implications.

Until recently, the scientific community was relatively quiet on the problem of the basic laws, and I personally see no reasons to consider the past. However, more recently there have been truly numerous and authoritative voices of doubts on the validity of the basic laws, as I shall indicate below. They are too many, too qualified, and too convincing, experimentally, theoretically, and mathematically to be ignored. Thus, the situation today is, from an adminustrative profile, basically different than only a few months ago.

Scientific accountability in the use of Public funds clearly calls for due consideratio of these doubts in the current operation of DOE. In fact, if the experiments currently under way in Foreign Countries confirm these doubts, your funding of large experiments on strong interactions based on the old laws clearly acquires questionable tones.

(3) Additional administrative implications for the controlled fusion. Once the technical jargon is removed, the controlled fusion boils down to the laboratory construction of bound states of nucleons. The control of such phenomenon is crucially dependent on the basic physical laws. For instance, if the magnetic moments of nucleons are altered in the transition from the conditions under which they have been tested until now (long range electromagnetic interactions), to the different physical conditions of short range nuclear interactions, this implies a departure from the prediction of orthodox theories in magnetic confinement.

On administrative grounds, an alteraction of the magnetic moment would therefore render a virtual vaste all investments on magnetic confinement, in the sense that the actual, serious, credible,physical outcome would not be proportionate to the investment. A similar situation occurs for virtually all other administrative aspects, e.g., those for inertial confinement, etc.

This is no surprise. The basic physical laws are truly fundamental, and therefore they call for a corresponding primary administrative consideration.

(4) Some theoretical reasons for violation. The only possibility for the rotational symmetry to be exact under strong interactions is when, first, the charge distributions of a hadron is absolutely rigid and admits no deformation whatsoever, for whatever impacts and collisions with other charge distributions. Second, when a number of additional restrictive conditions are met (strict potentiality of the forces, etc.). The verification of all such primitive conditions under strong interactions is remote.

The most natural physical situation is that the charge distribution of hadrons experiences deformation under sufficient impacts and other conditions.The only debatable aspect is the amount of deformation which is permissible under given conditions.

However, deformations of the charge distributions necessarily imply the breaking of the rotational symmetry, as well as, clearly, alterations of the intrinsic magnetic moments (these latter alterations were conjectured in nuclear physics some decades ago, but later ignored for apparent reasons of scientific politics).

Furthermore, the breaking of the rotational symmetry necessarily implies that of the T- and P-symmetry, clearly, because the T- and P-operators have a crucial dependence on spin. This essentially means an irreversible particle dynamics.

Again, this is not surprising. Inspection of our environment establishes beyond reasonable doubts that irreversibility in macroscopic systems occurs via rotationally noninvariant orbits. The argument above is a mere reduction to particle levels. But the technical implications for the entirety of the strong interactions are truly vast. Equally vast are therefore the administrative implications.

But these are only the crudest arguments suggesting violation. The literature contains a diversified litany of numerous arguments, all rendering the violation quite natural. In particular, deviations in nuclear physics are expected to be small for certain physical laws (say, rotation and Pauli's Principle) but truly large for others. For instance 1% deviation on rotation implies over 50% deviation on magnetic moment, and an even bigger deviation on reversibility, as you can read in the technical literature.

(5) Some experimental evidence for violation. The problems under consideration here have been studies at the

WORKSHOPS ON LIE-ADMISSIBLE FORMULATIONS

which I initiated in 1978 while I was at the Lyman Laboratory of Harvard and continued on a yearly based under your support. You will recall that our first meeting was attended by "3^1" participants, including myself. The second was attended by "3^2" scientists (the topic was still too advanced). The third was attended by "3^3". The fourth (of 1981) was already sufficiently well know that we had to contain participation to "3^3".

Owing to the results of these meeting, a formal conference was organized under the title

FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT

which was held at the Université d'Orléans, France, from January 5 to 9, 1982, under support from the French Government, with about 5% support from DOE via my grant (note that the expenses of my invited lecture were paid with French funds-the support was for other members of our group).

Some 100 experimentalists, theoreticians, and mathematicians from virtually all developed Countries attended this formal meeting on the basic laws (NOTE:nonpotential interactions necessarily violate conventional laws, in fact, they are nonunitary in their time evolution and, thus, irreversibleand rotationally noninvariant)The proceedings are currently in print. They comprise some 60 papers divided into three volumes for over 1,500 pages.

Official convoys from major laboratories were present. I have separately reported to you all names of physicists from Eastern Countries, with particular reference to the official convoy from the JINR of Dubna, USSR, and from Peking University.

Regrettably, no representative from major U.S. laboratories were present, particularly from those supported by DOE. This is regrettable, because I had personally solicited the participation by at least an observer via direct letters to the Laboratory Directors (Drs.Vineyard, Lederman, Panofsky, and others). Also, I repeatedly invited you to send a representative from your Office, and you apparently received repeated invitations directly from France. Our insistence was due to our sincere desire for your Office to be informed as much as possible, because of the clear administrative implications. The formal Conference at Orléans was the very best opportunity to do so, and it was unfortunate that your Office missed it.

It is impossible for me to outline the results of the Orléans Conference. I feel obliged however to state,in a way to remain in your formal file,that the experimental information presented at the Conference by distinguished experimentalists in favor of the violation is such that its ignorance can only create huge administrative problems. This letter is to suggest that you take this situation in all due account in all your future funding of the strong interactions, experimentally and theoretically.

First, the violation of the P-symmetry in nuclear physics is well establish-d since years, as you know. We therefore only reviewed it in a marginal way.

Second, Professor Slobodrian (Quebec) and Conzett (Berkeley) reported their experimental collaboration according to which the time reversal symmetry is violated in nuclear physics. The experimental information is already sufficiently detailed to identify the origin of the breaking in the spin symmetry, as clearly stated by the experimentalists in their presentation as well as their recent paper in Phys. Rev. Letters. Other experiments (such as a rudimentary attemt at Los Alamos) were studied in details and dismissed' for numerous technical reasons you may see in the proceedings. Yes, the discovery appears to be final: irreversibility originates at the elemental level of Nature. After all, this solution may be contrary to financial interests of a number of physicists, but it is the most logical and natural one.

Third, Professor Rauch (Director of the Atominstitut of Wien) reported the status of our knowledge on direct,credible measures on spin under strong interactions (no mumbo-jambo theoretical assumptions of quark type in the data elaboration, but only serious measures). He made it very clear that the numbers are not final at this time. But, he made it equally clear that the currently available numbers FAVOR THE VIOLATION. In fact, the direc measures of spin his group has conducted produce a value which is 1% BELOW that predicted by the orthodox physicist assuming a perfectly rigid charge distribution.In addition, Professor Rauch reviewed the recent best measures on neutron-tritium scattering (of 1981 and indicated the existence of a region in which

" .... a partial violation of Pauli's principle can be assumed."

(see enclosures).

There is no need for me to provide any additional information. That above is per se so substantial to deserve the best administrative consideration.

To give an idea of what is going on, I would like to add that, following the Conference, special meetings at very high levels were conducted in France. These meetings resulted in the formal, written recommendation to Stockholm for Professor Slobodrian and Conzett to be Candidate to Nobel Price for 1982. I cannot disclose confidential Foreign material, but I enclose copy of my personal recommendation to the Nobel Committee.

(6) Our past DOE support. All the studies reported here were initiated with DOE support and have been conducted with DOE support ever since. In fact, the studies were initiated with grant ER-78-S-02-4742.A000 (1978 while I was at Harvard), and where continued in the subsequent years with grants AS02-78ER-4742 (1979 also while at Harvard), DE-AC02-80ER10651 (1980) and DE-AC02-80ER10651.A001 (1981).

The utmost dominant reason for these studies is the experimental resolution of the basic laws under strong interactions. The results are too numerous to be indicated here. In fact I have personally written a number of research monograph with Springer-Verlag, and too numerous articles to remember without consulting my file. In addition, I have supported as many researchers in the problem of the basic laws as possible.

At the inauguration of the Orléans Conference, soon after the Opening by the President of the University, a formal acknowledgment to DOE was pronounced, and resulted in a lasting ovation.

(7) Our pending research application. We believe that the resolution of the problem of the basic laws for the strong interactions one way or another is unprochrastinable. Permit me the liberty of being as candid as possible on this point. In fact, if the Senate and the general public at large become aware of the existence and administrative magnitude of the problem, the implications can be substantial. After all, you do not have to be a physicist to see that the test of the basic laws is more, much more important than routine work.

The only debatable issue, is how to continue the research. At this point your Office must be realistic. At our Institute we have assembled ALL the true experts in the problem I am referring to a coordinated and organized group of mathematicians, theoreticians, and experimentalists in well over 15 different Countries. By comparison, other Institutions, such as MIT, SLAC, FERMILAB, BNL, etc. have remained substantially behind, because of their rather stubborn resistance to a type of research only superficially against their academic-financial interests. At any rate, the technical literature is now well over 5,000 published pages, and it will take years for other physicists to digest it and become true experts (selfproclaimed/corrupt/experts-referees can be made in minutes).

The most effective way to continue the research is via its originators, that is, our new Institute for Basic Research.

At this point, permit me the liberty of conveying another aspect, as clearly as possible. Until now, we have initiated, conducted, and la ched this new scientific current WITH TRULY MINIMAL DOE SUPPORT. In fact, our average yearly support has been of about $ 60K (sixty thousand dollars)per years. We can certainly continue with funds of such minimal magnitude, BUT I DO NOT THINK THAT SUCH A CONTINUATION WOULD BE IN THE BEST ADMINISTRATIVE INTEREST OF DOE because it could create administrative prioritary umbalances.

For these reasons we have suggested in our application a very moderate increase of support for the specific purposes of
- funding the position of Director of Our Institute who should coordinate and initiate contacts with other DOE-supported laboratories and institutions;
- funding one research associate for the theoretical formulation of experiments; and
- funding one research associate for the experimental profile.

This program can be implemented with an ultimate minimum of $ 150K (one hundred and fifty thousand dollars). This is due to our very low overheads (some 30% of salaries and wages) which is permitted by the voluntary assistance of Institute's members and their

spouses.

We are fully aware of the difficulties of your position, and of the complexity of the administrative choices. Nevertheless, we believe that it is time to set prioritary guidelines.

In this respect, permit me to recommend, respectfully, but as warmly as possible, that administrative priority be given to truly fundamental fields of research, and secondary attention be given to fields of secondary physical relevance. This has been the golden administrative rule which has permitted throughout the history of physics the achieveme of truly fundamental advances while meeting the highest possible standards of scientifi accountability vis-a-vis with the taxpayer. I beg you not you change it now in the interest of the Country as well as of DOE.

More particularly, I urge you to excersice extreme caution before committing large public funds to expensive experiments on minute technical details based on the belief of the validity of conventional laws. The doubts on the validity of these laws are now well established knowledge throughtout the world. Their ignorance, rather than avoiding responsibilities, can only multiply them.

If I or any member of our group can be of assistance to you and your Office in identify some technical implications of the problem of the basic laws in the proposals under consideration for funding, please let us know.

Very Truly Yours

Ruggero Maria Santilli
President
RMS-mlw

cc.: Honorable JAMES B. EDWARDS, Secretary, DOE
Dr. A.W. PRIVELPIECE, Director, Office of Energy Research, DOE
Dr. E.E.KINTNER, Ass. Director, Office of Energy Research, DOE
Dr. B. HILDEBRAND, Division of High Energy Physics, DOE
Dr. R. THEWS, Division of High Energy Physics, DOE
and
The Board of Governors, IBR

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

February 22, 1982

Honorable JAMES B. EDWARDS
Secretary
DEPARTMENT OF ENERGY
WASHINGTIN, D.C. 20585

Honorable Mister Secretary,

Permit me the liberty of recommending, most recpectfully, that you consider:

(1) your personal supervision of the decision regarding our case as per enclosed formal letter to Dr. Wallenmeyer of same date;

(2) your personal supervision in the possible funding of large, expensive experiments on strong interactions under the current conditions of mere belief of the basic physical laws, with consequential realistic possibility of invalidation of the totality of funding or most of it;

(3) the advisability of informing President Reagan personally in case of any questionable admininistrative priorities or funding.

On my part, I have not, and I do not contemplate informing President Reagan of the situation without prior consultation with you. In the final analysis, I am confident of the selfcorrecting administrative capabilities at DOE which deserve all necessary time and consideration.

Nevertheless, I suggest remaining on the alert. The scientific scene is in a very delicate and tense moment. We are facing possible real questions of scientific accountability investing billions of taxpayer's money, on one side, while, on the other side, there could exist at a number of academic institutions and laboratories a real disrespect of national interests for genuine technological advancements because of truly excessive and untolerable personal greed of physicists in administrative control. A situation of this type is indeed explosive and should be monitored in my view.

Speaking on personal grounds, you can rest assured that I am seriously committed to the prioritary general interests of the Country even in disrespect of my personal interests, if necessary. You should therefore expect from me nothing but a behaviour as responsible as possible. The understanding, however, is that I see an equal committment at DOE.

Best Personal Regards

Ruggero Maria Santilli
President
MRS-mlw

cc. The Board of Governors, IBR

Department of Energy
Washington, D.C. 20545

FEB 24 1982

Professor Ruggero Maria Santilli, President
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This will acknowledge receipt of your letter of February 11 concerning the status of your proposal "Theoretical Studies on Lie-Admissible Formulations". External review of this proposal has been completed, and it is currently undergoing an internal review in preparation for a final decision. The comments in your letter will be considered in our review, and you can expect to be notified of a decision shortly.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

Department of Energy
Washington, D.C. 20545

MAR 2 1982

Professor Ruggero Maria Santilli, President
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This will acknowledge receipt of your letter of February 22 with enclosed material relative to your proposal, "Theoretical Studies on Lie-Admissible Formulations". This material will be included as part of your proposal and considered in our review process.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

Department of Energy
Washington, D.C. 20545

MAR 24 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Reference is made to the proposal submitted by The Institute for Basic Research for support of a research program entitled "Theoretical Studies on Lie-Admissible Formulations" to be conducted under your direction.

This proposal has undergone a series of external peer reviews as well as an internal DOE High Energy Physics review. The review included considerations of the recent information supplied in your letters to me of February 11 and 22, 1982.

We have carefully considered this proposal and supplementary material in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

Department of Energy
Washington, D.C. 20585

MAR 1 1 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This will acknowledge receipt of your letters of February 11 and 22, 1982. Your proposal, "Theoretical Studies of Lie-Admissible Formulations" is currently undergoing a technical review in the Division of High Energy Physics. I am informed that the additional information you forwarded via your letters of February 11 and February 22, 1982, to Dr. Wallenmeyer is also being considered prior to a final decision. The Division of High Energy Physics will inform you of a decision shortly.

Sincerely,

Alvin W. Trivelpiece
Director, Office of
Energy Research

Department of Energy
Washington, D.C. 20545

MAR 24 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

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Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

March 29, 1982

Dr. WILLIAM A. WALLENMEYER
Director
Division of High Energy Physics
DEPARTMENT OF ENERGY
WASHINGTON D.C. 20545

RE: Applications entitled:
"Theoretical Studies on Lie-admissible Algebras"
under IBR administration

Dear Dr. Wallenmeyer,

Please be reassured that I accept with grace and respect the final negative decision by your office on our application. This is the result also of the several conversations with you and Dr. B. HILDEBRAND, and of the alternative interim solution that has emerged.

Thank you for your courtesy, consideration, and time.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-mlw
cc.: HonorableJAMES B. EDWARD, Secretary
Drs. A.W.PRIVELPIECE, E.E.KINTNER, B. HILDEBRAND, and R. THEWS, DOE
and
The Board of Governors, IBR

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

March 29, 1982

Dr. WILLIAM A. WALLENMEYER
Director
Division of High Energy Physics
DEPARTMENT OF ENERGY
WASHINGTON D.C. 20545

RE: Applications entitled:
"Theoretical Studies on Lie-admissible Algebras"
under IBR administration

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Thank you for your courtesy, consideration, and time.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-mlw
cc.: HonorableJAMES B. EDWARD, Secretary
Drs. A.W.PRIVELPIECE, E.E.KINTNER, B. HILDEBRAND, and R. THEWS, DOE
and
The Board of Governors, IBR

Department of Energy
Washington, D.C. 20545

APR 22 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

As per your request, enclosed are comments from reviewers of your proposal "Theoretical Studies on Lie-Admissible Formulations."

Sincerely,

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

Enclosures
As stated

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 1

Santilli is very knowledgeable on various modern mathematical methods. He is very methodological in search for literatures. He is certainly very competent in what he is working on.

It is rather difficult to state the significance and the merit of the proposal in terms of standard criterias, since the subject matter is not that of a main stream of current research activity in high energy physics. It may be even likely that many high energy physicists will not regard it as a branch of high energy physics. However, a nagging suspicion at least on my part (which may not be shared by many others) is that some parts of ideas, especially that of the use of Lie-admissible algebras instead of the conventional Lie-algebras may turn out to be very relevant to high energy physics some day soon. Also, I should mention that I do not agree with many views expressed by Santilli especially on unreality of quarks as well as possible violation of Lorentz covariance in strong interactions although he may turn out to be right. Nevertheless, the idea of Lie-admissible algebras is novel and intriguing, which may blossom in time to come. Together with many Workshops on the problem organized by Santilli in recent years, he is greatly contributing to development of this branch of activities which are very much ignored nearly by the majority of physicists. In this sense, the proposal has a merit and significance.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 2

I have not really followed the earlier work of the proposer on his Lie-Admissible approach, and so cannot pass on its intrinsic competence. My feelings are that this avenue is rather formal in nature and not in the mainstream of current research. With any one unorthodox idea, there is little likelihood that it will succeed and it is difficult for me to be sympathetic to a proposal of this magnitude when so much worthwhile research is going unfunded. I also believe that the experimental reasons advanced against the body of "conventional" theory leave something to be desired as well. Finally, the conferences to be sponsored, like past ones, seem devoted to exchange of ideas among the "faithful" or to try to extract support from the statements of attending "outsiders".

In summary, I feel that this proposal should not be funded on both scientific and fiscal grounds.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 3

The applicant has developed a branch of classical mechanics. He did this work first on his own, and then in collaboration with other mathematicians. I have the impression that his derivations are sound. The work demands an extensive knowledge of mathematical physics.

I believe this work could be of some practical use, although the physical research projects he suggests to pursue do not appear to me very promising on the whole. His notions like supraluminal velocities appearing in conjunction with strong interactions are not defined very carefully and are too immature to be useful.

I consider that this proposal would merit some support because something useful may come out, but I do not believe it will be very important for physics and would not give it a high priority.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 4

His typical paper surrounds with clouds of irrelevant mathematics an argument either vacuous or based on elementary errors. He has contributed nothing to the progress of science in the past and I firmly believe he will contribute nothing in the future.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 5

There are two essentially distinct aspects to this proposal. The first is a program of research in generalized mechanics, and is mathematical in nature. The second is an attempt to tie the proponent's mathematical ideas to possible new phenomena in particle physics and to possible implications for applications such as nuclear fusion research. I recommend strongly against further DOE support of either line of work. The research in generalized mechanics may be useful, but if supported by a U.S. agency it should be only after peer review by, and competition with, mathematicians doing work in related areas. I do not think that the Division of High Energy Physics of the DOE can give this aspect of Santilli's work the kind of scrutiny it requires. As to Santilli's attempt to test the fundamental laws, doing this in a useful way requires both experience with phenomenology and the ability to be careful and objective in handling experimental data. I do not think that Santilli has the needed experience or objectivity to carry out such studies. His comments on water waves on p. 6 suggest that he is not familiar with the important distinction between phenomenological Lagrangians (which usually are nonlocal) and fundamental Lagrangians, which is basic to much of what is being done now in high energy theory. His attribution on p. 24 of significance to the shift in the Rauch data from 716.8 ± 3.8° to 715.87 ± 3.8° is reading a great deal into a change of 1/4 of a standard deviation in a one standard deviation "effect!" Is he aware of how many two or three standard deviation "discrepancies" have come, and gone, in the process of refining the tests of QED? His claim on page 19 that his work, and the small effects he would like to find, are relevant for fusion research are not substantiated either in the proposal or the cited article. They seem highly implausible, given the availability of experimental data on nuclear properties, and especially because the parameters needed for controlled fusion are typically measured on a logarithmic scale, and are not sensitive to tiny effects which can only be measured (if at all) in precision experiments.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 6

I have read the proposal by R.M. Santilli entitled "Theoretical Studies on Lie-Admissible Formulations." In my opinion it is a non-proposal. Almost all of it is a Madison-avenue like self advertisement. After that comes about half a page in which the principal investigator asks for support to continue doing what he has been doing. I find nothing explicit in the proposal to evaluate.

In the self advertisement portion of the proposal, Dr. Santilli describes possible applications of Lie admissible systems to high energy physics. I think such applications are highly unlikely, but perhaps physicists would be in a better position to judge its applicability.

Because the document says "support me to continue my research," I went back and read some of the mathematical portions of earlier papers reporting on research supported previously by D.O.E. I found nothing exciting. A Lie admissible system is a non-associative algebra in which Lie bracket gives a Lie Algebra, i.e., the Jacobi identity holds. (The idea goes back to A.A. Albert.) Many such algebras can be obtained from associative algebras in specified ways. Dr. Santilli and his associates study such algebras. In my opinion, nothing important has emerged, even in regards to classical symplectic manifolds, Hamiltonians, etc.

If this were a mathematics proposal, I would give it a low rating. The merit of the proposal can only be its possible application to physics. Since that is implausible to me, I rate this proposal very low. It's a high risk venture with minimal payoff, especially not worth funding in these days of stringent funding.

PART XXIII:

REJECTION OF A SECOND, PRIMARY, GROUP PROPOSAL OF THE I.B.R. BY THE NATIONAL SCIENCE FOUNDATION AND THE DEPARTMENT OF ENERGY

Research Grant Proposal

submitted to the

U. S. DEPARTMENT OF ENERGY

by

The Board of Governors of
THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street
Cambridge, Massachusetts 02138
Tel. (617) 864 9859

entitled

STUDIES ON HADRONIC MECHANICS

Proposed Starting Date	Proposed Duration	Amount Requested
March 15, 1983	5 years	$ 835,250

ENDORSEMENTS

R. M. SANTILLI
Principal Investigator and President
The Institute for Basic Research
Cambridge, Massachusetts 02138
Soc. Sec. No. 032 46 3855
tel. (617) 864 9859

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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ABSTRACT

The studies proposed in this application constitute a new phase of research conducted since 1978 under DOE support by a coordinated group of mathematicians, theoreticians, and experimentalists. A main objective was the identification of methods for the treatment of extended particles with action-at-a-distance/potential as well as contact/non-potential forces.

The studies were initiated at Harvard University (1978—1980), were continued thereafter at the IBR (1980—1982), and resulted into: (a) the development of a generalization of Lie's theory based on the so—called isotopies and genotopies of the envelope, which is structurally more general than the graded/supersymmetric extensions; (b) the construction of the so-called Birkhoffian generalization of the Hamiltonian mechanics for the treatment of all local, analytic, nonpotential systems; and (c) the identification of the rudiments of a conceivable generalization of atomic mechanics (the ordinary quantum mechanics) specifically conceived for strong interactions and called **hadronic mechanics**. The new mechanics is physically motivated by the representation of hadrons as extended objects, and mathematically suggested by the operator image of the classical, Birkhoffian realization of the generalized Lie's theory.

This proposal recommends the conduction over a five year period of a comprehensive research on the hadronic mechanics by a coordinated group of theoreticians, under the assistance of experimentalists and mathematicians, with particular reference to the following aspects.

(I) **FOUNDATIONAL STUDIES**, including: finalization of the isotopies and genotopies of the Hilbert space, quantum postulates, and basic principles of the hadronic mechanics; finalization of the quantization procedures from the Birkhoffian to the hadronic mechanics; finalization of the hadronic generalization of the atomic (unitary and antiunitary) symmetries; identification of the hadronic image of the isotopic generalization of Galilei's relativity achieved during the first phase of studies for classical closed systems with nonpotential internal forces;etc.

(II) **APPLICATIONS TO EXPERIMENTAL DATA**, including: data elaboration of the following experiments in nuclear physics (a) established breaking of the atomic parity; (b) apparent breaking of the atomic time—symmetry; and (c) apparent breaking of the atomic rotational symmetry (deformation of extended charge distributions under contact interactions); interpretation of the variation of (a), (b) and (c) from nuclei to nuclei; proof of the validity for breakings (a), (b), and (c) of the covering, hadronic, isotopic unitary and antiunitary symmetries; proof of the compatibility of these nuclear settings with gauge theories on leptonic decays; etc.

(III) **APPLICATIONS TO QUARK THEORIES**, including: proof that spontaneous symmetry breakdown is a particular case of the isotopic generalization of Hilbert spaces; application of the hadronic mechanics to the construction of quarks as composite systems of more elementary particles (representations on a bimodular Hilbert space); use of conventional atomic mechanics for the exterior treatment and of hadronic mechanics for the interior one to achieve a strict confinement of quarks (identically null probability of tunnel effects); etc.

As it occurred for the first phase of studies (1978—1982), the proposed second phase (1983—1988) is expected to imply the organization of a number of conferences, and to result in the publication of a number of papers, conference proceedings, and research monographs.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

September 28, 1982

Dr. WILLIAM A. WALLENMEYER
Director (ER–22)
Division of High Energy Physics
U.S.Department of Energy
19901 Germantown Road
GERMANTOWN, Maryland 20874

FEDERAL EXPRESS

Dear Dr. Wallenmeyer,

We hereby submit for consideration by your Division of the DOE the original, duly signed copy of a research grant proposal entitled

STUDIES ON HADRONIC MECHANICS.

Eight additional copies of the proposal have been separately mailed to you.

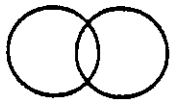
We shall remain at your disposal during the consideration of the proposal for any additional assistance you may need.

Very Truly Yours

Ruggero Maria Santilli
Principal Investigator and
President

RMS–mlw

cc.: Drs. B. HILDEBRAND and R. THEWS, DOE



I. B. I.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

September 25, 1982

Professor S. PETER ROSEN
Program Associate
Theoretical Physics Program
Division of Physics
NATIONAL SCIENCE FOUNDATION
1800 G Street
WASHINGTON, D.C. 20550

FEDERAL EXPRESS

Dear Professor Rosen,

We hereby submit for consideration by your Division of the NSF the original, duly signed copy of a research grant proposal entitled

STUDIES ON HADRONIC MECHANICS.

Fourteen additional copies of the proposal have been separately mailed to you.

We shall remain at your disposal during the consideration of the proposal for any assistance you may need.

Very Truly Yours

Ruggero Maria Santilli
Principal Investigator
and President

RMS—mlw

Department of Energy
Washington, D.C. 20545

OCT 6 1982

Professor Ruggero Maria Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

The research proposal entitled "Studies on Hadronic Mechanics" submitted on your behalf by the Institute for Basic Research has been received in the Division of High Energy Physics

This proposal is now under review and as soon as a decision with respect to support can be reached you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him on (301) 353-4829.

Budget pages 60e, 60g, 60i, and 60m do not add correctly and thus the 5 year amount requested also requires correction. Please send this office corrected budget pages.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: [redacted]

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 1, 1982

Dr. W. A. WALLENMEYER, ER–22
Director
Division of High Energy Physics
U. S. DEPARTMENT OF ENERGY GTN
WASHINGTON, D.C. 20545

RE: Applications entitled
STUDIES ON HADRONIC MECHANICS
Principal Investigator: R. M. Santilli

Dear Dr. Wallenmeyer,

I am contacting you to encourage the most comprehensive possible review of the proposal. It is my understanding that the proposal has already been sent to referees. Nevertheless, permit me to provide you with an additional list of experts in the field who are fully knowledgeable of my research. Additional copies of the proposal are at your disposal on request. At your discretion, the referees selected by your office should feel free to contact some of the experts in the field indicated in the enclosed list, in case technical advice on specific aspects is needed. In this letter, I provide you with some general information on the diversification of refereeing which appears to be needed for a serious review of the proposal.

ADDITIONAL MATERIAL. I have separately mailed to you:

- copy of the galleys of my second volume with Springer–Verlag entitled *Birkhoffian Generalization of Hamiltonian Mechanics*; and
- copy of the four volumes of proceedings of the Orléans International Conference on Nonpotential Interactions.

I would appreciate the courtesy of considering this material an integral part of the proposal. In fact, the volume on Birkhoffian mechanics constitutes the classical foundations of the proposal. It is evident that no mature judgment can be reached without at least some knowledge of this rather vast new field. Similarly, the proceedings of the Orléans Conference deal directly with the topic of the proposal and present the current state of the art in the experimental, statistical, and particle profiles of the project. Again, some (even minimal) knowledge of these proceedings is essential in order to avoid the venturing of personal feelings by the referees, rather

than technical reviews.

EXPERIMENTAL ASPECTS. I have separately listed as referees three leaders of experimental teams who are all familiar with the basic tests of the central ideas of the hadronic mechanics, as well as of the theoretical studies by our group. It appears recommendable that these experimentalists be consulted prior to reaching a final judgment on the proposal.

STATISTICAL ASPECTS. I have presented an additional list of statisticians, all experts in the relationship between irreversibility and nonpotentiality. Even though the proposal does not deal directly with statistical mechanics, the consultation of these referees appears recommendable. In fact, a primary motivation of the construction of the hadronic mechanics is to achieve compatibility and unity of thought between the experimentally established irreversibility and noncanonicity of the macroscopic physical reality, and particle mechanics. For a mature refereeing of the proposal, it is important that you consult statisticians who have an established record of scientific research in the problematic aspects underlying the unfulfilled dream of reconciliation of the current Lagrangian—Hamiltonian models in high energy physics, and the real world in our environment.

MATHEMATICAL ASPECTS. It is evident that maturity of judgment also calls for an inspection of the mathematical structure of hadronic mechanics, if nothing else, because of its novelty (isotopy of Hilbert space). A list of senior mathematicians all experts in the field has been enclosed.

PARTICLE ASPECTS. This is, of course, the central part of the proposal. A list of experts with an established record of contributions in the field is enclosed. Permit me to stress that, in our view, a referee is qualified for the physical review of the proposal if and only if he/she has a scientific record of PAPERS DEALING SPECIFICALLY WITH NONLAGRANGIAN—NONHAMILTONIAN INTERACTIONS. Otherwise, it would be the same as sending a proposal, say, on quarks, to referees without any record whatsoever of active research on quarks.

As a specific example, Professor S. Okubo has a rather extensive record of publications in the mathematical studies of Lie—admissible algebras. However, he has not published one single paper in their physical applications to contact/nonpotential interactions, nor it appears that he is knowledgeable of this vast new field. As a result, Professor Okubo would qualify as an excellent referee of the mathematical review of the proposal BUT NOT FOR ITS PHYSICAL PART.

I leave it to your judgment, of course, to consult referees without an established technical record in the field of the proposal. However, the understanding is that they may express, at best, personal feelings on the proposal, rather than professional reviews.

REJECTION OF REFEREE REPORTS. In the past, I have at times received negative referees' reports on research grant applications or on research papers without any technical contents whatsoever, or even with offensive language. Reports of this

type are generally more damaging to the institution that accepts them, than to the refereed person.

The topic of the proposal deals with a truly innovative project, the possible construction of a generalization of quantum mechanics. As such, the project may stimulate all sort of emotional attitudes, which, in turn, may result in reports potentially damaging to your office.

Permit me the liberty of recommending, most respectfully, that reports are inspected for scientific contents and value exactly as it is the case for the proposals, and that reports which are questionable on grounds of scientific ethics be rejected and returned to the referee.

Best Personal Regards,

Ruggero Maria Santilli

cc. Drs. B. Hildebrand and R. Thews, DOE

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

December 23, 1982

Dr. M. BARDON
Director
Division of Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Bardon,

I would appreciate the courtesy of the consideration whether a decision on our main proposal entitled

STUDIES ON HADRONIC MECHANICS, NSF Ref. No. PHY–8300195

can be reached in early January 1983. However, if this is not possible for any reason, please be reassured of our full understanding.

The reasons are due to our short term forecasts. In fact, our current financial support (a $ 40K contract with DOE) will be exhausted by March 1983. Lacking a decision by early January 1983, we will be forced to seek alternative financing of our research programs beginning from the second half of January, evidently, in order to have sufficient time prior to the deadline of March 1983. In particular, we would like to do our best to avoid the search of alternative forms of financing because of understandable, potential conflicts with an ongoing NSF consideration.

Thus, a decision by early January, whether positive or negative, would be ideal on our part, although, again, I do not know whether it is realizable from your profile. All our NSF applications have been submitted jointly to DOE, and a similar letter has been written to Dr. Wallenmeyer, Director of the DOE High Energy Physics Division.

I would like to take the opportunity of bringing to your attention the possibility that your Division considers an institutional support of the I.B.R., which would include our main proposal indicated above, as well as others already submitted or in the process of being submitted.

I assume you are aware of the fact that the research programs for which the I.B.R. was founded complement rather nicely those of other NSF institutionally supported entities, such as the Institute of Theoretical Physics at Santa Barbara. In fact, the research conducted under existing NSF support is based on the assumption of the exact validity of Einstein's special relativity for strong interactions. Our experimentalists, theoreticians, and mathematicians instead, are studying the possible need of suitable

corrections due to the extended character of hadrons, and the use of recently identified generalizations of Lie's theory beyond grading—supersymmetric extensions, called of Lie—isotopic and of Lie—admissible type.

The literature of this dichotomy is now rather vast, and estimated in the excess of 10,000 pages of printed research. To put it in a nutshell, you should first recall that conventional space—time symmetries, say, rotations, CANNOT be broken for a point—like charge, no matter what interactions you use. Point—like structures, however, are only a figment of academic imagination. In fact, hadrons have an extended size of the order of the range of the strong interactions. Once you abandon points and pass to the consideration of hadrons as extended objects, the following possibilities emerge, here expressed in nontechnical terms.

I. The extended charge distributions of hadrons are perfectly rigid, in which case conventional space—time symmetries continue to be exact; or, a bit more realistically.

II. The extended charge distributions of hadrons experience (small) deformations depending on the impact, interactions, and mutual penetration with other hadrons, in which case suitable correction to Einstein's special relativity must be theoretically identified and experimentally tested.

ALL durrent NSF support is along line I. The I.B.R. has been founded to explore alternative II. On administrative grounds we favor, of course, the continuation of primary support along line I; however, we believe essential for scientific accountability vis—a—vis the taxpayer, that NSF initiates funding also of alternative II, where at our institute or elsewhere.

On pure scientific grounds, the possibility of turning the I.B.R. into an NSF supported institution of the type existing at Santa Barbara would be ideal. In fact, this could maximize the interplay between the two alternatives in the sole interest of the pursuit of novel physical knowledge. The novelty, location, and flexibility of our Institute renders it particularly attractive. For instance, we still have open the position of Director, which could be filled by a scientist with sufficient qualifications to facilitate the flow of mutual exchanges with other NSF institutions.

Nevertheless, we beg you not to consider this latter alternative as necessary, and any other alternative considered appropriate by your office would be gratefully accepted by us.

However, permit me to express most respectfully but candidly, our fear that a continuation of the use of large public funds on the assumption of the exact validity of Einstein's special relativity for strong interactions, without a joint support for broader, potentially fundamental advances, may result to be excessively risky for the orderly condition of our community. At any rate, we are firmly convinced that the search of a suitable generalization specifically taylored for particles and interactions which were unknown at the inception of the special relativity, is necessary on scientific, economic, and military grounds.

I would like to take this opportunity to express to all of you at NSF our best wishes

for a Happy and Prosperous New Year.

Very truly yours,

Ruggero Maria Santilli
President and
Professor of Theoretical Physics

RMS/mlw

cc: Dr. EDWARD E. KNAPP, Director, NSF

Enclosures: Copy of title and abstract of proposals.

NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

January 10, 1983

Dr. R. M. Santilli,
President and Professor
Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of 23 December 1982 concerning your proposal PHY83-00195.

I understand that this proposal is presently being reviewed, but the process has not yet been completed. I expect that it will be possible to convey a decision to you by the end of January or soon after.

Sincerely yours,

Marcel Bardon
Director, Division of Physics

NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

MAR _ 3 1983

Dr. Ruggero M. Santilli
Division of Physics
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

I regret to inform you that the National Science Foundation is unable to support your proposal entitled "Studies on Hadronic Mechanics," PHY83-00195.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

As part of a Foundation effort to ensure that all principal investigators better understand the decisions made on their proposals, we are including copies of the reviews received (with identifying information removed).

Sincerely yours,

Marcel Bardon

Marcel Bardon
Director, Division of Physics

Enclosures

FOUNDATION — PROPOSAL EVALUATION FORM — NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY-8300195	INST FOR BASIC RESEARCH	11/25/82

PRINCIPAL INVESTIGATOR	NSF PROGRAM
RUGGERO M. SANTILLI	THEORETICAL PHYSICS

STUDIES ON HADRONIC MECHANICS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

This proposal is a continuation of the authors' project that has been going on for some time. It is difficult to make out what it really is, but basically it seems to be concerned with various nonstandard mathematical structures of dynamics that may be relevant to physics. In principle, this is not an unreasonable enterprise. But this very verbose proposal seems rather hollow inside. I fail to see any results that are remotely pursuasive or inspiring to the physicists at large. The author quotes one experimental paper on time reversal violation as a support for his ideas, but that paper is now discredited.* The merit of this proposal is extremely dubious or at least cast in serious doubt.

*Hardekopf et al., Phys. Rev. C25, 1090 (1982).

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD X FAIR __ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

Review A

NATIONAL SCIENCE FOUNDATION — PROPOSAL EVALUATION FORM — 1B — NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY-8300195	INST FOR BASIC RESEARCH	11/25/82
PRINCIPAL INVESTIGATOR	NSF PROGRAM	
RUGGERO M. SANTILLI	THEORETICAL PHYSICS	

STUDIES ON HADRONIC MECHANICS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

The proposal is a continuation of Santilli's line of works in the past years. He claims that he and his collaborators have laied a mathematical foundation on which physics is ready to be built . I do not agree with him. In the past five years, he and his followers have produced no solid achievement worth mentioning. None of their papers, except for one, were published in regular refereed journals where most of major mathematical and physical works have been published. I do not count the Hadronic Journal as one of them; Santilli himself is an editor and because of its low quality, many of institutions including ours stopped subscription sometime ago. The only paper of theirs which managed to get into a regular journal was the one by Ktorides, Myung, and Santilli (reference 18), which was held back for more than a year before acceptance.

Since Santilli asserts that a referee should not quarrel with his works on the basis of the "rudimentary outline presented in the proposal", I will argue in general terms. His words are often quite alien to theoretical physicists, even when he speaks of physics. As long as the part that I can understand is concerned, works of Santilli are trivial, wrong, or no more than presentations of frameworks that he wishes to work. I consulted with a few of my colleagues in our Mathematics Department. Some of them laughed at, but some other kindly took some time to look into. Their reactions to the mathematics part are roughly the same as my reaction to the physics part.

If anybody makes a proposal for a research contract, one has the obligation to present his (or her) works in the past and future in a language common to a substantial segment of the physics community. Aside from that, I do not consider that Santilli has achieved a progress that is worth continued support by NSF.

I recognize only two names of theorists among those quoted by Santilli. They are Okubo and Biedenharn. The latter declined joining Santilli according to a copy of the letter. Others have practically no track record in physcis research, to my knowledge. I do not believe that anything will come out of Santill's collaboration with them.

Santilli's institution seems to be his one-man institute. I have little knowledge of its quality since it has no history.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR ✓ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

Reviewer B

FOUNDATION — PROPOSAL EVALUATION FORM — 1B — NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY-8300195	INST FOR BASIC RESEARCH	11/25/82
PRINCIPAL INVESTIGATOR	NSF PROGRAM	
RUGGERO M. SANTILLI	THEORETICAL PHYSICS	PR

STUDIES ON HADRONIC MECHANICS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability).

Often proposals such as this on an unconventional topic by an investigator not affiliated with a well-known institution are very difficult to review. The unfamiliarity of the topic makes a thorough technical review quite time-consuming. The absence of an established, reputable sponsoring institution eliminates the somewhat reassuring safeguard that the investigator's credentials have been seriously examined and approved by a responsible body. These difficulties are certainly present in this case.

However, as is not commonly the case, this research has been funded by the DOE for the past four years. The results of this DOE supported work appear to have been nil. It is this reviewer's opinion that the research activities of the principal investigator have had no impact on the development of theoretical physics during this period. It would be a mistake to continue funding this activity.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR X (POOR)

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

Review C

FOUNDATION NSF FORM X-3

OPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY-8300195	INST FOR BASIC RESEARCH	11/23/82
INCIPAL INVESTIGATOR	NSF PROGRAM	
RUGGERO M. SANTILLI	THEORETICAL PHYSICS	PR

STUDIES ON HADRONIC MECHANICS

omments (continue on additional sheet(s) as necessary):
uality of the proposed research (including budget & institutional capability):

In my opinion this is a poor proposal and should not be funded.

The principal investigator, R. M. Santilli, has a very poor reputation among mathematical physicists and elementary particle physicists. The papers of Santilli's which I have looked at contain a large amount of inflated language but not a single interesting or significant result. In a number of articles, for example, he discusses a generalization of quantum mechanics which he names "hadronic mechanics." In all the pages of discussion no physical application of any significance is presented, however, and no nontrivial theorem, original to Santilli, is proved.

Santilli's proposed projects 1-11 appear no more likely to yield significant results than his past work has produced. His suggestions seem more likely to lead to exercises in formalism than to real solutions to significant physical or mathematical problems. In some of these discussions Santilli shows himself to be quite ignorant of even the basic ideas of a number of areas of modern elementary particle theory.

In his reference to lattice gauge theories, in project 1, for example Santilli apparently believes that lattices have generally been suggested as real physical objects rather than, as actually is the case, as mathematical conveniences to be removed by a limiting process.

All in all I think this is a bad proposal and is not appropriate for funding.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR X POOR

verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure. 28 DIV

Review D

Department of Energy
Washington, D.C. 20545

APR 0 1 1983

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Your proposal entitled "Studies on Hadronic Mechanics" is still under active consideration for funding, and will be acted upon during the next 6-month period.

We hereby request your permission to retain the proposal for this extended period of consideration and shall notify you of our decision regarding support as soon as possible.

Sincerely,

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

cc: Vaccaro & Alkon CP, CPAS

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

June 17, 1983

Dr. EDWARD KNAPP, Director
National Science Foundation .
WASHINGTON, D.C. 20550

Dear Dr. Knapp,

I acknowledge receipt of the rejections of ALL applications submitted to NSF by senior members of our Institute since its founding. We are referring to four applications submitted to the Division of Physics and three to the Division of Mathematics. All applications were on coordinated mathematical, theoretical, and experimental studies on the apparent lack of exact character of Einstein's special relativity for strong interactions (as now indicated by a number of international Institutes, besides ourselfs, all abroad). In particular, the applications dealt with quantitative studies that extended particles such as hadrons may experience (small) deformations under sufficiently intense external fields, with the consequential, manifest breaking of their rotational symmetry. The breaking of the Lorentz symmetry is then, under these circumstances, a known technical consequence.

For your information, some of the applications were rejected on ground of vulgarly offensive language written by manifestly corrupt referees (this was the case of some of the physical applications). Others were rejected despite the fact that the majority of the reports recommended funding quite warmly (this was the case of the applications by Professors [redacted], and by Professors [redacted] submitted to the Division of Mathematics).

The rejection of all these applications, therefore, is not the issue here. The issue is given by the premises leading to the rejections, as well as by the current lack of support at NSF of the problems addresses, despite (or because of) their truly fundamental character. Also, the processing of the applications did not stop short at the rejections, but implied additional, un-necessary damage to us. This was the case of the application by Professors [redacted] who hold a joint full professorship at other Institutions. Officers of the Division of Mathematics contacted these other Institutions, without any advance consultation with us, just prior to the rejection of the application, by therefore creating evident, totally un-necessary, personal problems. Another application was submitted back in November 1982 to the Division of Mathematics for support of a Workshop to be held in early August 1983. The rejection was kept for an un-reasonably long period of time, and was finally released because of my personal pressures on both the officer and the director of the division. The delay in the release of the rejection had the evident effect of preventing us from having sufficient time to seek alternative forms of funding, some of which may be incompatible with a consideration at NSF (e.g., when the use of scandalistic means is desired or rendered inevitable). Also, one referee of the primary physical application of our Institute (a group proposal for a coordinated mathematical-theoretical-experimental effort to generalize quantum mechanics for extended, and therefore deformable particles) included in the report statements to the effect that he/she: (a) had contacted one of the advisors of the project (Professor L.C. Biedenharn of Duke University); (b) had succeeded in pressuring him to withdraw from the project; and (c) had even secured a letter to the effect of documenting the withdrawal.

As indicated to you in preceding correspondence, individual members of our Institute are considering a national campaign aimed at having the Americal Physical Society formulate and adopt a much overdue CODE OF ETHICS, as well as having the judiciary and political systems create independent means for its strict enforcement. This letter is intended to give you and your officers all the necessary prior knowledge of the possibility that the totality of the documentation regarding our research grant applications, jointly with our individualized comments, of course, might be relesed to the appropriate committes of the U.S.Senate and House of Representative, as well as to the press. In case you and/or your officers have any objection to such a release, you should let us know immediately. However, in case no objection exist (or can be raised), no acknowledgement of this letter is needed.

Very Truly Yours

Ruggero M. Santilli

RMS–mlw

cc.: Drs. R.M.Sinclair, Acting Director, Div.of Phys.
E.F.Infante, Director, Div. of Mathematics
and
The White House

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, Presiden

June 20, 1983

Dr. R. THEWS
DOE, Division of Physics

RE: Main I.B.R. Application entitled
"STUDIES ON HADRONIC MECHANICS"
A group application involving experimentalists, theoreticians and mathematici
FINAL COMMUNICATION

Dear Robert,

The existence of the hadronic generalization of quantum mechanics for extended, and therefore deformable hadrons, has been proved by mathematicians experts of the simplectic geometry. A presentation will occur at our FIRST WORKSHOP ON HADRONIC MECHANICS this coming first week of August. In fact, quantum mechanics is the simplest possible realization of simplectic quantization, while hadronic mechanics is a more general one.

This result was inevitable from the generalizations of Lie's theory worked out by our mathematicians. The best way to put it physically is by indicating the identification of a suitable generalization of Einstein's special relativity for extended, deformable hadrons, as summarized in the enclosed paper in press at LETTERE NUOVO CIMENTO.

The dual Lie-isotopic/Lie-admissible structure for exterior-closed/interior open treatments is summarized in the enclosed additional note on the Lie-admissible structure of open nuclear reactions, also in press at LETTERE NUOVO CIMENTO. In particular, plese inspect the final part of this note, because it will tell you when referees are being intentionally corrupt. Our Lie-admissible treatment is a mere reformulation of nonunitary time evolutions used in quantum mechanics for dissipative processes since its inception. The latters have an inconsistent algebra in the infinitesimal behaviour, and the former bypass this problem, resulting in a consistent one. The gaining of a consistent algebra then permits calculations, such as the generalization of the theorem of detailed balancing, that would be impossible with the old fashioned nonunitary time evolution. Statements that Lie-admissible algebras have no meaningful application in particl physics are therefore of questionable ethical nature, in my view. In fact, the applications have been there for decades.

The developments going on in the construction of hadronic mechnaics are now too numerous for me to summarize them effectively. I restrict myself to the indicatio of the achievement of the unification of all dissipative Schrodinger's equations via our Lie-admissible structure achieved by a group at the University of Patras, Greece (see enclosed paper by Jannussis et al). This includes the equations firs proposed by Radicati(Univ. of Milano) in the early 40's.

I believe that a representative of DOE should attend our FIRST WORKSHOP ON HADRONIC MECHANICS (August 2 to 6, 1983), because I will be unable to summarize the outcome for your office as I did for all other meetings. I leave, of course, the decision to you.

I sincerely hope that a decision on our main application can be reached in the near future. Any additional delay can only be detrimental to all.

Sincerely,

cc. Drs. Wallenmeyer and Hildebrand

University of [redacted]

[redacted]

September 8, 1983

Dr. B. Hildebrand ER-221
Chief
Physics Research Branch
Division of High Energy Physics
U.S. Department of Energy GTN
Washington, D.C. 20545

Dear Dr. Hildebrand:

It is my pleasure to address myself to you as one of the participants of the "First Workshop on Hadronic Mechanics" and to thank you for supporting such a worthwhile and outstanding meeting. I was very impressed by the quality of papers presented. I felt distinctively that, in participating, I have had the honour and opportunity to be with a group of highly competent, productive, and progressive people.

I hope the Institute for Basic Research will receive continued support in its endeavours to proceed with such meetings, and I look forward to taking part in future work of the Institute. I believe this Institute and its Director, Dr. R. Santilli, are giving excellent service to the U.S.A. and beyond that to the scientific community interested in Theoretical Mechanics at large.

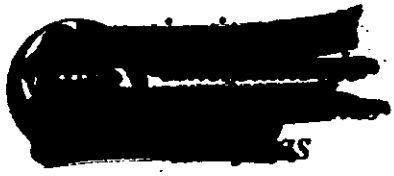
The U.S. Department of Energy must be commended on having the farsightedness of supporting an institute concerned with progressive scientific work of such high quality as I have experienced by being at the above mentioned workshop during early September of this year.

Sincerely yours,

[redacted]
Dean of Graduate Studies

HHEL/eg

bcc: Dr. R. Santilli



[redacted] August 28, 1983.

Prof. Ruggero M. Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge
Massachusetts 02138
U.S.A.

Dear Professor Santilli:

I am deeply indebted with you, with the Institute for Basic Research staff and specially with your kind family for the warm hospitality received during the Workshops.

I am also indebted for all what I learned in the two wonderful simultaneous Workshops hold at IBR. They were of the highest possible level.

Warmest regards,



Head of the Theoretical
Physics Laboratory

AJK/gbv.

[redacted]

[redacted]

[redacted] 14th September 1983

Professor R.M. SANTILLI
The Institute for Basic Research
Cambridge

Dear Friend,

It is my great pleasure to tell you how much I enjoyed our Summer Workshop at the I.B.R.. I greatly appreciated the open and frank scientific atmosphere which prevailed there, an atmosphere which is probably responsible for a deeper and deeper collaboration between mathematicians and physicists, and in turn, for the increasing number of results obtained. The presence of new colleagues joining us was for me an important indication of the I.B.R. workshops' success and I was particularly happy to note the lively exchanges developed in the group between what we must now recognize as scientists of three generations.

The development of the Institute is an important scientific achievement. Whatever the doubts of those of our colleagues in the world who argue that the necessity of our studies is not yet proven by experiment, it is a historical fact that the international center for non-conservative physics now exists and that, following the American pioneering tradition, it exists in the United States. I must confess that I regret for my part not having had the opportunity to organize it in Europe, but the existence of the I.B.R. in Cambridge is now an established fact. I am sure that all those who are helping you in the States — and, I think, the DOE for a great part — are perfectly aware of this fact and that, despite the current restrictions on expenditures, they will continue to ensure that the I.B.R. has sufficient financial support.

I am looking forward to seeing you again on the occasion of the next workshop, or possibly before.

Sincerely Yours,

[redacted]

[redacted]

Sept.1, 1983

[redacted]

To Dr. B. HILDERBRAND ER-221,
Chief,
Physics Research Branch
Division of High-Energy Physics
U.S. Dept. of Energy - GTN
Washington, D.C.-20545, USA

c/c to
Prof. R.M. Santilli

Dear Doctor Hilderbrand:

during the first ten days of August, 1983, I partecipated in the "First Workshop on Hadronic Mechanics" held at the Institute for Basic Research, Cambridge, Mass., under the direction of Prof. R. M. Santilli. Even if the I.B.R. could not provide any support this year, I succeeded in partecipating in that Workshop due to my strong interest in hearing about the most recent developments of the Lie-admissible formulations, especially as applied to elementary particles and to "hadron mechanics". I myself contributed a talk about the application of the methods of General Relativity to the description of hadron structure.

I would like to let you know that —even if I already expected to meet there outstanding physicists (so as Prof.Okubo)— I was very impressed by the high level also of the mathematicians and mathematical-physicists partecipating in the Workshop. I deem to be very profitable and promising such a collaboration among mathematicians and physicists, at the present stage of high-energy (theoretical) physics. I also enjoyed coming to know day by day —from the talks— about the very tempting theoretical framework that the Organizers of that Workshop had in mind when planning it, and that was partly unknown to me: I believe such a framework to be quite suitable for adapting quantum mechanical theories to the description of hadron structure and hadron interactions. I was also impressed by the ability of that framwork in describing —as "particular cases"— the approaches by Prof.P.Caldirola (Milan, Italy) both to dissipative systems and to leptons; the latter approach succeeds in evaluating lepton masses just by introducing a "fundamental time".

I enjoyed also the workshop atmosphere, quite cooperative and open-minded, as well as the heartly partecipation of the Organizers, particularly of Prof. R.M.Santilli, very attent and smart scientific leader.

I hope in the future to be able again to partecipate in the same series of Workshops, since I think that important results are coming out —and even more will come aout— from the research groups linked with the Institute for Basic Research.

Thank you for your attention. Yours sincerely,

[redacted]

Prof. of physics.

Department of Energy
Washington, D.C. 20545

OCT 19 1983

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Reference is made to the proposal submitted by the Institute for Basic Research for support of a research program entitled "Studies on Hadronic Mechanics" to be conducted under your direction.

We have carefully considered the proposal in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program. Due to the funding limitations which we are currently experiencing, we have found it necessary to decline support of many promising proposals such as yours.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859

November 10, 1983

Dr. WILLIAM WALLENMEYER
Director
Division of High Energy Physics
U.S.DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

I hereby withdraw from consideration by your Office our remaining applications, that is

Theoretical, Experimental and Applied Studies on a possible pulsating structure of the Coulomb Force of individual Electrons
Submitted on January 3, 1983 under the principal Investigator
Dr. [redacted]
and
Studies on the Quantization of Systems with Gauge Symmetries
Submitted on July 14, 1983 under the principalInvestigator
Dr. [redacted]

The withdrawal is evidently due to the recent declination of funding of the primary I.B.R. proposal on the development of Hadronic Mechanics by your office (as well as by N.S.F.). In fact, the declination does not permit the I.B.R. to have sufficient logistic structures to administer and properly conduct other projects at this time.

Very Truly Yours

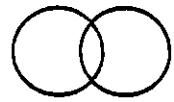
Roger Santilli
President

RMS-wls

cc.: [redacted]

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859



November 10, 1983

Drs Wallenmeyer and Hildebrand, DOE

Dear William and Bernie,

With your termination of my association to the DOE, I would like to take the opportunity to express to both of you as well as to Dr. D. PEASLEE the sentiments of my sincere appreciation and gratitude for your support during the past five years.

Everything that has been accomplished by our group during this period, including not so frequent scientific events such as the creation of new mechanics, is the result of your support, and I am sincerely grateful for it.

Under the circumstances, you should perhaps know that I am not contemplating to submit additional applications to your Office, while possible applications by other members of the I.B.R. will be discouraged, evidently, because of insufficient logistic backing. A formal letter of withdrawal of all the remaining applications is enclosed.

Nevertheless, if, sometime in the future, you foresee that my efforts can be helpful to DOE, please do not hesitate to let me know.

Wishing to you,.your families and your Division the best, I remain

Yours, Very Truly

R.Santilli

c.c,: Dr. D. Peaslee

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

January 1, 1984

Office of the Presidei

Dr. LARRY C. BIEDENHARN, jr
Department of Physics
University of Texas
AUSTIN, Texas 78712

Dear Dr. Biedenharn,

Your regrettable, apparent, cooperation with manipulatory practices during the consideration process by the U.S. National Science Foundation of the primary research grant application of our Institute, and the easily predictable series of consequences that this will and otherwise must imply, recommend that we terminate all our contacts for the foreseable future. Unless I hear from you, I shall therefore assume that, under the circumstances, you consider recommendable to resign from the Editorial Council of the Hadronic Journal, and I shall remove your name from it beginning from the first issue of Volume 7, that of January 1984. I shall also assume you agree on the advisability to terminate jointly all possible scientific and human contacts.

Very Truly Yours

Ruggero M. Santilli

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

PART XXIV:

REJECTION BY THE NATIONAL SCIENCE FOUNDATION AND THE DEPARTMENT OF ENERGY OF AN APPLICATION BY A SENIOR I.B.R. PHYSICIST

Research Grant Application

Submitted to the

DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street
Cambridge, Massachusetts 02138
tel. (617) 864-9859

entitled

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Proposed Starting Date:	Proposed Duration:	Amount Requested:
September 1, 1982	36 Months	$ 279,800

ENDORSEMENTS

[redacted]
Principal Investigator
The Institute For Basic Research
Soc. Sec. No. [redacted]
Teles. Office (617) 864-9859
[redacted]

R. M. Santilli
President
The Institute For Basic Research
Soc. Sec. No. 032-46-3855
Tele. (617) 864-9859

Accounting Firm of the Institute
Vaccaro and Alkon CP, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
ATT: Mr. R. Alkon, President
Tele. (617) 969-6630

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
ATT: Mr. J. Grassia, Senior Partner
Tele. (617) 956-1700

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APPENDIX: General Information on The Institute For Basic Research

ENCLOSURES:

G. [redacted]

G. [redacted]

G. [redacted]

G. [redacted] Phys. Letters 55A, 260 (1976)

G. [redacted]

ABSTRACT

The objective of this application is to develop a method of quantum–mechanical analysis of phase stability of pure metals, alloys, and compounds. The method will enable to calculate binding energies, enthalpies, Gibbs free energies, P–V and C–T–diagrams for a wide class of solids, both metallic and nonmetallic. The ultimate goal of the research is to develop a series of methods for theoretical (first–principle) predictions of physical properties of solids which will have direct implementation to creating new materials with given properties.

The first stage of the research will consist in developing a variational principle in the quantum–mechanical so–called Density–Functional–Formalism (DFF), which will allow to systematically determine directly electron charge–density distributions in crystalline solids, avoiding solving the corresponding Schrödinger equations for the wave functions. As an illustration of the method the calculations will be performed for some pure metals such as Titanium and Iron. The calculations of electron charge–density distributions, binding energies, enthalpies and P–V (pressure–volume) diagrams for these metals will serve as a test of the method.

The subsequent research will extend the method to the finite temperatures and two–component systems (alloys, compounds).

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 4, 1982

Dr. Thomas E. Walsh
Director
Electronic and Material Sciences
Air Force Office of Scientific Research
Bolling Air Force Base, DC 20332

Dear Dr. Walsh,

I hereby respectfully submit the research grant proposal entitled

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

under administration by our Institute, and with Principal Investigator Professor [redacted].

Permit me to indicate that Professor [redacted] has been recently appointed Associate Professor at the Division of Physics of our Institute following a truly impressive variety of recommendations from distinguished scholars. We are therefore filing this proposal with full confidence that Professor [redacted] will indeed meet all our expectations. Also, please take into consideration that we have filed this application with the minimal possible Institute overheads (a fraction of those charged by other Institutions) in order to facilitate the funding of Professor [redacted] research.

As you can see, we have made an effort to file the application in a form as informative as possible, including, besides budget and other conventional parts, the identification of the state of the art in the field, curriculum, and representative papers. Nevertheless, please keep in mind that we have avoided excessive length to facilitate review. We are therefore at your disposal for any additional information you may need. The inspection of the report by Professor [redacted] on his personal experience of the status in the U.S.S.R. of this field of research is recommended in particular.

In closing, permit me the liberty of indicating that Professor [redacted]'s current salary expires on September 1, 1982. We are fully aware of the complex and diversified iterim for the consideration of a proposal. Yet, your consideration of any possibility that might expedite the consideration of this proposal would meet with our sincere gratitude and appreciation.

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

cc: Board of Governors, IBR
[redacted]

DEPARTMENT OF THE AIR FORCE
AIR FORCE OFFICE OF SCIENTIFIC RESEARCH (AFSC)
BOLLING AIR FORCE BASE, DC 20332

JUN 1 1982

Professor [redacted]
Dept. of Physics
The Institute of Basic Research
Cambridge, MA 02138

Dear Professor [redacted]

We have completed our review of your research proposal, "VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS," assigned our Code No. 82-NE-202.

Although the proposed research is interesting, we are unable to consider sponsorship because we do not have an established or planned research program in this area. We have, therefore, not reviewed your proposal in detail. You will find enclosed all but one copy of your proposal.

We appreciate the opportunity to have considered your proposal and welcome your continued correspondence with the hope that we may be able to be of greater assistance at some future time.

Sincerely

for Clarence L. Hardnett
THOMAS E. WALSH
Director
Electronic & Material Sciences

Cy to: R. M. Santilli
President

Department of Energy
Washington, D.C. 20545

JUN 4 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Receipt is acknowledged of the proposal entitled "Variational Method of Calculating Structural Properties of Solids" with Professor [redacted] as principal investigator.

We are declining further consideration of this proposal and returning it to you at this time. This decision stems from the fact that we have completed our unsolicited proposal support actions for this fiscal year. Please be advised that this decision does not reflect adversely upon the technical merits of the proposal.

Thank you for allowing us to consider these research efforts.

Sincerely,

Louis Ianniello

Louis C. Ianniello, Director
Division of Materials Sciences
Office of Basic Energy Sciences

Enclosure

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

August 20, 1982

Dr. ERIC D. THOMPSON
Condensed Matter Theory
Division of Material Research
NATIONAL SCIENCE FOUNDATIONS
Washington, D.C. 20550

Dear Dr. Thompson,

During the period September 14, 15, 16, 1982, I shall be in Washington, and I would appreciate the possibility of visiting you.

We would like to present in more detail the part of the programs of our Institute which are pertinent for your Division. Also, we would like to know the status of the proposal by Professor [redacted] under consideration at your office, no. DMR–8212909.

In the meantime, I enclose a general presentation of our Institute.

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS/mlw

Enclosure

cc: Professor [redacted]

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

August 26, 1982

Professor Ruggero Maria Santilli
Department of Theoretical Physics
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli,

In reply to your letter of August 20, 1982, I shall have left the National Science Foundation before your visit but Dr. John Connolly will have resumed his position as program director of the Condensed Matter Theory program. I suggest that you telephone, (202-357-9737), to set up an appointment with him. Dr. Connolly has been advised of Dr. [redacted] proposal status. As of this date, Dr. [redacted] proposal is still under review.

Sincerely,

Eric D. Thompson

Eric D. Thompson
Program Director
Condensed Matter Theory Program
(202) 357-9737

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Professor [redacted]
Department of Physics
Institute for Basic Research
Cambridge, Massachusetts 02138

REF: DMR 8212909

Dear Professor [redacted]

We regret to inform you that the National Science Foundation is unable to support your proposal entitled "Variational Method of Calculating Structural Properties of Solids." Verbatim copies of the reviews are enclosed as provided for by current NSF policy.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals in the future. In that regard, the enclosed verbatim reviews may be useful to you. If you have any questions, please contact Dr. David R. Penn, Staff Associate, Condensed Matter Sciences Section, (202) 357-9737.

Sincerely yours,

Lewis H. Nosanow
Acting Division Director
Materials Research

Enclosures.

Copy to: Ruggero Maria Santilli
President

[redacted] | NSF PROGRAM
CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

[redacted] is an able, though probably not outstanding, solid-state theorist. He is well acquainted with current methods in the electronic structure of metals and has made useful contributions to pseudopotential theory. The proposed project on the stability of structures in metals and alloys is very similar to those being undertaken by many others in this country and Europe, stimulated to some extent by Miedema's empirical studies and Phillips' enthusiastic support for them.
What distinguishes this particular approach is the variational treatment of the density itself — without wavefunctions — which relies on his theorem (Eq. 8) through which the kinetic energy is directly related to the pressure. He purports to prove this rigorously, but I believe it is an approximation. The point is that changing the volume does not just scale the wavefunction; if there are potentials present (from the ions) it also deforms the wavefunction. Thus his approach should be classified with the Fermi-Thomas method as an approximate, in this case untested, approach. It is nevertheless interesting and worthy of support. It is a competing method which could turn out to be important.
It may be difficult to compare support here with more usual proposals from scientists in universities and industries. Institutional support would seem to be absent, in the traditional sense, but the funds would purchase the total commitment of the principal scientist. He apparently hopes to add a graduate student after the first year; I don't know if that is possible. It would be nice if it could be supported, at least partially.

Recent research achievements of the Principal Investigator(s):

OVERALL RATING: __ EXCELLENT __ VERY GOOD X GOOD __ FAIR __ POOR

NSF PROGRAM
CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capabili

1. The theoretical foundation of the proposal is unsound. The author expressed the "kinetic energy functional" in terms the bulk modulus and the derivatives of the poten energy by using the virial theorem. This is correct for the correct density. However the author wishes the final expression to have a variational principle and, therefore, requires the expression to hold for any trial density function. His proof, as given i Enclosure 1, is false. The scaling of the density and wave function, as given by eq. of Enclosure 1, when the volume of the system is changed, does not give the correspond density except for the homogeneous electron gas. For the usual statement of the viria theorem, this scaling of the wave function can be used because the error does not affe the first order change in energy due to the variational principle. The author cannot eq. (8) without the help of the variational principle. The rest of the proof is, ther fore, fallacious.

2. It is then proposed that the bulk modulus be expressed in terms of the density functio by means of the Feynman-Hellman theorem. Again that requires the variational principl The author's argument that the variational principle is not needed is false. His coun of factor of 1/N is simply wrong. In infinite solids or solids with periodic boundary there is an additional peril of using the Feynman-Hellman theorem. [See, for example, L. Kleinman, Phys. Rev. B1, 4189 (1970).] In sum, the author does not have a variatio principle for his expression.

3. What is billed as the "preliminary results" in sec. 3.2 is not a trial run of the pro- cedure outlined by the author of the proposal. Instead, it is a fit of an analytic ex sion for the density, eq. (18), to the result of a traditional self-consistent solutio

The resulting agreement in total energy is just a routine check of the virial theorem. The Feynman-Hellman theorem is not invoked. So these "preliminary results" do not pro that his procedure will work.

In view of the serious deficiency in the theory on which the proposed computation is based, support is not recommended.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR ✓ PO

[redacted] | NSF PROGRAM
CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

Stripped of all the verbiage what is actually being proposed for study is obtaining the electronic charge density in a solid on the basis of the density functional approach by exploiting the fact that the true electron numer density $n(\vec{r})$ minimizes the total electronic ground state energy.

By itself this is not a new idea. For example, John Smith used it to obtain the electronic charge density in the vicinity of a jellium surface (PR 181, 522 (1969)). He, however, used a simple expression for the kinetic energy functional T[n], viz. $T[n] = \frac{3}{10}(3\pi^2)^{2/3}\int n^{5/3}\, d^3r$. The new aspect of Krasko's proposal is the use of a much more elaborate form for this functional, that is obtained (implicitly) through the use of the virial theorem and the bulk modulus. If this approach is capable of yielding better results than those that can be obtained by the use of the simple functional quoted above, the added complexity will probably be justified. (Friedel oscillations of the charge density do not seem to be produced by the T[n] given above, for example.)

I can't help thinking, however, that it would be worthwhile to test the proposed approach on some simple, but not trivial system, for which results are known, e.g. the jellium surface, before tackling the more complicated muffin-tin model discussed in the proposal.

The description of other problems to be studied, on p. 11, is too sketchy for me to comment on them.

A great deal of money and a lot of time is being requested for checking out the proposed variant of the density-functional approach, largely because a 12-month salary is being sought for the principal investigator for each of three years. I think one should be able to decide whether the method will fly for a lost less money and in a much shorter period of time.

Recent research achievements of the Principal Investigator(s):

The principal investigator, who was unknown to me before I received this proposal seems to understand the density functional formalism quite well, and to have a record of productive work, both theoretical and computational.

OVERALL RATING: __ EXCELLENT __ VERY GOOD ✓ GOOD __ FAIR __ POOR

[redacted]

NSF PROGRAM
CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capabil

The aim set in the project is certainly timely, and the proposed means of achieving worth studying. However, the author is not as close to being able to avoid solving the af t ive Schrödinger equation as he seems to think, and the implementation of his program can expected to run into problems.

More specifically, the proposed way of determining the functional $F[n]$ is not convin

The claim on p.8 that ref.10 contains a proof of the Hellman-Feynman theorem under less r trictive conditions than it is commonly beleived to be valid is exaggerated. In fact, the "proof" in ref.10 heavily relies on plausibility arguments and is at any rate limited to independent-particle approximation in which the ground state is described by a Slater det minant and the electron density can be expressed in terms of occupied single-particle wav functions. To call this the "most general formulation", as is done on p.3 of ref.10, is s leading. The agreement with the results obtained by the Kohn-Sham formalism (p.11) may se impressing in relation to the total energy, but is meaningless on the scale of alloy form energies or structural energies.

To summarize: although it would be very useful to have a way of applying the density functional method without recourse to wave functions, the applicant is unlikely to achiev this goal along the prpposed line.

The above criticism concerns the first stage of the project (see §3.3 on p.11). The o line of the further stages is very vague. This is understandable with stages 2 and 3, whi concern the development of computational techniques, but cannot be accepted in connectio stage 4. The density functional theorem holds for the ground state of an electron system its application to finite temperatures involves some fundamental unresolved problems. St is an ambitious project in itself.

Finally, I find §3.4 of the proposal a little difficult to take seriously. With the ter facilities available in the Soviet Union, the Russian Menace can safely be ignored i field for quite a while. Furthermore, for practical applications the semi-empirical metho Miedema's are much more promising, and all information on those is available in the lite

Recent research achievements of the Principal Investigator(s):

The achievements of the applicant bear the mark of work done in relative isolation. He seems competent in the fundamental issues, and therefore can be expected to make sign cant contributions in the areas of stages 1 and 4. When it comes to numerical calculatio should seek cooperation with groups having more experience in sophisticated band calcula

OVERALL RATING: __ EXCELLENT __ VERY GOOD X GOOD __ FAIR __ P

I. B. R. — 891 —

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

January 3, 1983

Dr. LEWIS H. NOSANOW
Acting Division Director
Material Research
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

RE: DMR 8212909
Application by Prof. [redacted]

Dear Dr. Nosanow,

We acknowledge receipt of the concluding reports on the consideration of Professor Krasko's application by your office.

Even though your decision has been negative, we appreciate the interest you have indicated in the case, your diligence in considering all possible avenues, and your receptive attitude toward the consideration of possible new applications.

We are confident that Professor [redacted] will understand the reasons of the negative decision. On our part, we have supported him to our best in the past, and we shall continue to provide him with our best possible support, despite the limitations of our possibilities. In particular, we shall be at Processor [redacted] disposal at any time in case he wishes to submit a new application.

Have a happy and prosperous New Year.

Very Truly Yours

Ruggero Maria Santilli
President

RMS-mlw

cc. Prof. [redacted]

PART XXV:

REJECTION BY THE DEPARTMENT OF ENERGY OF AN APPLICATION BY FIVE, SENIOR, I.B.R. MATHEMATICIANS

Research Grant Proposal

submitted to the

U. S. DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street

Cambridge, Massachusetts 02138

Tel. (617) 864 9859

entitled

MATHEMATICAL STUDIES ON LIE-ADMISSIBLE ALGEBRAS

Proposed Starting Date
July 1, 1982

Proposed Duration
48 Months

Proposed Amount
$730,946

ENDORSEMENTS

[redacted]
Principal Investigator
The Institute for Basic Research
Cambridge, Massachusetts
tel. (617) 864 9859
and
Department of Mathematics
University [redacted]

[redacted]
Principal Investigator
The Institute for Basic Research
Cambridge, Massachusetts
tel. (617) 864 9859
and
Department of Mathematics
University [redacted]

[redacted]
Principal Investigator
The Institute for Basic Research
Cambridge, Massachusetts
tel. (617) 864 9859
and
Department of Mathematics
University [redacted]

[redacted]
Principal Investigator
The Institute for Basic Research
tel (617) 864 9859
and
Department of Mathematics
University [redacted]

[redacted]
Principal Investigator
The Institute for Basic Research
tel (617) 864 9859
and
Department of Mathematics
[redacted] University [redacted]

R.M. SANTILLI
President
The Institute for Basic Research
Cambridge, Massachusetts
tel. (617) 864 9859
Soc. Sec. No. 032 46 3855

Accounting Firm of the Institute
VACCARO & ALKON CP,CPAS
2120 Commonwealth Ave
Newton, Massachusetts 02166
tel. (617) 969 6630
att. Mr. R. Alkon, CPA, President

Law Firm of The Institute
WASSERMAN & SALTER
31 Milk Street
Boston, Massachusetts 02109
tel. (617) 956 1700
att. Mr. J.Grassia, Sen. Partner

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ABSTRACT

Lie-admissible algebras were introduced in 1948 by A.A. Albert. In 1967 R.M. Santilli first pointed out that Lie-admissible algebras may be more appropriate than Lie algebras for studying physical processes. Santilli refined his idea in a sequence of papers over several years. Meanwhile a few mathematicians wrote on the structure and classification of Lie-admissible algebras as a topic in pure mathematics. With the inception of the annual Workshops on Lie-Admissible Formulations in 1978, physicists and mathematicians began to meet together to discuss their interests in Lie-admissible algebras.

Since 1978 there has been growing evidence, at first theoretical but now based on experimental results, that Lie-admissible algebras are a proper mathematical tool to formulate and solve a number of physical problems. During the Fourth Workshop held in August 1981, it became clear that physics would benefit from solutions to certain mathematical problems. They include the development of a representation theory and universal envelope for Lie-admissible algebras, and classification and structure theory especially for mutation algebras. The principal investigators propose to work on these problems and other problems that the physics will suggest during the course of the Investigation.

The applications of the mathematical tools to be developed under this research project are rather promising and of diversified nature, encompassing a number of branches of physics, engineering, and applied mathematics at large. In fact, a number of recent papers have indicated that the theory of Lie-admissible algebras can be applied to: Newtonian mechanics and space mechanics (e.g. trajectory problems under drag forces); statistical mechanics and plasma physics (e.g. statistical ensembles inclusive of inelastic collisions and nonlocal nonpotential internal forces); particle physics (e.g. for the treatment of strong interactions as of nonlocal nonpotential type due to wave overlapping of particles); computer modeling and engineering (e.g. electrical circuitry and electronics with internal losses); and other fields.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street — 896 —
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 4, 1981

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
Physics Research Branch
DEPARTMENT OF ENERGY
Mail Station J-309
WASHINGTON, D.C. 20545

CERTIFIED MAIL

Dear Dr. Wallenmeyer,

I hereby respectfully submit the enclosed original of the research grant application entitled

MATHEMATICAL STUDIES ON LIE-ADMISSIBLE ALGEBRAS

under administration of our Institute and Principal Investigators: Professors [redacted]

The INITIATION DATE has been suggested at July 1, 1982; the DURATION is recommended for 48 months; and the PROPOSED AMOUNT IS $730,946.

As you can see in the proposal, the mathematical tools which are recommended for development have rather important and diversified applications in a number of disciplines, such as Newtonian and Space Mechanics, Statistical Mechanics and Plasma Physics, Particle Physics, Computer Modeling and Engineering, and other fields. The proposal, therefore, has all the elements for marking an important (if not historical) point in the development and pursuit of truly novel advancements in scientific knowledge.

Looking forward to hearing from you to finalize the material needed for the referee process, as well as for any additional assistance you may need, I remain,

Yours, Very Truly

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS/pm
encl.
cc: B. HILDEBRAND and R. THEWS, DOE

[redacted]
Principal Investigators

Department of Energy
Washington, D.C. 20545

December 16, 1981

Professor [redacted]
Professor [redacted]
Professor [redacted]
Professor [redacted]
Professor [redacted]
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Gentlemen:

Your research proposal entitled, "Mathematical Studies on Lie-Admissible Algebras," has been received.

Your proposal is now under review in the Division of High Energy Physics and as soon as a decision with respect to support can be reached, you will be advised. Dr. Robert Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him.

We appreciate your interest in submitting this proposal to the Department of Energy, and we will be pleased to give it review and consideration for suuport.

Sincerely,

John A. Wallenmeyer

William A. Wallenmeyer
Director
Division of High Energy Physics
Office of High Energy and Nuclear Physics

cc: Professor Ruggero Maria Santilli

Department of Energy
Washington, D.C. 20545

JUL 16 1982

Prof. [redacted]
Prof. [redacted]
Prof. [redacted]
Prof. [redacted]
Prof. [redacted]
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Gentlemen:

Reference is made to the proposal submitted by the Institute for Basic Research for support of a research program entitled "Mathematical Studies on Lie-Admissible Algebras," to be conducted under your direction.

We have carefully considered the proposal in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: Prof. Ruggero Maria Santilli

899

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

RE: Proposal entitled STUDIES ON LIE–ADMISSIBLE ALGEBRAS
by Professors [redacted],

Dr. W. A. Wallenmeyer, ER 22
Director
Division of High Energy Physics
U. S. Department of Energy GTN
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

Following your letter of declination of support of the above indicated proposal dated July 16, 1982, we would appreciate the courtesy of copies of all referee's reports.

Also, kindly advice us whether and when it is appropriate to resubmit the proposal, or submit a new proposal. For your information, subsequent to your declination, Professors Benkart and Osborn received support from the Division of Mathematics of NSF. Therefore, a possible resubmission/new proposal would be submitted only by Professors Myung, Oehmke, and Tomber to your office, as well as to the NSF (no other governmental agency has indicated the capability of considering a proposal in pure mathematics).

We would also appreciate any advice regarding the possible continuation of support of the WORKSHOPS ON LIE–ADMISSIBLE FORMULATIONS. The fifth Workshop was scheduled for support in the proposal by Professor Benkart et al, and it has not been rescheduled as of now. Please let us know whether the declination of support is for the entire proposal, including the Workshop, or some special provision could be made for the Workshop (the amount needed is of the order of $ 10K). Equivalently, please let us know if it is appropriate to submit a separate application by Professor Myung et al on the continuation of the Workshops. Again, the submission would be jointly to your office and to NSF.

For your information:

[1] the WORKSHOPS ON LIE–ADMISSIBLE FORMULATIONS are now restricted only to mathematicians;

[2] a separate new series of meetings, called WORKSHOPS ON HADRONIC MECHANICS, are scheduled for the continuation of the applications to experimental and to theoretical physics according to my proposal currently pending at your office under the title of "Studies on Hadronic Mechanics"; while

[3] the two workshops, in case funded, are intended to be conducted jointly here at the I.B.R., one in the morning and the other in the afternoon sessions.

This separation of mathematics and physics, while continuing the interaction, has been made recommendable by the considerable increase of participants in this new scientific current.

Finally, I would like to express my appreciation for your subsequent letter of August 2, 1982, indicating that you had informed the offices of Senators Jepsen and Levin and Congressman Dunn. In fact, our office has abstained from any contact of this type, and I believe that this has been the case also on the part of the Principal Investigators.

In closing, I would like to take the opportunity to express my appreciation for your courtesy and time, as well as my full understanding of the difficulties of the moment.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

cc: Drs. B. HILDEBRAND and R. THEWS, DOE
Drs. [redacted]

Department of Energy
Washington, D.C. 20545

AUG 2 1982

Professor Ruggero Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

This is in response to your letter of July 8 with regard to the proposal submitted by the Institute for Basic Research on behalf of Dr. [redacted], et al. As you know, we have declined that proposal, as is indicated in my letter of July 16 to [redacted] et al. We have informed Senators [redacted] and [redacted] and Congressman [redacted] of our action as you suggested.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

PART XXVI:

REJECTION

BY THE

NATIONAL SCIENCE

FOUNDATION

OF AN I.B.R.

WORKSHOP

IN MATHEMATICS

PROPOSAL TO THE NATIONAL SCIENCE FOUNDATION

FOR CONSIDERATION BY NSF ORGANIZATIONAL UNIT (Indicate the most specific unit known, i.e. program, division, etc.)	IS THIS PROPOSAL BEING SUBMITTED TO ANOTHER FEDERAL AGENCY? Yes ___ No ___ ; IF YES, LIST ACRONYM(S):

PROGRAM ANNOUNCEMENT/SOLICITATION NO.:	CLOSING DATE (IF ANY):

NAME OF SUBMITTING ORGANIZATION TO WHICH AWARD SHOULD BE MADE (INCLUDE BRANCH/CAMPUS/OTHER COMPONENTS)

THE INSTITUTE FOR BASIC RESEARCH (I.B.R.)

ADDRESS OF ORGANIZATION (INCLUDE ZIP CODE)

96 Prescott Street, Cambridge, Massachusetts 02138

TITLE OF PROPOSED PROJECT

FIFTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS

REQUESTED AMOUNT	PROPOSED DURATION	DESIRED STARTING DATE
$ 12,280.00	one week	August 2, 1983

PI/PD NAME AND SOCIAL SECURITY NO. (SSN)*	PI/PD PHONE NO.
[redacted]	(617) 864 9859

PI/PD DEPARTMENT	PI/PD ORGANIZATION
I.B.R., Division of Mathematics, Cambridge, Univ. of [redacted]pt. of Mathematics,	Ma [redacted]

ADDITIONAL PI/PD AND SSN*	ADDITIONAL PI/PD AND SSN*

ADDITIONAL PI/PD AND SSN*	ADDITIONAL PI/PD AND SSN*

FOR RENEWAL OR CONTINUING AWARD REQUEST, LIST PREVIOUS AWARD NO.:	SUBMITTING ORGANIZATION IS ___ IS NOT ___ A SMALL BUSINESS CONCERN (see CFR Title 13, Part 121 for definitions).

*Submission of social security numbers is voluntary and will not affect the organization's eligibility for an award. However, they are an integral part of the NSF information system and assist in processing the proposal. SSN solicited under NSF Act of 1950, as amended.

CHECK APPROPRIATE BOX(ES) IF THIS PROPOSAL INCLUDES ANY OF THE ITEMS LISTED BELOW:

- ☐ Animal Welfare
- ☐ Endangered Species
- ☐ Historical Sites
- ☐ Human Subjects
- ☐ Marine Mammal Protection
- ☐ Pollution Control
- ☐ National Environmental Policy Act
- ☐ Research Involving Recombinant DNA Molecules
- ☐ Proprietary and Privileged Information

PRINCIPAL INVESTIGATOR/ PROJECT DIRECTOR	AUTHORIZED ORGANIZATIONAL REP.	OTHER ENDORSEMENT (optional)
NAME [redacted]	NAME R.M.SANTILLI	NAME
SIGNATURE	SIGNATURE	SIGNATURE
TITLE Principal Investigator	TITLE President, I.B.R.	TITLE
DATE	DATE 10-26-82	DATE

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ENCLOSURES

- Table of Contents of the Proceedings of the Second Workshop on Lie–admissible Formulations (1979)
- Table of Contents of the Proceedings of the Third Workshop on Lie–admissible Formulations (1980)
- Table of Contents of the Proceedings of the First International Conference on Nonpotential Interactions and Their Lie–admissible Treatment (1982)
- M. L. TOMBER, *The history and methods of Lie–admissible algebras, II,* Hadronic J. 5, 360–430 (1982)

1. CO–ORGANIZERS:

Professor [redacted]
The Institute for Basic Research
Cambridge, Massachusetts 02138
and
Department of Mathematics
University [redacted]

Professor [redacted]
The Institute for Basic Research
Cambridge, Massachusetts 02138
and
College of Arts and Sciences
Natural Science Division
University of [redacted]

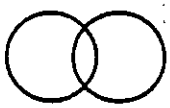
Professor [redacted]
The Institute for Basic Research
Cambridge, Massachusetts 02138
and
Department of Mathematics
[redacted] University

ABSTRACT

Lie–admissible algebras were introduced by **A. A. Albert** in 1948. In 1967, **R. M. Santilli** first pointed out that Lie–admissible algebras may be more appropriate than Lie algebras for the description of physical systems, because they permit the treatment of conventional potential interactions, as well as the nonpotential ones due to collisions among extended particles. Santilli refined his ideas in a series of papers and monographs over several years. Meanwhile, a few mathematicians (including the organizers of the proposed Workshop) conducted independent research on the structure and classification of Lie–admissible algebras as a topic in pure mathematics. The **First Workshop on Lie–admissible algebras** was held at Harvard University in August, 1978. The meeting initiated the gathering of mathematicians, theoreticians, and subsequently also experimentalists to study the Lie–admissible algebras at the pure mathematical level jointly with their applications.

The group meet again at the **Second** (1979), **Third** (1980), and **Fourth Workshop** (1981) held under financial support from the U. S. Department of Energy with a growing number of participants. By the end of 1981, the results were sufficient to warrant the organization and conduction of the **First International Conference on Nonpotential Interactions and Their Lie–admissible Treatment**, which was held on January 1982 at the Université d'Orléans, France, under joint financial support by the French and the U. S. Governments. All these studies resulted in the publication of nine volumes of proceedings for the period 1978–1982 plus a considerable number of papers.

This proposal recommends the continuation of the Workshops although specialized to pure mathematics only. The physical part is now scheduled at the **Workshops on Hadronic Mechanics**. The interplay between mathematics and physics will be kept via the conduction of the two workshops during the same period of time, e.g., by having the sessions on mathematics in the morning and those on physical applications in the afternoon. This proposal recommends minimal support for the logistic organization of the Workshops, as well as for travel expenses of ten mathematicians, all experts in Lie–admissible algebras and related fields. Additional mathematicians and theoreticians are expected to participate with their own support.



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 4, 1982

Dr. ALVIN THALER
Director
Special Programs
Division of Mathematics
NATIONAL SCIENCE FOUNDATIONS
WASHINGTON, D.C. 20550

Dear Dr. Thaler,

I hereby submit for consideration by your Division the proposal entitled

FIFTH WORKSHOP ON LIE–ADMISSIBLE FORMULATIONS

with Principal Investigator Professor [redacted], and co-investigators Professors [redacted]

The original, duly signed, proposal is enclosed, while nine copies have been separately mailed to you. In case a list of experts in the field of the proposal may be of any assistance to you, please do not hesitate to let me know.

You will note that the organization of the Workshop has been made to coincide with that of the First Workshop on Hadronic Mechanics, which treats some of the most relevant physical applications of the Lie–admissible algebras. In this way, we have separated the mathematical sessions (submitted to you) from the physical ones (under consideration by DOE and NSF). Nevertheless, we continue to promote interactions between mathematicians, from one part, and physicists from the other.

I shall occasionally keep you informed of the organization of the two Workshops. Thanking you for the courtesy of your recent phone call, I remain

Yours Very Truly

Ruggero Maria Santilli
President

RMS-mlw

encl.

June 2, 1983

Dr. E.F.INFANTE, Director,
Division of Mathematics and Computer Sciences
NATIONAL SCIENCE FOUNDATION, Washington, D.C. 20550

Dear Dr. Infante,

It appears that another link in the chain of rejections of I.B.R. applications is forthcoming. In fact, Two weeks ago I contacted by phone Dr. A. Thaler of your division to inquire about our application for funding our forthcoming FIFTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, NSF No. MCS-8303592, Principal Investigators: Professors [redacted]
Dr. Thaler informed us that he expected the declination of the proposal.

I would gratefully appreciate your consideration of the case with particular reference to the following aspects.

1) It is important that rejections be communicated as soon as possible, particularly when a negative decision has already been reached. In this case, the application was filed on November 4, 1982. It appears that, without my call, the rejection of application NSF-MCS-8303592 would have remained dormient for considerable additional time, while a considerable number of distinguished scholars from numerous countries were waiting for a decision (see enclosed list of interested participants).

Despite my call two weeks ago, the formal rejection has not yet arrived. This is highly detrimental to us because a number of alternative forms of funding the workshop are directly incompatible with a formal consideration process at NSF and, as such, they cannot be initiated during such a consideration.

The net result is that considerable delays in the communication of the rejection jeopardize substantially the possibilities for alternative fundings. We are therefore going back to the old basic question I brought to your attention: that NSF at times does not stop short at the rejection of applications, but keeps going with actions that produce additional unnecessary damage.

It is my unpleasant duty to bring these issues to your personal attention. In fact, their knowledge is a necessary pre-requisite for the improvement of the relationship between NSF and the scientific community.

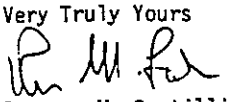
2) Please feel free to contact any member of the enclosed list. However, permit me to recommend that you do not communicate the list to NSF referees, particularly those in physics. In fact, NSF referees (in physics) would likely call members of the list to discourage their participation. This has been documented and it is a simple incontrovertible truth. For instance, one of the referees of our application to the physics division of NSF entitled "Studies on Hadronic Mechanics" (recently rejected) had the courage to put in the report itself statements to the effect that:
(a) he had contacted an advisor of the project, Professor L.C.Biedenharn of Duke Universi
(b) he had exercised pressures on the advisor to withdraw from the project; and
(c) he had even secured a letter from the advisor ensuring his withdrawal.

It is extremely unfortunate that the NSF division of physics did accept referee reports of this type for the decision making process involving the dispersal of public funds. In fact, the acceptance of reports of this type is much more damaging to NSF than to us.

I stressed the work "physics" because we have no record of similar occurrences in the refereeing process of the mathematics division.

Again, please accept the sentiments of my gratitude for your consideration and time.

Very Truly Yours



Ruggero M. Santilli
96 Prescott Street
Cambridge, Ma 02138

P.S. My two letters enclosed in the document for advance consultation for a group proposal/institutional support have been accepted for publication in Lettere Nuovo Cimento.

You should know that one of the letters (that on the Lie-admissible structure of open nuclear reactions) had been rejected and rejected repeatedly by the journals of the American Physical Society with truly unbelievable, and at times hysterical referee reports, totally deprived of the most minute scientific content or even a shadow of constructive criticism. The same letter was accepted in LCN in three weeks without any modification.

This, and too many other episodes, confirm the existence of a severe problem of ethics in the U.S. physics community. After all, academic dances for personal interests and straight scientific corruption have existed in the U.S.physics community since its birth, as it is the case for all human sectors in all countries. With the passing of the decades, the problem has deteriorared considerably because of numerous factors ranging from the increase of the amount of money managed by the physics community, to the total, absolute absence of any governmental or judicial control.

At this point in time, NSF is perceived as a victim of the deterioration of ethical standards. However, the acknowledgment of the existence of an ethical problem in the academic community, particularly in refereeing, should be made (at least internally) at NSF, and the necessary measures to cope should be identified and enforced. Lacking suitable action, it is easy to predict a deterioration of the way NSF is perceived by the community.

In the final analysis, the existence of a problem of ethics in physics is a rather common topic of talks these days. The lack of acknowledgement of its existence by NSF would evidently constitute a problem, and a sizable one at that.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

June 9, 1983

Dr. Ruggero M. Santilli
95 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I have just received your letter of June 2, concerning proposal MCS-8303592 entitled Fifth Workshop on Lie-Admissible Formulations with Professors H. C. Myung, R. Ohemke, A. A. Sagle and M. L. Tomber as principal investigators. I have had a brief discussion with Dr. Thaler on this matter, and I have shared your letter with him.

At this time, the review and evaluation process of this proposal has not been completed. This is the reason that you have not yet been notified of any decision on the part of the Foundation regarding this proposal.

Thank you for your letter.

Sincerely yours,

E. F. Infante
Division Director
Mathematical and Computer Sciences

cc: Dr. A. Thaler

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Mathematical and Computer Sciences
Mathematical Sciences Section

JUN 13 1983

Professor [redacted]
Department of Mathematics
University of [redacted]
[redacted]

Dear Professor [redacted]

We regret that the National Science Foundation is unable to support your proposal MCS-8303592 entitled "Fifth Workshop on Lie-Admissible Formulations."

Support for conferences and symposia is derived from research funds, and in evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: other proposals received by the Foundation in the same general field of science; the distribution among fields of science within the program of the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

Even though we are unable to support this proposal we would be pleased to consider other proposals which you might wish to submit.

Sincerely yours,

E. F. Infante
Division Director
Mathematical and Computer Sciences

Enclosures

cc: R. M. Santilli
President, Institute for Basic Research
Cambridge, Massachusetts

Alvin I. Thaler
Program Director for Special Projects
Mathematical Sciences Section

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editio

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8303592	Institute for Basic Research	3/15/83

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	Special Projects/NSS

TITLE

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Whether or not research on Lie-admissible algebras merits support of the kind requested rests on two grounds: (1) importance of the work to physics; (2) the extent and particularly the depth of the results judged as mathematics. I cannot comment on the first part except to wonder why virtually all the work is published in the somewhat obscure Hadronics Journal. Regarding the second point, there is now a considerable body of results on Lie-admissible algebras. This is respectable work produced by competent mathematicians. In reading [redacted] history of the subject I get the feeling that it has developed rather unsurprisingly, with appropriate use being made of Lie algebra theory and other aspects of (mostly nonassociative)algebra. But I don't see anything of real depth. This is in conformity with the fact that the people contributing papers on the subject (I exclude Albert, who apparently only contributed the definition in passing in a paper devoted to other matters) include some good mathematicians, but none of really high international stature.

My opinion, based on the mathematicasas mathematics, is that work in this field is worthy of support but does not have priority.

The frequency of these conferences seems high. The proposal conference seems well-organized and appropriate people have been invited as speakers (although it appears some light-weights are included).

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

TIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editions

POSAL NO. | INSTITUTION

MCS-8303592

11/24

NCIPAL INVESTIGATOR

LE

MENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEAI
TINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

[redacted]

RECEIVED DEC 9 1983

Please return to Thaler/O M S

This is the first time I have (in effect) been asked for a judgment on Santilli and the Hadronic Journal activities. Right away I am going to disclaim any competence to judge the physics in question. A couple of months ago I did ask the opinion of a physicist friend. He smiled and shrugged his shoulders. Well, that's not much by way of hard evidence. I shall turn to the mathematics per se.

The Albert school of nonassociative algebras has been perking along now for 40 years. It has attracted a sizeable number of able mathematicians. While largely an American enterprise, it has attracted adherents abroad, notably in the Soviet Union. Out of all the numerous specialties in today's mathematical scene, it is one of the most vulnerable to the charges of being isolated and lacking significance. And indeed, except for fleeting contacts with group theory, combinatorics, and algebraic geometry, it has been largely isolated. Of course, this could change tomorrow. Personally I expect that in due course new insights will arise that will shed a new light on the theory of general nonassociative algebras. In the meantime it seems to me that the study of various classes of algebras defined by identities has ourun its motivation and its examples. I set the dividing

(continued on a separate page)

ALL
IG: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

tim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable ncluding the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibi- NSF, reviewers' comments will be given maximum protection from disclosure.

line at noncommutative Jordan algebras and Malcev algebras. And that puts Lie-admissible algebras outside the pale.

The overall verdict: "good".

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8[illegible]552	Institute for Basic Research	

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	Special Projects/MSS

TITLE

Fifth Workshop on Lie-Admissible Formulations

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

This general area has not been useful or fruitful in theoretical physics, especially in particle physics.

"Mathematical aspects are more interesting. If the proposal is to be funded, it must be judged on the basis of its mathematical merits."

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PART XXVII:

REJECTION BY THE NATIONAL SCIENCE FOUNDATION OF AN I.B.R. APPLICATION BY TWO, SENIOR, MATHEMATICIANS

PROPOSAL TO THE NATIONAL SCIENCE FOUNDATION

FOR CONSIDERATION BY NSF ORGANIZATION UNIT(S) (Indicate the most specific unit known, i.e. program, division, etc.)

IS THIS PROPOSAL BEING SUBMITTED TO ANOTHER FEDERAL AGENCY? Yes ___ No ___ IF YES, LIST ACRONYM(S)

PROGRAM ANNOUNCEMENT/SOLICITATION NO.

CLOSING DATE (IF ANY):

NAME OF SUBMITTING ORGANIZATION TO WHICH AWARD SHOULD BE MADE (INCLUDE BRANCH/CAMPUS/OTHER COMPONENTS)

THE INSTITUTE FOR BASIC RESEARCH (I. .R.)

ADDRESS OF ORGANIZATION (INCLUDE ZIP CODE)

96 Prescott Street, Cambrisge, Massachusetts 02138

TITLE OF PROPOSED PROJECT

MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND H-SPACES WITH APPLICATIONS TO THE GEOMETRY OF NONPOTENTIAL DYNAMICAL SYSTEMS

REQUESTED AMOUNT	PROPOSED DURATION	DESIRED STARTING DATE
$ 467,660.00	five years	March 1982

PI/PD NAME AND SOCIAL SECURITY NO. (SSN)*	PI/PD PHONE NO.
[redacted]	(617) 864 9859 [redacted]

PI/PD DEPARTMENT	PI/PD ORGANIZATION
Division of Mathematics, I.B.R. Cambridge, Massachusetts and Department of Mathematics, University [redacted]	

ADDITIONAL PI/PD AND SSN*	ADDITIONAL PI/PD AND SSN*
[redacted] Co-Inv. [redacted] Mathematics Department, [redacted] niversity	te [redacted] ty, [redacted]
ADDITIONAL PI/PD AND SSN*	ADDITIONAL PI/PD AND SSN*

FOR RENEWAL OR CONTINUING AWARD REQUEST, LIST PREVIOUS AWARD NO.:

SUBMITTING ORGANIZATION IS ___ IS NOT ___ A SMALL BUSINESS CONCERN (see CFR Title 13, Part 121 for definitions).

*Submission of social security numbers is voluntary and will not affect the organization's eligibility for an award. However, they are an integral part of the NSF information system and assist in processing the proposal. SSN solicited under NSF Act of 1950, as amended.

CHECK APPROPRIATE BOX(ES) IF THIS PROPOSAL INCLUDES ANY OF THE ITEMS LISTED BELOW:

- ☐ Animal Welfare
- ☐ Endangered Species
- ☐ Historical Sites
- ☐ Human Subjects
- ☐ Marine Mammal Protection
- ☐ Pollution Control
- ☐ National Environmental Policy Act
- ☐ Research Involving Recombinant DNA Molecules
- ☐ Proprietary and Privileged Information

PRINCIPAL INVESTIGATOR/ PROJECT DIRECTOR	AUTHORIZED ORGANIZATIONAL REP.	OTHER ENDORSEMENT (optional)
NAME [redacted]	NAME R.M.SANTILLI Soc.Sec.No. 032 46 3855	NAME
SIGNATURE [redacted]	SIGNATURE [signature]	SIGNATURE
TITLE Principal Investigator	TITLE President, I.B.R.	TITLE
DATE 11-2-82	DATE 10-27-1982	DATE

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Biographycal notes on Principal Investigators

Reprints

— Table of Contents of

[redacted]

Academic Press New York/London (1973)

— [redacted]

— [redacted]

ABSTRACT

Recent developments have lead to the identification of several new applications of nonassociative algebras, with particular reference to Lie—admissible algebras, mutation algebras, and Cayley algebras. The applications essentially deal with the description of nonpotential systems, and range from trajectory problems under drag, to computer modeling, to neural systems, etc. These developments have been promoted by the Hadronic Journal; they have been studied at four Workshops on Lie—admissible Formulations (1978—1981) as well as at the recent First International Conference on Nonpotential Interactions and their Lie—admissible Treatment; and are now coordinated by the Institute for Basic Research.

In this proposal we recommend the generalization of the familiar concept of potential dynamical systems defined by a Lie group action into that of nonpotential dynamical systems defined via the action of analytic H—spaces and nonassociative algebras. In this way, the applications of the Lie—admissible, mutation, and Cayley algebras can be unified by using reductive algebras which are tangent algebras to H—spaces that param terize the dynamical system. This also extends the ideas of quantum dynamics and leads to (nonlinear) quadratic dynamical systems, and the utilization of nonassociative algebras for their determination. In particular, this extends the Sagle—Holmes results on the quadratic approximation for H—space multiplications, and the relationship between a Campbell—Hausdorff type formula and alternative algebras.

We then consider how these dynamical systems transform under a coordinate change and show that this leads to a new way to classify many nonassociative algebras by using a group S of coordinate changes. Thus, we may consider a direct classification of algebras by their dynamical or differential geometrical properties. For example, on a reductive space G/H we propose to classify those algebras related by the group S which induce connections on G/H having the same geodesics.

The approach permits the determination of the physics and of the differential geometry on G/H directly in terms of the algebra inducing the connection and the corresponding quadratic approximation to the dynamical system. Thus, we shall investigate new and applicable relations between H—spaces, dynamical systems, algebras and differential geometry, by having in mind specific applications to mechanics, computer sciences, and other disciplines.

I. B. 920 R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 15, 1982

Dr. HARVEY KEYNES
Program Director, Modern Analysis
Division of Mathematics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Keynes,

We hereby submit for consideration by the Division of Mathematics of NSF the research grant proposal entitled

MATHEMATICAL STUDIES ON REDUCTIVE LIE–ADMISSIBLE ALGEBRAS AND H–SPACES WITH APPLICATIONS TO THE GEOMETRY OF NONPOTENTIAL DYNAMICAL SYSTEMS

with Professor [redacted] as Principal Investigator, and Professor [redacted] as Co-Investigator.

The original, duly signed, proposal is enclosed, while nine additional samples have been mailed to you via separate parcel.

I trust in your leniency regarding the fact that its length exceeds fifteen pages. The Principal Investigators have found considerable difficulty in containing the length of the proposal to fifteen pages, owing to the novelty and diversification of the project.

I would appreciate knowing whether the consideration process takes into account the rather considerable and fast growing applications of mathematical studies on Lie–admissible algebras in particle physics, statistical mechanics, Newtonian Mechanics, and other disciplines. For this purpose, I remain at your disposal either for an outline of these applications or for the preparation of a list of physicists working in this field. Also, Professor Peter Rosen, of the Division of Physics at NSF, has a fairly complete file on this subject. I am confident you will find him very cooperative.

In addition, I would like to bring to your attention the fact that the research conducted by Professors Sagle and Holmes is very closely related and actually complementary to the studies conducted by Professors Myung, Oehmke, Tomber, Osborn, and Benkart.

Finally, I remain at your disposal for the preparation, on request, of a list of mathematician experts in Lie–admissible algebras, as well as for any additional assistance you might need.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Enclosure

cc: Professors [redacted]

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Mathematical and Computer Sciences
Mathematical Sciences Section

APR 14 1993

Professor [redacted]
Division of Mathematics
Institute For Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor [redacted]

We regret to inform you that the National Science Foundation is unable to support your proposal no. MCS-8305548 for "Mathematical Studies on Reductive Lie-Admissible Algebras and H-Spaces with Applications to the Geometry of Nonpotential Dynamical Systems."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to the other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research supported by the Foundation; and finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

E. F. Infante
Division Director
Mathematical & Computer Sciences

cc: Dr. R.M. Santilli
President, I.B.R.

Dr. Zbigniew H. Nitecki
Program Director for Geometric Analysis

I. B. 922 - R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

April 18, 1983

Dr. E.P.INFANTE, Division Director
Mathematical & Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

RE: Research grant application entitled
"Mathematical studies on Lie–admissible algebras...."
by [redacted]
NSF reference number MCS–8305548

Dear Dr. Infante,

We acknowledge receipt of your letter of April 14, 1983, declining support of the proposal by [redacted] and [redacted] jointly with copies of the referees' reports. Please be reassured that our Institute shall [redacted] and that the same will be provided by the applicants.

Part of the I.B.R. function is to provide an independent appraisal of N.S.F. referees' reports on proposals submitted by our Institute, without any participation and prior consultation with the principal investigator and/or his/her associates. The hope is that the information may result to be of some value to N.S.F., particularly for the improvement of the refereeing process.

We are therefore taking the liberty of enclosing, most recpectfully, our appraisal of the reports on the proposal by Sagle and Holmes. As you can see, we have found reports "A" and "B" to be ethically and scientifically sound, and we agree with their consideration by N.S.F. However, we have found report "C" to be definitely invalid and unsuitable for a formal consideration under a governmental process for several reasons indicates in the enclosures.

Owing to this occurrence, we would gratefully appreciate your consideration of the case and your selection of the following alternatives.

[1] The enclosed analysis of referee's report "C" is sufficient for N.S.F. to initiate the re–consideration of the proposal, via the solicitation of a third new review; or

[2] [redacted] should formally apply for a re–consideration, in case interested; or

[3] The I.B.R. should recommend them to submit an entirely new proposal, although based on essentially the same topics.

In addition to the intrinsic mathematical merits of the proposal, we recommend that N.S.F. takes into consideration the fact that the decision whether to fund or reject the proposal by [redacted] (as well as the additional proposal by [redacted] and [redacted] also on Lie–admissible algebras, and the related proposal for the support of the Fifth Workshop on the topic) do imply, out of necessity, the corresponding decision whether our new Institute shall continue its activities or, regrettably, shall be suppressed.

In case I can be of any assistance for any needed additional information and/or comment, please do not hesitate to contact me.

Very Truly Yours

Ruggero M. Santilli
President
RMS-mlw
encls.
cc: Professor [redacted]
Dr. Z.H.NITECKI, N.S.F.

IBR APPRAISAL OF THE ENCLOSED REFEREE'S REPORT"A"ON THE PROPOSAL
"Mathematical studies on reductive Lie-admissible algebras, H-spaces, ..."
by [redacted]

The IBR considers this review ethically and scientifically sound. Indeed, the referee acknowledges explicitly the fact that the applicants, being pure mathematicians, are not expected to enter into physical (or other) applications, by therefore setting up the credibility grounds for a sound refereeing.

The only deficiency found in this report is the lack of identification of the evident fundamental character of the proposal. In fact, the project submitted by [redacted] and [redacted] deals with a generalization of a truly fundamental part of(all branches of) mathematics, Lie's theory, with evident, far reaching, mathematical and physical implications.

Despite this latter insufficiency, the IBR considers the review valid, and agrees with its consideration by NSF.

FOUNDATION

924

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editi

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8305548	INST FOR BASIC RESEARCH	JAN 31

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	GEOMETRIC ANALYSIS PROGRM

TITLE

MATHEMATICAL SCIENCES: MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND H-SPACES WITH APPLICATIONS TO

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

This is a long, detailed, proposal. A quick outline of the central theme is as follows. Define a nonassociative algebra to be a vector space, A, over a field F with bilinear multiplication $\alpha : A \times A \to A$. With such an algebra A, introduce another algebra, A^-, having multiplication $[x,y] = \alpha(x,y) - \alpha(y,x)$. The algebra A is called Lie-admissible if A^- is a Lie algebra.

Next, H-spaces are defined as generalizations of a Lie group, i.e., an H-space is an analytic, n-dim. manifold, M, with an analytic multiplication $\mu : M \times M \to M$ and an element (identity) $e \in M$ such that $\mu(e,m) = \mu(m,e) = m$ for all $m \in M$. (H-spaces are common in algebraic topology; differential geometric properties of H-spaces have been studied by J. Stasheff and others.) If, relative to a local coordinate patch at the identity, $F : \mathbb{R}^n \times \mathbb{R}^n \to \mathbb{R}^n$ represents μ, one expands F in a taylor series, the second order term of which gives a bilinear form describing the "tangent algebra" of H at e. This is analogous to the derivation of the Lie algebra of a Lie group from the group multiplication. In previous work, Sagle shows (loosely stated) that for any Lie admissible algebra A there is a Lie group G and local coordinate chart at the identity (not canonical coordinates necessarily) such that A can be obtained from the second order terms of the group multiplication relative to these coordinates. A major part of this proposal deals with using tangent algebras to H-spaces, and associated differential geometric properties, to classify anti-commutative algebras.

Throughout, the abstract and proposal allude to "specific applications to mechanics, physics, computer sciences and other disciplines," but nowhere is anything even close to a specific application given. This exemplifies the "usual hope" that perhaps someone (else) will find an actual application for this nice mathematical structure! To be honest, it is a proposal to study abstract algebras and relate them to H-spaces in a way analogous to the relationship between a Lie algebra and its associated Lie group.

REPORT A

(Continued on attached page)

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

[redacted] (continued)

[redacted] has produced some "solid" mathematical work. The text "Introduction to [redacted]" by [redacted] and [redacted], is a good (standard) basic text; it does not compare (at a research level) with a text such as [redacted]

The (attached) Pacific J. paper "Analytic H-spaces, [redacted] ..." by [redacted] is a substantial mathematical contribution. (In my opinion, the only "solid" paper in [redacted]'s publication list.) In any case, the collaboration of [redacted] and [redacted] has shown to lead to good results.

Finally, the budget is inflated to the point of absurdity! The proposers should consider that $18,000 (an item listed for office expenses for one year) is often the total amount for a one year grant to a mathematician (i.e., includes salary, overhead, travel, publication costs, etc.). The research proposed is not so spectacular (indeed, to a large extent it can be labeled "generalization") to warrant the level of funding requested.

REPORT A

IBR APPRAISAL OF THE REFEREE'S REPORT "B" ON THE PROPOSAL
"Mathematical studies on reductive Lie-admissible algebras, H-spaces, ..."
by [redacted] and [redacted]

This referee appears to be in good faith, as evidentiated by the explicit acknowledgment of lack of expertise on physical issues, and the abstention of any judgment based on that profile.

Also ethically and scientifically sound is the acknowledgment that the proposal does indeed contain "fresh ideas".

Nevertheless, the IBR disagree with the referee's appraisal of the scientific stature of [redacted]. Perhaps, this referee should spend some time in a research library, inspect the quotations of [redacted] work in physical circles (let alone in mathematical ones), and compare these quotations with those on his/her own work or that by others.

Equally gratuitous is the comment on [redacted]. In fact, the opposite comment would have been more pertinent. We are referring to the fact that the possible funding of the proposal would imply the possibility of major advances for [redacted]

Despite these shortcomings, the IBR considers this review valid, and agrees with its consideration by NSF.

...AL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8305548	INST FOR BASIC RESEARCH	

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	GEOMETRIC ANALYSIS PROGRM

TITLE
MATHEMATICAL SCIENCES: MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND H-SPACES WITH APPLICATIONS TO

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

[redacted] knows a lot of the kind of mathematics which appears to be relevant to the needs of the physicists involved with the I.B.S., and it should be valuable to them to have Sagle involved too. I can make no judgment on the importance of their physics. As far as the mathematics is concerned, the proposal is a reasonable approach and contains fresh ideas---particularly attractive is the use of S in classifying algebras. The proposal is worthy of support. I am tempted to say more, that it should be supported (as regards [redacted]). One should keep in mind, however, that in [redacted] long career, while he has produced some interesting work on nonassociative algebras and then on homogeneous spaces, he really has not produced any major work. The reprints appended to the proposal are consistent with this, as they include a number of interesting observations and generalizations, but appear to contain nothing deep.

Regarding [redacted], his record appears to be not at all strong (distinctly weaker than [redacted].

- REPORT B -

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

IBR APPRAISAL OF THE ENCLOSED REFEREE REPORT "C" ON THE PROPOSAL

"Mathematical studies on reductive Lie-admissible algebras, H-spaces, ..."
by [redacted]

The IBR considers this report invalid, and recommends NSF to abstein from its consideration during a formal governmental process.

The recommendation is based on (a) the manifest lack of knowledge of this referee of the physical applications of the Lie-admissible algebras; (b) the lack of explicit admission by this referee of such a condition; while (c) the great majority of the report is based on physical consideration, without any significant content of the true aspect of the proposal, the mathematical program.

To prevent shadows of ethical nature, this referee should have: (1) acknowledged his/her lack of expertise of the physical profile; (2) absteined from any judgment based on such a profile; and (c) restricted the report to topics of pure mathematics.

To begin, this referee states that the proposal "cites no clear case in which this relevance [of the Lie-admissible algebras in physics] is documented." This claim is false. In fact, the proposal quotes several articles by a number of physicists in which the relevance of the Lie-admissible algebras to a number of branches of physics is fully identified. The applicants were discouraged from reviewing such relevance in their proposal because the review would have been considered verbose and redundant by any expert in the field.

For the purpose of having a formal record at NSF on the issue, we mention here the following most rudimentary elements, with the understanding that the literature on the subject is already sustantial.

I: Relevance of Lie-admissible algebras in quantum mechanics. It is now well known that all dissipative nuclear process have a Lie-admissible algebraic structure, and that such a structure permits the achievement of results that have not been otherwise possible until now. In fact, dissipative nuclear processes have been historically represented via non-Hermitean Hamiltonians, i.e., via non-unitary time evolutions. Their Lie-admissible re-formulation is then trivially permitted by the rules

$$A' = e^{iHt} A e^{-iHt} \equiv e^{iBct} A e^{-itDB}, \quad \begin{matrix} H = Bc, \; H^\dagger = DB \\ B = B^\dagger \\ c^\dagger = D \end{matrix} \qquad (1)$$

The relevance of the reformulation is due to the fact that the conventional form admits the infinitesimal version

$$i\, dA/dt = AH^\dagger - HA \qquad (2)$$

which DOES NOT CHARACTERIZE A CONSISTENT ALGEBRA, trivially, because the "product" $AH^\dagger - HA$ is trilinear (being dependent on A, H AND $H^\dagger$). The Lie-admissible formulation, on the contrary, admits the infinitesimal form

$$i\, dA/dt = ADB - BCA, \quad D, C = \text{fixed} \qquad (3)$$

which does indeed characterize a consistent algebra, the product being a bona-fide bilinear product. In turn, the achievement of a time evolution with a consistent algebraic structure, permits the achievement of a number of physical advances that are of otherwise difficult, if not impossible derivation.

For instance, reformulation (1) permits the quantitative treatment of the irreversibility of dissipative nuclear reactions via a generalization of the principle of detailed balancing which is of direct experimental verification , and whose derivation via the conventional, nonunitary, form has remained obscur for decades. The reformulation also permits the treatment of the expected deformtation of the extended charge distribution of nucleons under external strong and electromagnetic fields that also predicts experimentally measurable effects (1% deformation of the spherical shape $xx + yy + zz = 1$ into the ellipsoids $xa^{-2}x + yb^{-2}y + zc^{-2}z = 1$ for low energy neutrons in the field of Mu-metal nuclei). A number of additional direct applications (including quantum field theory) are ignored here for brevity, but the interested reader can trace them in the physical literature quoted in the proposal.

II: Relevance of Lie-admissible algebras in classical mechanics. In a way much similar to the operator case above, the "true" Hamilton's equations [those with external terms for nonpotential forces, as originally conceived by Hamilton] do not characterize a consistent algebra in the brackets of the time evolution. However, the equations can be subjected to a simple Lie-admissible reformulation by therefore achieving a consistent, bilinear, nonassociative product. The approach is not a mere mathematical curiosity. As an example, the exponentiation of Hamilton's equations with external terms is unknown, while their Lie-admissible form is. In turn, this permits the generalization of known methods of characterizing conserved physical quantities via Lie modules, into the characterization of TIME RATES OF VARIATIONS of physical quantities via a Lie-admissible bimodule, and several other developments omitted here for brevity. Significantly, the approach achieves the so-called "direct universality", that is the capability to represent in the frame of the observer all (generally nonconservative) Newtonian systems verifying minimal topological restrictions (usually, local ty, regularity, and class C^1).

III: Relevance of Lie-admissible algebras in classical and quantum statistical mechanics. It is very well known that algebras and related mathematical tools have seen a limited application to statistics, when compared to particle physics. One of the reasons is the fact that virtually all collisions terms cannot be incorporated into the Hamiltonian. As a result, the brackets of the time evolutions of densities in phase space do not characterize a consistent algebra. This deficiency is removed by the Lie-admissible algebras which do permit the achievement of consistent algebras in the brackets of the time evolution for all known collisions terms in plasma equations. The implications of this occurrence are far reaching for mathematics, as any referee interested in advances can see.

Any possible residual doubt on the lack of qualification of this referee can be dissipated by the additional remarks expressed in the report. As another example, the referee states that "The principal interactions of physics are constrained by symmetries..." Such a remark applies to CLOSED, ISOLATED systems for which total conservation laws hold. All known applications of Lie-admissible algebras (as identified in the literature quoted in the proposal) are, instead, for OPEN NONCONSERVATIVE systems. Under these latter conditions, the breaking of the symmetries of the former conditions must be generally assumed to avoid evident inconsistencies (e.g., to prevent that energy is conserved for a dissipative reaction).

The mathematical part of the review, even though extremely brief, is equally deficient. As one example, the referee disclaims the relevance of the combination of nonassociative algebras and differential geometry, while such combination is notoriously basic for rigorous studies of quantization; etc. The final claim that the proposed research can be worked out by graduate students is astonishing. In fact, the proposal deals with the generalization of a truly fundamental part of contemporary mathematics. By the same token, if this report is taken seriously, NSF should abstain from funding

all faculty members [including the termination of possible support to this referee] because their research can be likely conducted by graduate students.

The [redacted] proposal constitutes a truly novel and fundamental project whose mathematical relevance is self-evident, and whose physical potential is equally incontrovertible. In fact, recent advances in basic knowledge have been permitted by the advances in the mathematical studies of Lie algebras (by mathematicians and physicists). There is no doubt that, lacking a corresponding mathematical study of the more general Lie-admissible algebras, physical advances along the lines indicated earlier will be suppressed.

Owing to these and other aspects, the NSF is recommended to dismiss this report from any consideration, and seek a more qualified referee.

SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM FC

NSF Form 18 (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8505548	INST FOR BASIC RESEARCH	

PRINCIPAL INVESTIGATOR [redacted]

NSF PROGRAM: GEOMETRIC ANALYSIS PROGRM

TITLE: MATHEMATICAL SCIENCES: MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND H-SPACES WITH APPLICATIONS TO

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

This proposal is perhaps most remarkable for its claim of relevance to theoretical physics of a rather formal conjunction of non associative algebra with general aspects of differential geometry. Unfortunately it cites no clear case in which this relevance is documented. Since it appears a priori unlikely the claim is unconvincing.

This is of course not to say that non-associative systems may not play a significant role in physics, but only that the particular highly general and formal material proposed for investigation has no apparent non-trivial role. In particular, general classes of non-potential interactions of the types to which the proposed formalism non-trivially applies are not clearly relevant, if indeed they exist at all. The principal interactions of physics are constrained by symmetry and/or causality considerations, and it is not shown that the proposed formalism has anything useful to offer in connection with them.

The main point remaining in the proposal would be purely mathematical work. This is tenable but the mere association of two fields such as non associative algebra and differential geometry is not in itself necessarily of much interest, and the burden of proof is on the proposer to cite interesting and non-trivial developments. What does appear is the kind of study that any competent mathematician can readily execute when needed; suitable for use as graduate student exercises, no doubt, but not clearly anything beyond that.

REPORT C

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

April 29, 1983

Dr. Ruggero M. Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I am writing in response to your letter of April 18, concerning the proposal MCS-8305548 submitted to the National Science Foundation by your organization with [redacted] as principal investigator. Enclosed is a copy of NSF Notice No. 84, which explains the Foundation's procedures for reconsideration of declined proposals. Please note that such procedures are initiated by the principal investigator for the proposal, rather than the officials at the submitting organization. Similarly, in accordance with NSF policy, verbatim copies of reviews are sent only to the principal investigator, and are intended to be treated as confidential information. Therefore, any discussion with you concerning these reviews would not be appropriate. The relevant program officer, Dr. Nitecki, or I will be happy to discuss this declination, and the reviews on which the decision was based, with the principal investigator for the proposal.

Sincerely,

E. F. Infante
Division Director
Mathematical and Computer Sciences

Enclosure

cc: Professor [redacted]

Dr. Z. Nitecki
Program Director for Geometric Analysis

PART XXVIII:

REJECTION BY THE NATIONAL SCIENCE FOUNDATION OF AN I.B.R. APPLICATION BY THREE, SENIOR, MATHEMATICIANS

PROPOSAL TO THE NATIONAL SCIENCE FOUNDATION

FOR CONSIDERATION BY NSF ORGANIZATIONAL UNIT (Indicate the most specific unit known, i.e. program, division, etc.)	IS THIS PROPOSAL BEING SUBMITTED TO ANOTHER FEDERAL AGENCY? Yes ___ No ___ ; IF YES, LIST ACRONYM(S):

PROGRAM ANNOUNCEMENT/SOLICITATION NO.:	CLOSING DATE (IF ANY):

NAME OF SUBMITTING ORGANIZATION TO WHICH AWARD SHOULD BE MADE (INCLUDE BRANCH/CAMPUS/OTHER COMPONENTS)

THE INSTITUTE FOR BASIC RESEARCH (I.B.R.)

ADDRESS OF ORGANIZATION (INCLUDE ZIP CODE)

96 Prescott Street, Cambridge, Massachusetts 02138

TITLE OF PROPOSED PROJECT

STUDIES ON LIE-ADMISSIBLE ALGEBRAS

REQUESTED AMOUNT	PROPOSED DURATION	DESIRED STARTING DATE
$ 292,2210	Three years	March 1983

P./PD NAME AND SOCIAL SECURITY NO. (SSN)*	PI/PD PHONE NO.
[redacted]	(617) 864 9859 [redacted]

P./PD DEPARTMENT	PI/PD ORGANIZATION
Division of Mathematics, I.B.R. and Dept. of Math., Univ. of [redacted]	

ADDITIONAL PI/PD AND SSN*

[redacted] tel.s (617)864 9859/ [redacted]
Div. of Math., I.B.R., and Dept of Math. Univ. of [redacted]

ADDITIONAL PI/PD AND SSN*

[redacted] tel.s (617) 864 9859/ [redacted]
Div. of Math. I.B.R., and Dept. of Math., [redacted] Univ., [redacted]

FOR RENEWAL OR CONTINUING AWARD REQUEST, LIST PREVIOUS AWARD NO.:	SUBMITTING ORGANIZATION IS ___ IS NOT ___ A SMALL BUSINESS CONCERN (see CFR Title 13, Part 121 for definitions).

*Submission of social security numbers is voluntary and will not affect the organization's eligibility for an award. However, they are an integral part of the NSF information system and assist in processing the proposal. SSN solicited under NSF Act of 1950, as amended.

CHECK APPROPRIATE BOX(ES) IF THIS PROPOSAL INCLUDES ANY OF THE ITEMS LISTED BELOW:

☐ Animal Welfare	☐ Human Subjects	☐ National Environmental Policy Act
☐ Endangered Species	☐ Marine Mammal Protection	☐ Research Involving Recombinant DNA Molecules
☐ Historical Sites	☐ Pollution Control	☐ Proprietary and Privileged Information

PRINCIPAL INVESTIGATOR/ PROJECT DIRECTOR	AUTHORIZED ORGANIZATIONAL REP.	OTHER ENDORSEMENT (optional)
NAME [redacted]	NAME R.M.SANTILLI	NAME
SIGNATURE [redacted]	SIGNATURE [signature]	SIGNATURE
TITLE Principal Investigator	TITLE President, I.B.R.	TITLE
DATE 10-21-1982	DATE 10-18-1982	DATE

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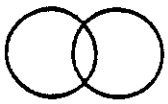
Information on The Institute for Basic Research

ABSTRACT

Lie-admissible algebras were introduced in 1948 by A.A. Albert. In 1967 R.M. Santilli first pointed out that Lie-admissible algebras may be more appropriate than Lie algebras for studying physical processes. Santilli refined his idea in a sequence of papers over several years. Meanwhile a few mathematicians wrote on the structure and classification of Lie-admissible algebras as a topic in pure mathematics. With the inception of the annual Workshops on Lie-Admissible Formulations in 1978, physicists and mathematicians began to meet together to discuss their interests in Lie-admissible algebras.

Since 1978 there has been growing evidence, at first theoretical but now based on experimental results, that Lie-admissible algebras are a proper mathematical tool to formulate and solve a number of physical problems. During the Fourth Workshop held in August 1981, it became clear that physics would benefit from solutions to certain mathematical problems. . They include the development of a representation theory and universal envelope for Lie-admissible algebras, and classification and structure theory especially for mutation algebras. The principal investigators propose to work on these problems and other problems that the physics will suggest during the course of the Investigation.

The applications of the mathematical tools to be developed under this research project are rather promising and of diversified nature, encompassing a number of branches of physics, engineering, and applied mathematics at large. In fact, a number of recent papers have indicated that the theory of Lie-admissible algebras can be applied to: Newtonian mechanics and space mechanics (e.g. trajectory problems under drag forces); statistical mechanics and plasma physics (e.g. statistical ensembles inclusive of inelastic collisions and nonlocal nonpotential internal forces); particle physics (e.g. for the treatment of strong interactions as of nonlocal nonpotential type due to wave overlapping of particles); computer modeling and engineering (e.g., electrical circuitry and electronics with internal losses); and other fields.



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 29, 1982

Dr. HARVEY KEYNES
Program Director
Modern Analysis
Division of Mathematics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Keynes,

We hereby submit for consideration by the Division of Mathematics of NSF the research grant proposal entitled

STUDIES ON LIE–ADMISSIBLE ALGEBRAS

with Professor [redacted] as Principal Investigator, and Professors [redacted] and [redacted] as Co-Investigators.

The original, duly signed, proposal is enclosed, while nine additional samples have been mailed to you via separate parcel.

To my understanding, the proposal might qualify along the lines of a group proposal. Therefore, I trust in your leniency regarding the fact that its length exceeds fifteen pages. At any rate, the Principal Investigators have found considerable difficulty in containing the length of the proposal to fifteen pages, owing to the novelty and diversification of the project.

I would appreciate knowing whether the consideration process takes into account the rather considerable and fast growing applications of mathematical studies of Lie–admissible algebras in particle physics, statistical mechanics, Newtonian Mechanics, and other disciplines. For this purpose, I remain at your disposal either for an outline of these applications or for the preparation of a list of physicists working in this field. Also, Professor Peter Rosen, of the Division of Physics of NSF, has a fairly complete file on this subject. I am confident you will find him very cooperative.

In addition, I would like to bring to your attention the fact that the research conducted by Professors [redacted] is very closely related and actually complementary to the studies conducted by Professors [redacted] under support of the Division of Mathematics of NSF.

Finally, I remain at your disposal for the preparation, on request, of a list of mathematicians experts in Lie—admissible algebras, as well as for any additional assistance you might need.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Enclosure

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

January 24, 1983

Ruggero Maria Santilli, Professor of Theoretical Physics and President

Dr. R.E.KAGARISE
Acting Assistant Director
Division of Mathematics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Kagarise,

I am contacting you in regard to the following research grant proposals our Institute has submitted to your Division:

PROPOSAL 1:
TITLE: Studies on Lie–admissible Algebras
PRINCIPAL INVESTIGATORS: Professors [redacted] *and* [redacted]
DATE OF SUBMISSION: October 29, 1982
NSF ID NO: MCS–8303574

PROPOSAL 2:
TITLE: Mathematical studies on reductive Lie–admissible algebras and H–spaces, with applications to the geometry of nonpotential dynamical systems
PRINCIPAL INVESTIGATORS: Professor [redacted] *and* [redacted]
DATE OF SUBMISSION: November 15, 1982
NSF ID NO: MCS–8305548

PROPOSAL 3:
TITLE: Fifth Workshop on Lie–admissible Formulations
PRINCIPAL INVESTIGATORS: Professors [redacted]
DATE OF SUBMISSION: November 4, 1982
NSF ID NO: MCS–8303592

We would appreciate the indication of the period of time in which a decision on these applications could be reached. The information would be particularly valuable for our planning, particularly for Proposal 3 dealing with the organization of a meeting. In fact, we have prepared the announcement of the meeting, but we are delaying its distribution pending the decision for possible funding.

I am taking the opportunity of enclosing a general presentation of the research conducted at our Institute with the emphasis on the experimental and theoretical profiles. We hope that the presentation may give an idea of the value of the research in Lie–admissible generalization of Lie theory, along the lines of the proposals under consideration at your Division.

Very Truly Yours

Ruggero M. Santilli
President
cc: Professors [redacted]
RMS–mlw
encls.

NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

January 27, 1983

Dr. Ruggero M. Santilli, President
Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

The proposal, "Studies on Lie-Admissible Algebras," submitted by your organization on behalf of Professors [redacted] and [redacted] has been assigned to the Algebra and Number Theory Program. It will now be reviewed and evaluated before a recommendation is made on its final disposition.

A number of factors go into the recommendation, including the vitality of the area of investigation and the potential for applications, as mentioned in your letter. Nonetheless, I must point out that budgetary considerations force the declination of a number of very strong proposals every year. It is impossible to forecast the outcome of the evaluation process at this early stage.

Finally, I am enclosing copies of a letter which has been sent to Professors Myung, Oehmke and Tromber. Please feel free to respond to the questions raised there if such a response could clarify the situation.

Sincerely yours,

Judith S. Sunley

Judith S. Sunley
Program Director for
Algebra and Number Theory

Enclosures

NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

January 27, 1983

Professor [redacted]
Department of Mathematics
[redacted] University
[redacted]

Dear Professor [redacted]

We have received your proposal entitled, "Studies on Lie-Admissible Algebras" submitted through the Institute for Basic Research. While it is not clear where the proposed work would be performed, it appears to us that it would be carried out at Michigan State University. If this is so, we will need a letter signed by the appropriate university business officer (not the department chairperson) stating that the necessary university facilities, etc., will be made available without charge, if such is the case, or what arrangements the university deems appropriate. The arrangements for secretarial service, etc., should also be specified.

While such matters as health insurance, workman's compensation, social security, etc., are not properly our concern in a specified case, we suggest that you may wish to explore your situation in such matters during periods when you are employed by an institution other than Michigan State University.

Sincerely yours,

Judith S. Sunley
Program Director for
Algebra and Number Theory

Copy to:
[redacted]
Vice President
Finance and Operations
[redacted] University

R. M. Santilli, President
Institute for Basic Research

NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

January 27, 1983

Professor [redacted]
Department of Mathematics
University [redacted]

Dear Professor [redacted]

We have received your proposal entitled, "Studies in Lie-Admissible Algebras," submitted through the Institute for Basic Research. While it is not clear where the proposed work would be performed, it appears to us that it would be carried out at the University of Northern Iowa. If this is so, we will need a letter signed by the appropriate university business officer (not the department chairperson) stating that the necessary university facilities, etc., will be made available without charge, if such is the case, or what arrangements the university deems appropriate. The arrangements for secretarial service, etc., should also be specified.

While such matters as health insurance, workman's compensation, social security, etc., are not properly our concern in a specified case, we suggest that you may wish to explore your situation in such matters during periods when you are employed by an institution other than the University of Northern Iowa.

Sincerely yours,

Judith S. Sunley
Program Director for
Algebra and Number Theory

Copy to:
Authorized Organizational Representative
University of [redacted]

R. M. Santilli, President
Institute for Basic Research

NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

January 27, 1983

Professor [redacted]
Department of Mathematics
University [redacted]
[redacted]

Dear Professor [redacted]

We have received your proposal entitled, "Studies on Lie-Admissible Algebras" submitted through the Institute for Basic Research. While it is not clear where the proposed work would be performed, it appears to us that it would be carried out at the University of Iowa. If this is so, we will need a letter signed by the appropriate university business officer (not the department chairperson) stating that the necessary university facilities, etc., will be made available without charge, if such is the case, or what arrangements the university deems appropriate. The arrangements for secretarial service, etc., should also be specified.

While such matters as health insurance, workman's compensation, social security, etc., are not properly our concern in a specified case, we suggest that you may wish to explore your situation in such matters during periods when you are employed by an institution other than the University of Iowa.

Sincerely yours,

Judith S. Sunley
Program Director for
Algebra and Number Theory

Copy to:
[redacted]
Vice President for Educational
Development and Research
University of [redacted]

R. M. Santilli, President
Institute for Basic Research

University [redacted]
Grants and Contracts

[redacted]

February 8, 1983

Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, D.C. 20550

Dear Ms. Sunley:

I am writing to you to answer the questions raised in your letter of January 27, 1983 to Professor [redacted] related to his proposal "Studies in Lie'Admissible Algebras" submitted through the Institute for Basic Research.

Professor [redacted] work will be carried out at the University [redacted]. He will retain his office and access to any other university facilities which his work requires. [redacted], chair of the department of Mathematics and Computer Science has assured me that the department will also provide the necessary secretarial support (estimated by Professor [redacted] at 40 hrs.) for [redacted] work under the grant.

Thank you for your mention of the matter of fringe benefits. The university will continue to provide all normal annual benefits for Professor [redacted] during the period of the grant.

If you have any further questions about the University's support for Professor [redacted], do not hesitate to contact me or [redacted]

Sincerely,

[redacted]
Administrator

HJB/ds

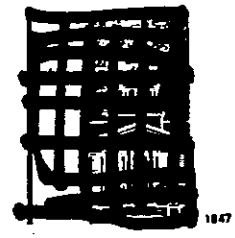
cc: [redacted]

The University [redacted]

[redacted]

[redacted] Science

[redacted]



February 9, 1983

Ms. Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, D.C. 20550

Dear Ms. Sunley:

In response to your letter of January 27, 1983 concerning the proposal "Studies on Lie-admissible Algebras" submitted by the Institute for Basic Research I would like to make the following comments.

During selected time periods I would be employed by the Institute of Basic Research to perform the activities as delineated in the proposal; most likely during some Summer months.

At no time would there be a conflict or overlap of my employment by the University [redacted] and my employment by the Institute of Basic Research.

All administrative expenses accrued in the implementation of the grant would be the responsibility of the Institute of Basic Research.

If secretarial funds are provided by the grant and if it is necessary to have typing done in [redacted] such typing would be privately contracted and paid from the grant funds.

It is anticipated that no University facilities will be required.

Sincerely,

[redacted]

Professor and Chairman
Department of Mathematics

Approved by:

[redacted]

[redacted]n
Research Coordinator
Office of the Vice President
For Educational Development and Research
Acting for [redacted]
Vice President and Dean

[redacted] UNIVERSITY

VICE PRESIDENT FOR FINANCE AND OPERATIONS AND TREASURER
CONTRACT AND GRANT ADMINISTRATION
[redacted]

February 2, 1983

Dr. Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, DC 20550

Dear Dr. Sunley:

In response to your letter of January 27, 1983, [redacted] University would be pleased to provide office space, library privileges and other customary services, specifically, including secretarial work to Dr. [redacted] during the period of his proposed grant with the National Science Foundation through the Institute for Basic Research.

Very truly yours,

[redacted]
[redacted] Assistant Director
Contract and Grant Administration

APPROVED: [redacted]
[redacted] Assistant Vice President

[redacted] UNIVERSITY

VICE PRESIDENT FOR FINANCE AND OPERATIONS AND TREASURER
CONTRACT AND GRANT ADMINISTRATION
[redacted]
[redacted]

February 2, 1983

Dr. Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, DC 20550

Dear Dr. Sunley:

In response to your letter of January 27, 1983, [redacted] University would be pleased to provide office space, library privileges and other customary services, specifically, including secretarial work to Dr. [redacted] during the period of his proposed grant with the National Science Foundation through the Institute for Basic Research.

Very truly yours,

[redacted]
[redacted] Assistant Director
Contract and Grant Administration

APPROVED: [redacted]
[redacted] Assistant Vice President

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

February 10, 1983

Professor Ruggero M. Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santelli:

I am replying to your letter of January 24 to Dr. Kagarise concerning three proposals submitted to the National Science Foundation by the Institute for Basic Research.

MCS-83-03574, "Studies on Lie-admissible Algebras", was logged in on November 17, 1982.

MCS-83-05548, "Mathematical studies on reductive Lie-admissible algebras and H-spaces, with applications to the geometry of nonpotential dynamical systems", was logged in on December 30, 1982.

MCS-83-03592, "Fifth Workshop on Lie-admissible Formulations", was logged in on November 17, 1982.

As stated in Grants for Scientific Research, NSF-81-79h, enclosed, "applicants should allow 6 to 9 months for review and processing... Every effort is made to reach a decision and inform the applicant promptly." We do aim for the 6 month end of the range but vagaries of the return of reviews by the ad hoc mail reviewers, changes in workloads, etc., do sometimes make that impossible. I have checked with the program directors who have been assigned the responsibility of handling these proposals and have been told that the review and evaluation process seems to be proceeding routinely.

Please feel free to write or to call (202-357-7341) if you have any further questions.

Sincerely yours,

William Gorlen

William G. Rosen
Head
Mathematical Sciences Section

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

March 3, 1983

Ms. JUDITH S. SUNLEY
Program Director
National Science Foundation
WASHINGTON, D.C. 20550

RE: Applications entitled
STUDIES ON LIE–ADMISSIBLE ALGEBRAS
Principal Investigators: Drs. [redacted]
and [redacted]

Dear Ms. Sunley,

It is our understanding that, following your request of January 27, all administrations of the principal investigators have provided you with a formal authorization for a possible administration of the contract by the I.B.R.

We hope that the answers you received are satisfactory, and that the consideration of the proposal can now proceed toward a speedy resolution.

Your interest in the proposal is sincerely and gratefully appreciated.

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

cc: Drs. [redacted]

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Mathematical and Computer Sciences
Mathematical Sciences Section

APR 21 1983

Dr. [redacted]
Department of Mathematics
University [redacted]

Dear Dr [redacted]

We regret to inform you that the National Science Foundation is unable to support your proposal no. MCS83-03574 for "Studies on Lie-Admissible Algebras."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research supported by the Foundation; and finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

E. F. Infante
Division Director
Mathematical and Computer Sciences

cc: Dr. R. M. Santilli, President
Institute for Basic Research
Cambridge, Massachusetts 02138

Dr. Judith S. Sunley
Algebra and Number Theory

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8303574	INST FOR BASIC RESEARCH	
PRINCIPAL INVESTIGATOR	NSF PROGRAM	
[redacted] C	ALGEBRA AND NUMBER THEORY	

TITLE
MATHEMATICAL SCIENCES: STUDIES ON LIE-ADMISSIBLE ALGEBRAS

COMMENTS (QUALITY OF THE PROPOSED RESEARCH; RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Whether or not research on Lie-admissible algebras merits support of the kind requested rests on two grounds: (1) importance of the work to physics: (2) the extent and particularly the depth of the results judged as mathematics. I cannot comment on the first part except to wonder why virtually all the work is published in the somewhat obscure Hadronics Journal. Regarding the second point, there is now a considerable body of results on Lie-admissible algebras. This is respectable work produced by a competent mathematicians. In reading Tomber's history of the subject I get the feeling that it has developed rather unsurprisingly, with appropriate use being made of Lie algebra theory and other aspects of (mostly nonassociative) algebra. But I don't see anything of real depth. This is in conformity with the fact that the people contributing papers on the subject (I exclude Albert, who apparently only contributed the definition in passing in a paper devoted to other matters) include some good mathematicians, but none of really high international stature.

My opinion, based on the mathematics as mathematics, is that work in this field is worthy of support but does not have priority.

Comments on the individual mathematicians applying: [redacted] has done almost no research since early in his career, and even then did only a very small amount; a weak case for support.

[redacted] did some significant work on nonassociative algebras earlier in his career and then some other work (semigroups) before apparently recently turning to Lie-admissible algebras. He is a competent mathematician who can make reasonable contributions but I don't foresee him doing something profound; worthy of support.

[redacted] His early work was industrious but apparently unexciting. Then in 1978 he began publishing copiously on Lie-admissible algebras in the Hadronic Journal. Since he is, in volume, by far the largest contributor to the subject, my remarks about the subject apply especially to him; worthy of support.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8303574	INST FOR BASIC RESEARCH	
PRINCIPAL INVESTIGATOR	NSF PROGRAM	
[redacted] C	ALGEBRA AND NUMBER THEORY	

TITLE
MATHEMATICAL SCIENCES: STUDIES ON LIE-ADMISSIBLE ALGEBRAS

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEETS AS NECESSARY.

I have to disclaim any ability to judge the relevance of the proposed research to physics. I turn to the mathematics per se.

Of the numerous specialties in today's mathematical scene, the Albert school of nonassociative algebras is unusually vulnerable to the charge of being isolated and lacking significance. Of course this could change tomorrow. In the meantime it seems that the study of various classes of algebras defined by identities has outrun its motivation and its examples. Overall verdict: "good".

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act; 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8303574	INST FOR BASIC RESEARCH	

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted] C	ALGEBRA AND NUMBER THEORY

TITLE
MATHEMATICAL SCIENCES: STUDIES ON LIE-ADMISSIBLE ALGEBRAS

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Some aspects of this proposal look interesting, but at the present time, I do not think that the theory of Lie-admissible algebras has proved its importance. Perhaps my evaluation should be taken as a challenge to the proposers to uncover deeper mathematical or physical phenomena. When and if they can do this, their proposals will be stronger. I would be very happy to see my evaluation proved wrong by future significant discoveries by the proposers.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☒ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

I. B.954 - R.

THE INSTITUTE. FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

April 30, 1983

Dr. E.F.INFANTE, Division Director
Mathematical and Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Infante,

I feel obliged to express my extreme reservation regarding the way your division has handled the application by Professors [redacted] (Univ. of [redacted] and IBR), [redacted] (Univ [redacted] and IBR), and [redacted] ([redacted] Univ.), NSF ref. no. MCS-8303574. I am referrin first to the fact that, early in 1983, your office contacted the administration of each primary affiliation of the applicants to request authorization for the IBR administratio of the (possible) grant. This contact was made without any prior knowledge whatsoever, either to us here, or to any of the applicants. The subsequent rejection then seems to confirm rumors repeatedly heard in academic corridors, that at times NSF does not stop at the rejection of grants, but goes ahead with actions which, whether intentionally or accidentally, have the net result of discrediting or otherwise damaging the applicants, and/or their administrative conduits.

When, on the top of this, you see that the numerical majority of the referees (the 2/3) warmly recommends funding, that the applicats are all senior, full professors, and that the content of the proposal is truly of potentially fundamental advances, then you cann dispel shadows of political manipulations, including the possibility that the negative decision was the result of pressures by corrupt elements outside your division.

You should keep in mind that the preceding application rejected by your office had also received the favorable support by the numerical majority of the referees (2/3) and was equally of high caliber, both for the applicants and for contents. I am referring to the application by Professor [redacted] (Univ. [redacted] and IBR) and [redacted] ([redacted].), NSF ref. No. MCS-8305548. The question we must therefore naturally ask from this ripetitive pattern, is whether your chain of rejections (and additional ones now expected) is a form of language to express a conceivable opposition by NSF and its academic affiliate against the organization of our Institute as currently conceived, that is, in a way independent from academic-financial-ethnic interests at Harvard University and at the Massachusetts Institute of Technology, which way is evidently essential for genuine advan

In the primary interest of NSF, I would like to recommend that the entire situation of our various applications to both your division as well as to the division of physics be reviewed, possibly with direct consultation with the NSF Director's Office and The White House Office of Science and Technology.

In particular, I suggest that my original suggestion be considered in the appropriate diversification of aspects. I am referring to the granting of an institutional support to the IBR which combines all our projects in experimental physics, theoretical physics, mathematics, international meetings, and related activities.

As you know, the IBR was funded by a group of independent experimentalists, theoretician and mathematicians for the specific purpose of attempting a generalization of Einstein's special relativity for strong interactions. Our experimental, theoretical, and mathematical programs are all deeply inter-related toward this single goal. The elements for success are there at all levels. I am referring here to the technical capability of available laboratories to conduct the needed tests; to the theoretical elaborations; and most importantly, to the backgrounds mathematical research.

A (very brief) summary paper on the generalization of the special relativity is enclosed, while over 10,000 pages of published research are at your disposal, including five research monographs, nine volumes of reprints of international meetings, and a massive number of papers.

As you can see, the main idea is that hadrons, since are extended in space, are deformable under sufficiently intense external fields and/or collisions, that is, their spherical charge distribution $xx + yy + zz = 1$ can be deformed into ellipsoids $xa^2x + yb^2y+zc^2z=1$ with consequential, manifest, breaking, first, of the rotational symmetry and, second, of the entire Lorentz symmetry, although in a sufficiently small amount.

Our experimental program (which we could not submit to the NSF division of physics because of difficulties beginning with the presentation) is centered in the repetition of the experiment Prof. Rauch (Director of the Atominstitut of Wien, Austria) has been conducted since 1975 via neutron interferometry. NOTE THAT HIS LATEST MEASURES CONFIRM THE BREAKINGS.

Our theoretical program is centered in the achievement of the so called isotopic and genotopic liftings of quantum mechanics in its various aspects, as an operator image of the generalization of classical Hamiltonian mechanics already achieved by our group (the so-called Birkhoffian mechanics), and as described in more details in our summer meetings.

Finally, our mathematical studies concern truly vital information for the above theoretical and experimental research. It can only be done, to our knowledge, via a generalization of the very heart of contemporary mathematics, Lie's theory. This is exactly the topic of the chain of proposals you have rejected.

Apart from evidently manifest, large scientific implications, I feel obliged to bring to your personal attention, to the attention of the NSF Director and to the attention of The White House Office of Science and Technology, the potentially significant military damage which may result by this NSF posture of chains of rejections. Admittently, none of these applications has been studied so far. However, you are aware that Eintein's special relativity permitted the discovery of two new basic weapons that have changed the face of the world, fission and fusion bombs. It is known in the scientific environment that, by no means, these are the only ways of extracting weapons (or energy, if you prefer) from hadrons. Simply we have not yet found other alternatives. Then a possible generalization of the special relativity specifically conceived for hadrons has a self-evident potential for truly basic, new, military applications I leave to your imagination while, on my part, I intend to be silent at this time.

The difficulties we have encountered in the conduction of our research have been simply beyond the wildest imagination. In fact, the program started under DOE support while I was a member of Harvard University, but, the moment it was clear in its objectives, I was forced to leave Harvard despite the availability of support, and we even received a formal prohibition to held our third workshop on Harvard premises because occurring a few weeks after the termination of my employment (although its organization was an important part of my contract). Similar interferences and opposition occurred at MIT leaving us no other alternative than that of organizing an independent institute of research.

These occurrences should be openly, plainly, and clearly indicated as a necessary condition to achieve a maturity of judgment in our grant applications. In fact, the selection of referees from these local institutions would be a mere farse under the circumstances. At any rate, all the numerous episodes are fully documented and well known to many. Most importantly, the clear identification of this situation is essential for your achievement of maturity of final decisions.

The ultimate reasons for this organized, at time hysterical opposition we have encountered is also known and it is not related to our persons (in fact I personally have several friends at both Harvard and MIT). It is due to the vested, organized, academic-financial-

ethnic interests that surround or, better, are based on Einstein's special relativity, as I am sure all of you can imagine if not personally knowleageable of it. These interes are therefore opposed to the very idea of the generalization of the special relativity.

However, their credibility is virtually null, and their political nature is manifest. In fact, you do not have to be a physicist or a mathematician to see that a sphere can be deformed into an ellipsoid, with consequential breaking of spherical symmetry (the breaking of the Lorentz symmetry is then a mere technical consequence).

Thus, our strength rests on the fact that the ordinary taxpayer can readily see the academic dances regarding the currently assumed exact validity of the special relativity in particle physics. In fact, every taxpayer can see that a sphere can be deformed, and that the academic baron is doing dances for his/her own personal interests, but certaintly not in the interest of advances, whenever he/she claims that extended particl are absolutely rigid (a necessary condition to salvage the special relativity).

The implications for NSF regarding this situation are staggering. I have attempted a number of times to bring them to the attention of the various officers thoughout a numbe a years (over a decade) with total and complete failure until now.

Quite openly, I have now reached a point where I begin to have doubts on the orderly communication of the information, and that perhaps a full disclosure to the U.S. (as well as the international) community is more appropriate. We should not forget that NSF is spending truly large amounts of taxpayers money on the MERE BELIEF of the exact character of the special relativity for strong interactions, while ALL our research grant applications throughout the years on the problem have been rejected. This includes the rejection of the primary application of the IBR, that on physics, made quite recentl Needless to say the application was exactly on the fundamental aspects of the generaliza of the special relativity (liftings of the Hilbert space).

At any rate, time is running out for all, the NSF and the IBR. Our institute was funded as a result of (for us) immense sacrifices. It has been operated now for two years witho one penny of governmental support. As chief executive officer I must take all the necess ry precaution to prevent further damage to its members resulting from the rather massive chains of rejections we have seen during this period. Most importantly, we must take a number of decisions that are predictably difficult for all.

I am therefore recommending that, whatever decision may be taken by your office or by the NSF Director's Office, if any, should be taken in the very near future. There is simply no more time for additional time-extentive investigations and considerations.

Very Truly Yours,

Ruggero Maria Santilli
President

cc.: [redacted] and Dr. E. Knapp, NSF Director

PART XXIX:

REJECTION BY THE NATIONAL SCIENCE FOUNDATION OF AN I.B.R. APPLICATION BY TWO SENIOR PHYSICISTS

submitted to the

U. S. DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street

Cambridge, Massachusetts 02138

Tel. (617) 864 9859

entitled

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

Proposed Starting Date	Proposed Duration	Amount Requested
March 1983	One Year	$ 95,400

ENDORSEMENTS

[redacted]

Principal Investigator
(experimental physics)
[redacted] National Laboratory
I. B. R., Cambridge, MA

[redacted]

Co-Investigator
(theoretical physics)
The Institute for Basic Research
Cambridge, Massachusetts 02138
[redacted]

10-2-82

R. M. SANTILLI
President
The Institute for Basic Research
Cambridge, Massachusetts 02138
Tel. (617) 864 9859

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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ENCLOSURES

[1] [redacted]

[2] [redacted]

[3] [redacted] Lettere al Nuovo Cimento 2, 402 (1971)

REFERENCE ADDED IN PROOF:

[redacted]

ABSTRACT

Despite well known technological advances, the electric field of electrons is still assumed to be of the type conceived by Coulomb back in 1785, that is, to be constant at fixed distances. Santilli recently remarked that, if the electron has any dynamical structure (e.g., it is an elementary oscillation of space), its field will likely possess an explicit time dependence. He therefore proposed the hypothesis according to which the electric field of individual electrons has an oscillatory behaviour in time with frequency of 7.57×10^{20} sec^{-1}, by therefore being inclusive of both attractive and repulsive actions. Their separation results into a pulsating force and occurs during the interactions of individual pairs of electrons, positrons, or electron—positrons. When a sufficiently large collection of electrons is considered, all pulsating effects disappear owing to the very high frequency, and the conventional Coulomb law is recovered. A number of conceivable experiments were indicated, including those via the use of the positronium.

Independently from these studies, [redacted] and his associates proposed the hypothesis that the positronium admits the C—violating decay $^1S_0 \rightarrow 3\gamma$ besides the conventional C—conserving decay $^3S_1 \rightarrow 3\gamma$, and predicted the ratio of the rates of these decays to be of the order of 10^{-10}. The hypothesis was formulated on the basis of a number of similarities existing between the decay of the positronium and that of $K^o_{S,L}$ particle. The established CP violation of the latter then suggested a conceivable C—violation of the former. [redacted] hypothesis can be tested today thanks to advances in particles accelerators and detectors.

[redacted] hypotheses are clearly inter—related, inasmuch the former provides a theoretical background of the latter. This proposal recommends the conduction of a comprehensive study of the hypotheses considered, ranging from theoretical studies of mutual compatibility and compatibility with existing data, to the feasibility study of a number of experiments. The study of possible implications of the hypotheses for computers, solid state, and other systems is also recommended.

Department of Energy
Washington, D.C. 20545

OCT 2 8 1982

Dr. Ruggero M. Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I have reviewed your preliminary proposal, "Theoretical and Experimental Studies on a Possible Pulsating Structure on the Coulomb Force of Individual Electrons", and find that the subject matter is substantially distinct from that research eligible for support within the reach of the Atomic Physics Program.

As a result of the above, it should come as no surprise to you that I am not able to refer to you any work that treats the same subject as described in your proposal.

It appears appropriate for you to discuss the intent of your above-stated research with members of the Office of High Energy and Nuclear Physics. Since you have or have had support from that Office, you are probably better informed than I on whom to contact.

Sincerely,

J. V. Martinez
Fundamental Interactions Branch
Division of Chemical Sciences
Office of Basic Energy Sciences

NATIONAL SCIENCE FOUNDATION
WASHINGTON. D C. 20550

October 12, 1982

Dr. R. M. Santilli, President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of 4 October 1982 concerning the preliminary draft of the proposal by [redacted]. After examining it, I would suggest that the Elementary Particles Program under Dr. David Berley would be suitable for the experimental aspects and the Theoretical Physics Program under Dr. Boris Kayser for the theoretical aspects.

With best wishes,

S. Peter Rosen
Program Associate for
Theoretical Physics

I. B.[963] - R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

January 6, 1983

Dr. DAVID BERLEY
Division of Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Berley,

I respectfully submit for consideration by N.S.F. the enclosed original application entitled

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

with Principal Investigator Professor [redacted]

The application is submitted to you because of its primary emphasis on the formulation of experiements for the resolution of the problem at some future time. Theoretical aspects are considered, but only in a way subordinate to this primary goal.

The application has been submitted jointly to your Office and to the Division of High Energy Physics of the Department of Energy. Any additional submission will be promptly communicated to you.

I may add that an informal advance consultation on the project was submitted to DARPA (Dr. C. Romney, Deputy Director). It was agreed that the proposal is of too basic character to be within DARPA's guidelines. However, it was also agreed that, in case the basic aspects are positively resolved, we shall contact DARPA again because of a number of rather intriguing possibilities of military applications of this possible pulsating effect, let alone conventional non-military applications.

Your consideration of the proposal is appreciated. In case you need any additional assistance, please do noy hesitate to let me know.

Very Truly Yours

Ruggero M. Santilli
President

RMS-mlw

I. B. R. — 964 —

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

January 6, 1983

Dr. W.A.WALLENMEYER, Director ER-22
Division of High Energy Physics
DEPARTMENT OF ENERGY - GTN
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

I respectfully submit for consideration by your office the research grant application entitled

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

under the Principal Investigator, Prof. [redacted]. The original is enclosed, while nine additional copies have been separately mailed to you.

The proposal is submitted jointly to your Office and to the Division of Physics of the NSF. Any additional formal submission will be promptly communicated to you.

I may add that an informal, advance-consultation on the project occurred with Dr. C. Romney (202 694 3035), Deputy Director of DARPA. It was agreed that the proposal is of too basic character to be within DARPA's guidelines. However, it was also agreed that we should keep DARPA informed of possible positive outcomes of the basic aspects of the projects because of rather intriguing military possibilities (non-military possibilities are self-evident for the project).

You should be informed that the military possibilities are foreseen as conceivable at this time when the project submitted here is combined with our main research grant application on the development of the hadronic mechanics (the mechanics specifically conceived for the interior of strongly interacting systems). In fact the possibilities deal with the case when electrons are totally immersed within hadrons (in which case the insufficiency of conventional quantum mechanics is more transparent), and deal with systematic studies on the problem whether fusion and fission are the only forms of hadronic weapons, or other forms are possible.

Needless to say, all our applications to your Office, including this one, have been prepared without any mention whatsoever of military applications. A verbal report of possibilities that might be worth considering has been made to Dr. Romney, while an informative report is currently under preparation for his office (only). Confident on your benevolent understanding, I would like to report from now on military possibilities only to DARPA's Office. Jointly, I would like to encourage you to enter into direct contact with Dr. Romney, with full confidence that you will find him most cordial and cooperative. No information on the matter has been released to NSF until now.

Very Truly Yours

Ruggero Maria Santilli
President

cc.: Dr. C. ROMNEY, Deputy Director, DARPA, 1400 Wilson Blvd, Arlington, Va 22209
Drs. B. HILDEBRAND and R. THEMS, Div. of High Energy Physics, DOE

Department of Energy
Washington, D.C. 20545

FEB 1 1983

Dr. [redacted]
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. [redacted]

The Department of Energy's Division of High Energy Physics has completed its review of your proposal, "Theoretical, Experimental, and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons," and has referred this proposal to the Division of Nuclear Physics for final action. We in the Division of Nuclear Physics have examined the proposal and find that the proposed research topics are not appropriate for consideration by the Division of Nuclear Physics. Therefore, we must advise you that we cannot support this research proposal. Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

Enloe Ritter

Enloe T. Ritter
Director
Division of Nuclear Physics

cc:
Div. of High Energy Physics, DOE
R. M. Santilli, I.B.R.

Department of Energy
Washington, D.C. 20545

MAR 2 2 1983

Dr. R. M. Santilli
Institute of Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

We are in receipt of your letter of February 4, 1983 to Dr. Ritter of the Division of Nuclear Physics concerning the proposal "Theoretical, Experimental, and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons" submitted by Dr. [redacted]. The proposed research appears to be appropriate for consideration by the Division of High Energy Physics. With your permission, we are initiating the technical review process. As soon as a decision with respect to support can be reached you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him on (301) 353-4829.

We appreciate your interest in submitting this proposal and will be pleased to give it consideration for support.

Sincerely,

Bernard Feldman

for William A. Wallenmeyer
Director
Division of High Energy Physics

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 8 1983

[redacted]
Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Gentlemen:

I regret to inform you that the National Science Foundation is unable to support your proposal entitled "Theoretical, Experimental, and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons," PHY83-06700.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

As part of a Foundation effort to ensure that all principal investigators better understand the decisions made on their proposals, we are including copies of the reviews received (with identifying information removed).

Sincerely yours,

Rolf M. Sinclair

Rolf M. Sinclair
Acting Director, Division of Physics

Enclosures

IONAL SCIENCE FOUNDATION — PROPOSAL EVALUATION FORM — 15 — NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MPS-8305700	INST FOR BASIC RESEARCH	5/23/83
PRINCIPAL INVESTIGATOR [redacted]	NSF PROGRAM: DIR-MATH & PHYSICAL SCIEN	

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability)

The research proposed here is a theoretical and experimental study of the speculative hypothesis that the electric field of the electron oscillates in time. Experimental study of the C violating decay $^1S_0 \to 3\gamma$ is also proposed.

Testing the oscillating electric field hypothesis might be of interest but the proposal has weak points discussed below.

The hypothesis is completely speculative and has no support from experiment or from current theoretical ideas. Thus it seems unlikely a priori that the hypothesis would be confirmed. In fact, the proposal does not clearly state how the results of the positronium decay experiment would bear on the oscillating field hypothesis.

Santilli's work has emphasized general questions, rather than the more phenomenological work described in the proposal.

The study of applications in macroscopic systems seems unpromising because the hypothetical oscillations produce no known observable effects on the atomic scale.

The institutional capability of the Institute for Basic Research is unknown to this reviewer.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD ✓ FAIR __ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

REVIEWER A

.TIONAL SCIENCE FOUNDATION (PROPOSAL EVALUATION FORM) 19. NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MPS-8306700	INST FOR BASIC RESEARCH	3/23/83
PRINCIPAL INVESTIGATOR	NSF PROGRAM	
[redacted]	DIR-MATH & PHYSICAL SCIEN	

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

This may well be the most marginal research proposal that I have ever been asked to review. Not only are the foundations for the work very speculative but there is no clear statement of the line of investigation to be followed and the results expected from this line of investigation. Most of the proposal consists of papers describing the speculations of the principal investigators each of which states the ideas are only partially worked out and gives promise of more detailed future publication. There is no clear reference to experiments carried out in the past which shed some light on the questions under consideration. This proposal should clearly be rejected in its present form.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR ✓ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

REVIEWER B

ATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FOR(970 1E

NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MPS-8306700	INST FOR BASIC RESEARCH	3/23/83
PRINCIPAL INVESTIGATOR	NSF PROGRAM	
[redacted]	DIR-MATH & PHYSICAL SCIEN	

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capabilit

It is astounding that such nonsense as this can be promulgated as a serious research proposal. The original ludicrous speculation that the charge on the electron varies rapidly and harmonically in time was published in an unrefereed journal (Hadronic Journal). Rather, it was in a journal in which the [redacted]. Two charges interacting will have the same time-variations so as to produce a result in accord with experiment. How this can possibly be compatible with relativity is not made clear. Under no circumstances should precious resources be wasted on such trash.

[redacted]

Worse than Poo

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR __ POO

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requ from Chairpersons of Congressional committees having responsibility for NSI reviewers' comments will be given maximum protection from disclosure.

REVIEWER C

...AL NO.	INSTITUTION	PLEASE RETURN BY
MPS-8306720	INST FOR BASIC RESEARCH	5/23/83
INCIPAL INVESTIGATOR [redacted]	NSF PROGRAM DIR-MATH & PHYSICAL SCIEN	

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

...mments (continue on additional sheet(s) as necessary):
...ality of the proposed research (including budget & institutional capability):

This proposal suggests the theoretical investigation of the hypothesis that the charge on the electron (and other point-like particles) is an oscillating function of time (Eq. 3. p. 4). The viability of the idea is discussed semiclassically and nonrelativistically. Even limiting the discussion in this way I have serious problems with the discussion presented. Furthermore, the most serious questions which occur cannot be addressed in such a restrictive framework.

A few of the specific problems which have occurred to me are listed below. References are to the included paper [redacted]

(1) I think the evaluation of the integrals on page 779-780 is incorrect, and that a correct evaluation of (2-23) will lead to $\delta(\omega_{km} - 2\omega)$ and $\sigma(\omega_{km} + 2\omega)$ terms, thereby leading to gross energy nonconservation in electron-electron Rutherford scattering.

(2) According to condition (2) two separated interacting macroscopic charge distributions have a time independent Coulomb interaction. On page 774 it is claimed that a proton behaves like a macroscopic charge distribution. On the other hand Fig. 1a tells us that the electron charge oscillates with average value zero. I do not see how to combine these ideas in a way which would lead one to conclude that the electron proton interaction is the normal electrostatic one. A vanishing interaction seems a more reasonable consequence.

(3) The relation of the hypothesis to Maxwell's equations is not mentioned, but since photons are referenced there must be some idea of an electromagnetic field implied.

Finally, I find I do not understand the "prima facie" arguments which would suggest that such a hypothesis should be seriously considered. This immediately raises questions about charge conservation, which is inconsistent with the basic hypothesis, and how electrons which are remote from one another but at varying distances manage to keep these charge oscillations in phase so as to lead to a maximal repulsive interaction. Similar remarks would apply to the maximal attractive interaction between an electron and positron.

I conclude that the proposal is inadequately motivated and insufficiently developed to warrant support at this time.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR X POOR

...erbatim but anonymous copies of reviews will be sent only to the principal ...nvestigator/project director. Subject to this NSF policy and applicable ...aws, including the Freedom of Information Act, 5 USC 552, and formal requests ...rom Chairpersons of Congressional committees having responsibility for NSF, ...eviewers' comments will be given maximum protection from disclosure.

REVIEWER D

PHY83-06700
Institute for Basic Research
[redacted]

TYPED FROM HANDWRITTEN REVIEW

"I cannot help but recommend against providing any support for the proposed research.

"To begin with, insofar as the pulsating structure of the Coulomb law is concerned, Santilli's arguments for its existence are devoid of merit. The process $e^+ + e^- \rightarrow 2\gamma$ is explained to extraordinary accuracy by the theory of quantum electrodynamics. Secondly, two numbers appear for the [frequency] of the oscillation. The first is on the order of 7.57×10^{20} sec^{-1} which implies that processes which probe distances on the order of 2×10^{-10} cm should see it. It would seem that this should be felt in experiments which involve energies on the scale of 1 Mev or less. This would imply that X-rays of heavy atoms should be affected in an already observed way. The second number is on the order of 10^{-21} seconds and avoids this difficulty, however Bhabbe scattering experiments at SLAC check the Coulomb law down to distances of 10^{-16} cm or times $\sim 10^{-26}$ sec. Hence there is no reason to believe anything interesting can be seen at the level suggested in this proposal.

"In addition to the problems which I have with respect to the scientific merit of this work, I have examined enough of Santilli's publications to have become convinced that they are of poor quality. I think that any NSF monies spent in further support of work of this caliber will be wasted. As proposed half of the grant (it would seem) will go to support of [redacted] either as a co-investigator, or in the guise of a senior research associate. Given my opinion of his previous work and the low quality of the present proposal I cannot in good conscience countenance such a waste of funds.

"Insofar as institutional capabilities; so far as I know the IBR has none."

Overall rating: "Very Poor"

REVIEWER E

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

June 20, 1983

Dr. R. THEWS
DOE, Division of Physics

RE: Application entitled
"EXPERIMENTAL AND THEORETICAL STUDIES ON THE POSSIBLE PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELESTRONS"
Principal Investigator: [redacted]
FINAL COMMUNICATION

Dear Robert,

I enclose a joint paper with [redacted] regarding the main hypothesis of the application in print at LETTERE NUOVO CIMENTO.

I have contacted several colleagues in this topic and none of them had a truly scientific objection against its plausibility. You should recall that the hypothesis has solid grounds of compatibility with experimental data at the nonrelativistic/quantum mechanical level. As it has been the case for all nonrelativistic advances, a relativistic extension may be found sooner or later.

In particular, the application has been rejected by NSF, as you eventually know. What you should additionally know is that the referee reports are of an incredible mumbo-jambo nature, totally deprived of the most minute scientific content. It is politics brought to unbelievable extremes of antiscientific behaviour. I have absteined from commenting to NSF on their reports. However, I would be delighted to indicated to you their lack of any value whatsoever.

I believe that, for the peer review to be a bit more valuable, applicants should inspect the referee reports and communicate their comments PRIOR to any decision by Governmental Agencies. I do not know whether this procedure can be implemented by DOE, and, whatever the case, I shall respect your decision.

In short, I believe that the hypothesis is too basic and important to be left at the level of mumbo-jambo academic dances. It must be either proved or disproved beyond reasonable doubts. This is the objective of the application: hire a U.S. young physicist to prove or disprove the hypothesis via a genuine scientific process.

Sincerely,



P.S. Some conceivable military applications have been indicated to Dr. Romney of DARPA.

Cc. Drs. Wallenmeyer and Hildebrand.

Department of Energy
Washington, D.C. 20545

JUL 21 1983

Professor [redacted]
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [redacted]

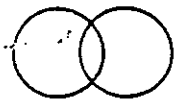
Your proposal entitled "Theoretical, Experimental, and Applied Studies on a Possible Pusating Structure of the Coulomb Force of Individual Electrons" is still under active consideration for funding, and will be acted upon during the next 6-month period.

We hereby request your permission to retain the proposal for this extended period of consideration and shall notify you of our decision regarding support as soon as possible.

Sincerely,

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

cc: Vaccaro & Alkon, CP, CPAS



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

September 20, 1983

Dr. R. L. THEWS
Physics Research Branch
Division of High Energy Physics
Department of Energy
WASHINGTON, D.C. 20545

RE: Application entitled:
"Theoretical, Experimental, and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons"

Principal Investigator: [redacted]

Dear Dr. Thews,

Following your request, we are pleased to authorize the retention of the proposal by your office for any additional period of time.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw



Department of Energy
Washington, D.C. 20545

NOV 15 1983

Dr. Roger Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

As you requested in your letter of November 10, 1983, we are considering as withdrawn your proposals entitled "Theoretical, Experimental and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons" under the Principal Investigator [redacted] and "Studies on the Quantization of Systems with Gauge Symmetries" under the Principal Investigator [redacted]

Your interest in submitting these proposals to the Department of Energy is appreciated.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: [redacted]

PART XXX:

REJECTIONS BY THE NATIONAL SCIENCE FOUNDATION AND THE DEPARTMENT OF ENERGY OF AN I.B.R. APPLICATION BY A SENIOR PHYSICIST

Research Grant Proposal

submitted to the

U. S. DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street
Cambridge, Massachusetts 02138
Tel. (617) 864 9859

entitled

STUDIES ON NONPOTENTIAL SCATTERING THEORY

Proposed Starting Date	Proposed Duration	Amount Requested
March 1983	Two Years	$ 175,300

ENDORSEMENTS

[redacted]
Principal Investigator
The Institute for Basic Research
Cambridge, MA, and
Department of Physics
University of [redacted]
Tel. (617) 864 9859

R. M. SANTILLI
President
The Institute for Basic Research
Cambridge, Massachusetts 02138
Soc. Sec. No. 032 46 3855
Tel. (617) 864 9859

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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BIOGRAPHICAL NOTES BY THE PRINCIPLE INVESTIGATOR, 22

ENCLOSURES:

- [redacted]
- [redacted]
- [redacted]

ABSTRACT

Recent studies by a number of scholars have indicated the possibility that the Hilbert space admits a new generalization, called isotopic, which is structurally more general than available extensions, e.g., of rigged— or C*—type. This implies the possibility of generalizing the various aspects of quantum mechanics into a form capable of representing extended particles under conditions of mutual penetration, and which admit as classical image the Birkhoffian generalization of Hamiltonian mechanics for contact nonpotential interactions.

Along these studies, the Principal Investigator has indicated the possibility of generalizing the conventional potential scattering theory into a form called "nonpotential" primarily for its classical image, while its actual technical structure is that of the isotopic generalization of time evolutions, eigenvalue equations, perturbative expansions, etc. The existence of a corresponding isotopic generalization of the formal, abstract, theory of scattering has also been indicated.

Owing to the well known scientific and administrative relevance of the scattering theory in the data elaboration of high energy experiments, this proposal recommends a comprehensive study of the generalized scattering theory, for the primary purpose of ascertaining whether or not it constitutes a viable alternative to the current data elaboration of experiments implying mutual distances of particles smaller than their size.

The proposed research is articulated into three parts:

- A first part of **foundational character**, for the study of formal aspects;
- A second part of **phenomenological character**, for the study of formalisms ready for applications; and
- A third part of **experimental character**, for applications to the re—elaboration of existing experiments and for comparative analysis with available elaboration via the conventional potential scattering theory.

The research team recommended comprises the Principal Investigator ([redacted]), a U.S. expert in scattering theory to be hired full time as Senior Research Associate, and a number of advisors and/or consultants for experimental, theoretical, and mathematical aspects.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 19, 1982

Dr. W. A. WALLENMEYER, ER–22
Director
Division of High Energy Physics
U. S. Department of Energy GTN
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

I hereby submit for consideration by the Division of High Energy Physics of the U. S. Department of Energy, the research proposal entitled .

Studies on Nonpotential Scattering Theory

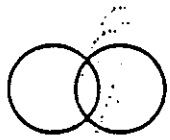
with Professor [redacted] as Principle Investigator. The original, duly signed, proposal is enclosed. The needed amount of additional copies have been separately mailed to you.

As you will notice, this proposal is deeply linked to the proposal currently under consideration by your office entitled "Studies on Hadronic Mechanics". Therefore, I remain at your disposal to mail you additional copies of the latter proposal, in case needed.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 19, 1982

Dr. S. PETER ROSEN
Program Associate
Theoretical Physics Program
Division of Physics
NATIONAL SCIENCE FOUNDATION
1800 G Street
WASHINGTON, D.C. 20550

Dear Dr. Rosen,

I hereby submit for consideration by the Division of Physics of the National Science Foundation, the research proposal entitled,

Studies on Nonpotential Scattering Theory

with Professor [redacted] as Principle Investigator. The original, duly signed, proposal is enclosed. The needed amount of additional copies have been separately mailed to you.

As you will notice, this proposal is deeply linked to the proposal currently under consideration by your office entitled "Studies on Hadronic Mechanics". Therefore, I remain at your disposal to mail you additional copies of the latter proposal, in case needed.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Department of Energy
Washington, D.C. 20545

OCT 2 9 1982

Professor [redacted]
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [redacted]

The research proposal entitled "Studies on Nonpotential Scattering Theory" submitted on your behalf by the Institute for Basic Research has been received in the Division of High Energy Physics.

This proposal is now under review and as soon as a decision with respect to support can be reached you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him on (301) 353-4829.

We appreciate your interest in submitting this proposal and will be pleased to give it consideration for support.

Sincerely,

Early C. Fowler
for

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: Vaccaro & Alkon CP, CPAS

Department of Energy
Washington, D.C. 20545

MAY 12 1983

Professor [redacted]
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [redacted]

Your proposal entitled "Studies of Nonpotential Scattering Theory" is still under active consideration for funding, and will be acted upon during the next 6-month period.

We hereby request your permission to retain the proposal for this extended period of consideration and shall notify you of our decision regarding support as soon as possible.

Sincerely,

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

cc: Vaccaro & Alkon CP, CPAS

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 8 1983

Dr. [redacted]
Department of Physics
96 Prescott Street
Institute for Basic Research
Cambridge, Massachusetts 02138

Dear Dr. [redacted]

I regret to inform you that the National Science Foundation is unable to support your proposal entitled "Studies on Nonpotential Scattering Theory," PHY83-02271.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

As part of a Foundation effort to ensure that all principal investigators better understand the decisions made on their proposals, we are including copies of the reviews received (with identifying information removed).

Sincerely yours,

Rolf M. Sinclair

Rolf M. Sinclair
Acting Director, Division of Physics

Enclosure

Copy to:

Dr. Ruggero M. Santilli
President

NATIONAL SCIENCE FOUNDATION | PROPOSAL EVALUATION FORM | NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY-83[illegible]27	INST FOR BASIC RESEARCH	1/ 6/83

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	THEORETICAL PHYSICS

STUDIES ON NONPOTENTIAL SCATTERING THEORY

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability

I have no confidence in the soundness of the approach to physics taken by this investigator or the institution with which he is associated. An example that leads to such a lack of confidence is the following.

In the second paper attached, equations (3.1), (3.2), and (2.4) lead to $U_-(t,t_o) = [U_+(t_o,t)]^{-1}$ for $t_o \geq t$ if U_+^{-1} exists. If U_+^{-1} does not exist then we do the same with (3.11) and (3.12), which imply [with the initial condition $U_+(t,t) = U_-(t,t) = 1$] that for $t_o \leq t$

$$U_+(t,t_o) = \exp[iH\int_{t_o}^{t} dt'\mu]$$

$$U_-(t_o,t) = \exp[iH\int_{t_o}^{t} dt'\lambda]$$

and hence $U_+(t,t_o)$ must have an inverse for some $t \geq t_o$. But then it follows that $\mu = \lambda$, and there is nothing new.

The works of the Santilli-group and other works by the principal investigator are, in my mind, characterized by the use of mathematical tools without judgment. I have no objection to the use of abstract mathematics in physics when necessary. Here my feeling is that the tools are running away from the physics.

I know that physicists have to be wary of such judgments and it is easy to produce examples in the history of modern physics where similar judgments were in error. Nevertheless one has to use ones best sense and should not be intimidated by these historical precedents into believing that every far-out idea is worth supporting. Perhaps to be far-out in a necessary condition for substantial progress, but it surely is not a sufficient condition.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR ✓ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE FOUNDATION

PROPOSAL 987 EVALUATION FORM

NSF FORM X-3

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY-83-227*	INST FOR BASIC RESEARCH	7/ 6/83
PRINCIPAL INVESTIGATOR [redacted]	NSF PROGRAM THEORETICAL PHYSICS	

STUDIES ON NONPOTENTIAL SCATTERING THEORY

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

This proposal is based on a (trivially) incorrect assumption. The proponent believes that elementary particle interactions are analyzed in terms of an S-matrix derived from a non-relativistic, single-particle Schrödinger equation with a static potential: cf. in particular paragraphs 4 and 6 on page 4 and paragraph 4 on page 5 of the Proposal. (The proponent could have convinced himself of the incorrectness of this assumption by simply consulting any current textbook on elementary particle physics or quantum field theory.)

The proposed research intends to maintain this basic framework (which is known to be incompatible with the special theory of relativity), proposing instead to modify the laws of quantum mechanics, in this referee's opinion, without cogent physical reasons for doing so. To my knowledge, the only experimental evidence claimed to support this scheme (originated by R.M. Santilli) is to be found in a paper by Ktorides et al., Phys. Rev. D22, 892 (1980). In that paper, the authors claim that deviations of the radii of light (!) nuclei from the liquid-drop formula, $R = r_0 A^{1/3}$, arise from a breakdown of the Pauli exclusion principle, rather than from, say, shell structure (which is ignored in that paper).

I also note that all references in the Proposal (with the exception of a book authored by R.M. Santilli) are to be found in Hadronic Journal, edited by R.M. Santilli, who is also involved in this Proposal and is directing the Institute for Basic Research. Although I have no specific reason to doubt the integrity of the refereeing process of Hadronic Journal, the list contained in the Proposal does indicate some reluctance on behalf of the staff of the Institute for Basic Research to submit their work to the criticism of other members of the Physics Community, by publishing in other journals (Phys. Rev., Nucl. Phys., etc.)

I conclude that the proposed research is most likely to be irrelevant from the point of view of the development of particle physics and it should not be funded under any circumstances.

OVERALL RATING: __ EXCELLENT __ VERY GOOD __ GOOD __ FAIR ✓ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

B

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

File

NSF Form 18 (9-81) Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY83-02271	Inst for Basic Research	ASAP

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	Theoretical Physics PP

TITLE

Studies on Nonpotential Scattering Theory

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Quality Dr. Santilli has for a number of years been conducting research in a rather unconventional direction. The present principal investigator is one of his associates. The basis is a generalization of the mathematical structure of nonrelativistic quantum mechanics. It permits breaking of many symmetries and (therefore) of conservation laws. Violation of time reversal invariance is an example. It is essentially fed in by hand (put R≠S). There is at present no established experimental evidence in favor of the proposed generalization.

Since the proposed dynamics is not relativistic I do not understand the proposals claim of relevance for high energy physics.

The proposal also says nothing of the relation (if any) of this work to conventional high energy physics.

The principal investigator has a respectible number of publications. Most of these have been off the main stream such as tachyon theory and his work on Lie-admissable algebras. He has a very wide range of interest, from strong interactions to black holes.

The budget. Item G6 is so large presumably because of the 1 1/2 offices which belong to the private Institute for Basic Research rather than to a university. Item G3 is not clear to me, especially in view of the rather abstract nature of the work and the large arbitrariness that is available for quantitative comparison with experiment.

Summary. The theory seems to me to have too much arbitrariness to be useful: one can fit anything with it and at the same time one loses the symmetries that makes conventional theories beautiful. It is not motivated by experimental evidence, and it has not been shown to be in any way superior to our present theories.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☒ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY83-02271	Inst for Basic Research	ASAP

PRINCIPAL INVESTIGATOR	NSF PROGRAM	
[redacted]	Theoretical Physics	PR

TITLE

Studies on Nonpotential Scattering Theory

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

This proposal provokes a very mixed reaction from the present reviewer. The basic trouble is that in the research program, of which the present proposal is a part, matters which may be physically and mathematically deep and interesting are mixed with matters which are trivially irrelevant. Example: On p. 1320 of the paper Foundations of the Hadronic Generalization of the Atomic Mechanics II Myung and Santilli, one finds the statement "But there are other reasons to suggest a departure from the original idea of Atomic Mechanics. They are given by the clear experimental evidence according to which, in the transition from the two-body problem under electromagnetic interactions to that under strong interactions, there is the disappearance of excited states. In fact, while the hydrogen atom and the positronium admit an infinite variety of excited states, no excited state has been experimentally established until now for the deuteron. The same situation may occur also for other composite particles supposed to be of two-body nature, such as the π^0.

This drastic change in physical behaviour is, perhaps, the most forceful experimental evidence suggesting a revision of the Atomic Mechanics into a form specifically conceived for the strong interactions..."

This statement is really foolish. It has been understood for fifty years that in Schrödinger mechanics short range forces in the two body problem will yield only a finite number of bound states. A precise form of this argument was the core of Wigner's argument in 1932 that the small binding energy of the deuteron implies a short range for the neutron-proton force. Is it on grounds like this that the authors propose we abandon conservation of probability in hadronic mechanics? If so, this reviewer would have to rank the proposal poor.

On the other hand, the research program is making a serious attempt to enlarge the framework of hadronic mechanics and in the course of that work is considering a number of interesting problems. (The reviewer regards the book of Santilli Foundations of Theoretical Mechanics I very useful. In it, the inverse problem of mechanics is studied: which equations of motion are derivable from an action principle?). The attempt to generalize quantization from Hamiltonian to Birkhoffian mechanics is laudable.

(continued on page 2)

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

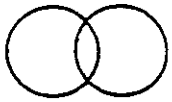
Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

Comments on: 2.

Studies on Nonpotential Scattering Theory
R. Mignani

As far as the specific research program proposed on non-potential scattering theory is concerned, the reviewer again has mixed feelings. The previous work of the proposed principal investigator [redacted] as described in Hadronic Journal [redacted] is not very impressive - a few simple formalities. On the other hand, the research proposal, if carried out successfully, would enable one to confront the very speculative ideas of the general research program with experiment and that is good.

For reasons which should be evident from the above statements this reviewer will not give an overall rating to the proposal. However, in his opinion the very speculative motivations of this research are more likely to be fruitful in yielding greater understanding of more conventional approaches to hadronic mechanics than they are to give a description of Nature.



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

June 20, 1983

Dr. R. THEWS
DOE, Division of Physics

RE: Research grant application
"STUDIES ON THE NONPOTENTIAL GENERALIZATION OF THE SCATTERING THEORY"
Principal Investigator: [redacted]
FINAL COMMUNICATION

Dear Robert,

I would like to confirm the main objective of the application, to hire a young U.S. physicist for the research under [redacted] supervision. Please keep in mind this main objective because referees will likely indicate that I will pocket the money (this was the case of referees at NSF for the same proposal).

Also, a number of developments are going on already in the generalization. They are expected to appear in European Journals. This is due to the known hysterical oppositions at the Journals of the APS on our studies, I reported to Bernie time ago. As the situation now stands, we do not foresee any submission to APS journals for the foreseable future, except when we see the appearance of papers of excessively manifest manipulatory nature, or with massive omissions of references (which have already occurred).

You should not be surprised at this. An entire new mechanics, the Birkhoffian Mechanics, was build without one single paper appearing in APS journals, as documented in a tacit form by scanning the references of my Vol. II with Springer-Verlag. We are having a mere continuation. In fact, we expect the construction, this time, of the hadronic mechanics, without one single paper appearing in APS journals.

This is an aspect that should be identified as clearly as possible, to prevent misjudgments in the processing of the application.

Please keep in mind that the existence and non-triviality of the nonpotential scattering theory is beyond any doubt, as additional material, besides that of the application, can prove. The only debatable aspect is the complyance of the theory with experiments.

This is another important point you should keep in mind. In fact, your referees are likely the same as those of our papers submitted to APS journals, that is, persons whose minds have been deformed by politics beyond the levels of scientific ethics.

In short, we are fully aware of your difficulties in the processing of this (and other) application. For this, you can count on our understanding.

Sincerely,

Ruggero

cc. Drs. Wallenmeyer and Hildebrand.

Department of Energy
Washington, D.C. 20545

OCT 17 1983

Professor [redacted]
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [redacted]

Reference is made to the proposal submitted by the Institute for Basic Research for support of a research program entitled "Studies on Non-potential Scattering Theory" to be conducted under your direction.

We have carefully considered the proposal in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program. Due to the funding limitations which we are currently experiencing, we have found it necessary to decline support of many promising proposals such as yours.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: Dr. R. M. Santilli

PART XXXI:

REJECTION BY THE NATIONAL SCIENCE FOUNDATION OF AN I.B.R. APPLICATION BY A SENIOR APPLIED MATHEMATICIAN

Research Grant Proposal

submitted to the

DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street
Cambridge, Massachusetts 02138
Tel. (617) 864 9859

entitled

STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

Proposed Starting Date	Proposed Duration	Amount Requested
January 1, 1984	Three Years	$ 185,230

ENDORSEMENTS

[redacted]
Principal Investigator
Department of Mathematics
University of [redacted]
Tel. [redacted]

R. M. SANTILLI
President
The Institute for Basic Research
Cambridge, Massachusetts 02138
Tel. (617) 864 9859
Soc. Sec. No. 032 46 3855

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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CURRICULUM OF PRINCIPAL INVESTIGATOR, 12

ENCLOSURES:

1. Table of Contents of [redacted]
2. [redacted]
3. [redacted]
4. [redacted]
5. [redacted]

ABSTRACT

The successes of gauge theories in explaining some phenomena in physics of elementary particles emphasize the importance of an understanding of the quantum nature of gauge fields. The present quantum theories of gauge fields use either the Feynman path integral approach, in which one has a problem of the existence of path integrals, or a gauge condition approach, in which one has a problem of the independence of the resulting theory from the gauge condition used in its formulation. The aim of the proposed project is to study the mathematical problems appearing in attempts to develop an intrinsic, gauge invariant, canonical quantization theory, using as a guideline the geometric quantization theory.

The difficulties with a canonical quantization of gauge theories stem from the fact that gauge invariance leads to constraints given by the vanishing of the generators of gauge transformations. According to P. A. M. Dirac [1950], one should quantize the extended phase space and require that the physical states are gauge invariant. An alternative invariant approach is a quantization of the reduced phase space. In sufficiently regular cases both approaches are possible and yield equivalent results. [V. Guillemin and S. Sternberg, 1982; J. Śniatycki, 1982]. In the case of non—linear gauge fields, the regularity conditions are not satisfied: the constraints have quadratic singularities [J. Arms, J. Marsden and V. Moncrief, 1981], the reduced phase space is not a manifold, and there may be a loss of essential information incurred in reduction [J. Śniatycki, 1982]. In this case, one can generalize the process of reduction leading to the reduced Poisson algebra which need not be the Poisson algebra of a symplectic manifold [J. Śniatycki and A. Weinstein, 1982]. The problem of quantization of reduced Poisson algebras and its equivalence to the quantization of the corresponding generalized phase spaces are to be investigated.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 14, 1983

Dr. W. A. WALLENMEYER, ER–22
DIRECTOR
Division of High Energy Physics
U. S. Department of Energy GTN
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

We hereby submit for consideration by your Division the research grant application entitled,

STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

Principal Investigator: [redacted]

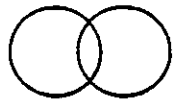
The original, duly signed application, is enclosed. Nine additional copies have been separately mailed to you.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Enclosure



THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 14, 1983

Professor E. F. Infante
Division Director
Mathematical and Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Professor Infante,

We hereby submit for consideration by your Division the research grant application entitled,

STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

Principal Investigator: [redacted].

The original, duly signed application, is enclosed. Nine additional copies have been separately mailed to you.

We trust that you will select the appropriate program within your Division for the consideration of this proposal.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Enclosures

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Division of Mathematical Sciences

DEC 16 1983

Professor [redacted]
Division of Mathematics
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [redacted]

We regret to inform you that the National Science Foundation is unable to support your proposal no. MCS-8317816 for "Study of Quantization of Systems With Gauge Symmetries."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to the other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research supported by the Foundation; and finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

E. F. Infante
Division Director
Division of Mathematical Sciences

cc: Dr. R. M. Santilli
I. B. R. President

Su-Shing Chen
Program Director for Geometric Analysis

PROPOSAL EVALUATION FORM FOR 1000

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8317816	INST FOR BASIC RESEARCH	

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	GEOMETRIC ANALYSIS PROGRM

TITLE

MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

The proposer is a leader in the areas of geometric quantization and classical field theory. The problem of quantization of gauge fields is a very hard one but for which much progress has been made by physicists; many of them work in the Euclideanized framework, using hard analytic tools like the Atiyah-Singer Theory.

On the spacetime side it is not clear what the analogue to these tools is. In any event, it is missing from this proposal (and that of virtually every other worker). In this context, the proposal to investigate the differences between geometric type quantization before and after reduction, the effect of singularities and various ramifications of these ideas is an excellent program. Perhaps the work of Moncrief (Phys. Rev D 18 (1978) 983) would provide a good example here.

The proposal to bring the P.I. to the Boston area for a half year is very good. However, it perhaps is not justified for three consecutive years.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

SCIENCE FOUNDATION

— 1001 —

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
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PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8317816	INST FOR BASIC RESEARCH	NOV 9 1983

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	GEOMETRIC ANALYSIS PROGRM

TITLE

MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

The application of geometric quantization methods to gauge theories is long overdue. [redacted] program seems to me to be very appropriate at this time. Having had long experience and success in both of these areas I think he is highly qualified and should be supported.

OVERALL RATING: ☒ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

...IENCE ...NDATION

PROPOSAL EVALUATION FORM 1002 — NSF Form 1B (9-81) *Supersedes All Previous Editi*

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8317816	INST FOR BASIC RESEARCH	SEP 28 1983

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	GEOMETRIC ANALYSIS PROGRM

TITLE

MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC. CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Professor [redacted] is one of the experts in geometric quantization and its application to quantum mechanics. The proposed research lies in the main stream of activity in this field and is likely to lead to interesting results.

The work should be supported.

OVERALL RATING: ☐ EXCELLENT ☒ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
MCS-8317816	INST FOR BASIC RESEARCH	

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	GEOMETRIC ANALYSIS PROGRM

TITLE

MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

[redacted] is clearly a competent differential geometer, doing interesting, although not terribly exciting research in the theory of symplectic manifolds. [redacted] proposes to study the problem of the quantization of systems with constraints, especially field theories with constraints. Given the importance of gauge theories in modern physics, this is clearly an important subject. [redacted] proposes to study this problem using the method of geometric quantization. I must confess extreme skepticism about the success of such a program, given the fact that there does not seem to be a canonical procedure for choosing the proper polarization to successfully quantize nonlinear systems in situations where everyone agrees what the correct quantization should be. Despite these reservations, I find some merit in the proposal, and were [redacted] a U.S. scientist applying for the usual summer salary grant, I would have made a rating of good, and indicated that I regarded this proposal as in the borderline area, somewhere near the bottom of that area (and therefore, in the current situation, probably just below the cut-off for support).

As it stands, we have a scientist from Canada who proposes to visit an institution in the United States, and asks for 6 months' salary. The amount of money he is asking for is roughly enough to support something like 2.5 of the typical grants for younger scientists. Given the fact that a number of people certainly roughly as good as [redacted] have had grants refused because of the tight budgetary constraints, I regard it as completely wrong to seriously consider funding this proposal.

In summary, on purely scientific judgment, I would rate this proposal as good, but taking into account the amount of funds asked, and other considerations, I would rate this proposal as poor.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

...NDATION — PROPOSAL EVALUATION FORM — NSF Form 1B (9-81) *Supersedes All Previous E*

PROPOSAL NO.	INSTITUTION	PLEASE RETURN
MCS8317816	INST FOR BASIC RESEARCH	

PRINCIPAL INVESTIGATOR	NSF PROGRAM
[redacted]	GEOMETRIC ANALYSIS

TITLE

MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), E
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Typed at NSF from handwritten copy.

Geometric Quantization has generated much mathematical interest in recent times, but unfortunately it has not as yet succeeded in providing much new insight into the problem of quantization of physical theories. My impression is that they have considerable difficulty going beyond the old Bohr-Sommerfeld theory. Nevertheless, it is an interesting avenue of approach and well worth supporting [redacted] has been quite conspicuous in this area. Although this proposal seems quite vague as to what are the new features which might enable him to succeed where so many others have failed, his past performance indicates that much good and interesting work could result. I'm a bit trouble however about the financing of this research. The relationship of [redacted] to I.B.R. is never mentioned; the request is for ½ year salary in each of the successive 3 years. Is [redacted] still in [redacted] or will he be in Boston? In the proposal he refers to research to be conducted by a student - where? I could see supporting this project in the usual 2/9 summer fashion, but I don't find it of such high priority as to require such an expensive crash program. This is the kind of work done best at a university campus with the concomitant educational spinoff.

OVERALL RATING: ☐ EXCELLENT ☒ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PART XXXII:

REJECTION BY THE DEPARTMENT OF ENERGY OF AN APPLICATION BY SANTILLI UNDER THE SMALL BUSINESS INNOVATION RESEARCH ACT.

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Tables of Contents of
R.M.Santilli, Foundations of Theoretical Mechanics,
Volume I (1978) and II (1982), Springer-Verlag, New York/
Heidelberg/Berlin

Curicculum Vitae of Principal Investigator

HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

January 19, 1983

Dr. R. GAJEWSKI,
SBIR Program Manager
U.S.DEPARTMENT OF ENERGY-GTN
WASHINGTON, D.C. 20545

Dear Dr. Gajewski,

Following a kind mailing by Dr. W. WALLENMEYER of the SBIR Program Solicitation, we hereby enclose a Phase I Research Proposal entitled

DEVELOPMENTS AND APPLICATIONS OF BIRKHOFFIAN MECHANICS

which essentially consists of a continuation of research previously conducted under contracts between DOE and Hadronic Press numbers DE-AC02-80ER-10651, A001 and A002.

We would like to respectfully bring to your attention the fact that the current support will be exhausted by mid March 1983. As stated in the application, all research personnel and facilities will be terminated by the Hadronic Press upon such exhaustion of support. Their likelyhood of resumption at some later time is in doubt at this moment. Any possibility of speedy consideration of the proposal for possible continuity of support would be gratefully appreciated.

In the hope of facilitating the review task, I enclose a list of eminent scholars throughout the world who are familiar with the project and who could provide you with a speedy review or refer you to qualified referees (that is, referees with a record of research in non-Lagrangian/non-Hamiltonian mechanics).

The scientific outcome seems to be truly promising and deserving DOE consideration. In fact, the birth of the Birkhoffian generalization of Hamiltonian mechanics (see the enclosure of the application) marks a rather momentuous development of mechanics, with far reaching implications of scientific and military nature, and of classical as well as quantum mechanical character.

Very Truly Yours

Ruggero Maria Santilli
Principal Investigator
RMS-mlw
cc. Drs. W. WALLENMEYER and B. HILDEBRAND, Division of High Energy Physics, DOE

ENCLOSURE: Original contract.
Nine additional copies are mailed separately.

Department of Energy
Washington, D.C. 20545

January 31, 1983

Dr. R. M. Santilli
Principal Investigator
Hadronic Press, Inc.
Post Office Box 7
Nonantum, MA 02195

Dear Dr. Santilli:

Your proposal entitled "Developments and Applications of Birkhoffian Mechanics," has been received in the Small Business Innovation Research Program Office and assigned the number 0011. Please refer to this number in any future communication you may have with the Department concerning your proposal.

Thank you for participating in the Department of Energy's SBIR Program.

Sincerely,

Carolyn Klose

Carolyn Klose
SBIR Program Office

HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

February 2, 1983

Dr. R. Gajewski,
S.B.I.R. Program Manager
Department of Energy
WASHINGTON, D.C. 20545

RE: Application entitled "Developments and Applications of Birkhoffian Mechanics".

Dear Dr. Gajewski,

Soon after submitting the proposal, we realized that the limitation on length included the enclosures. In fact, the indication of "no additional attachments" is only at the end of the paragraph 4.3, while before reference is made only to "no more than 20 pages excluding the budget." My curriculum, being of 19 pages, is then cause of invalidation. If this is indeed the case (which is unclear to us), we would gratefully appreciate the courtesy of considering one of the following two alternatives.

(a) We are hereby authorizing your office to detach and dispose of all enclosures, by reducing the proposal to only the presentation (pages 1 through 15) and the budget. In case this cannot be realized at your office for any reasons,

(b) Kindly remail to us all copies of the proposal (including the original, if necessary,and at your discretion). We shall then detach all enclosureshere and remail the reduced copies to you.

Thanking you in advance for your courtesy and time, we remain

Yours Very Truly

Ruggero M. Santilli
Principla Investigator

RMS-mlw

Department of Energy
Washington, D.C. 20545

March 2, 1983

Dr. R. M. Santilli
Hadronic Press, Inc.
Post Office Box 7
Nonantum, MA 02195

Dear Dr. Santilli:

I am sorry to inform you that your proposal, "Developments and Applications of Birkhoffian Mechanics," has been found deficient in the following respects:

> The cover page of the proposal is not signed by the Principal Investigator and the Corporate/Business Official, and the proposal, including attachments, exceeds 20 pages. (See Sections 7.1 and 4.3 of the Solicitation, DOE/ER-0153.)

Therefore, the proposal must be declined.

The effort you took in preparing and submitting the proposal is very much appreciated.

Sincerely,

Ryszard Gajewski

Ryszard Gajewski
SBIR Program Manager

HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

March 7, 1983

Dr. R. GAJEWSKI
SBIR Director
Department of Energy
WASHINGTON, D.C. 20545

RE: Application entitled
"Development and Applications of Birkhoffian Mechanics", DOE-SBIR NUMBER 0011.

Dear Dr. Gajewski,

Our records disagree with the content of your letter of March 2 declining the consideration of the proposal.

[1] The original proposal was duly signed by the President of Hadronic Press and by myself as principal investigator, and mailed to you via certified mail. The additional 10 copies,as customaries for all copies of all proposals, were not signed. No mention whatsoever of the lack of signature was indicated by Ms. C. Klose of your office in the acknowledgment of the arrival of the proposal dated January 31, 1983 (see enclosed copy).

[2] Soon after submission, we realized that the enclosure of my curriculum would cause invalidation owing to its length (19 pages). We therefore wrote you asking the removal of all enclosures from the proposal and their disposal, or the return of the various copies to us for such removal and subsequent remailing to you (see enclosed copy of our letter dated February 2, 1983). Since we did not hear from you, we evidently assumed that your office had indeed removed all enclosures.

Please do not interpret this letter as a petition for you to reconsider your decision. We merely intended to establish a record on the peculiarities of the case.

Very Truly Yours

R.M.Santilli
Principal Investigator

RMS-mlw
encls.

Department of Energy
Washington, D.C. 20545

March 28, 1983

Dr. R. M. Santilli
Hadronic Press, Inc.
Nonantum, Massachusetts 02195

Dear Dr. Santilli:

Thank you for the information provided in your letter dated March 7, 1983. Based on that information, your proposal "Development and Applications of Birkhoffian Mechanics" (0011) will be processed and evaluated.

Sincerely,

Ryszard Gajewski

Ryszard Gajewski
SBIR Program Manager

Department of Energy
Washington, D.C. 20545

June 30, 1983

[redacted]
President
[redacted]

Ref: SBIR Proposal 0011. "Developments and Applications of Birkhoffian Mechanics"

Dear Mr. [redacted]

We have completed the review of over 1,700 proposals submitted in response to the Department of Energy's Small Business Innovation Research (SBIR) Program Solicitation which ended March 1, 1983, including the referenced proposal submitted by you. Unfortunately, the budget for the program allowed only about one hundred to be funded; regrettably, yours is not among them.

Let me assure you that every proposal was examined thoughtfully. Each was evaluated by scientists or engineers knowledgeable in the subject area of the submittal, with a final review by my office. Realizing how much effort went into the preparation of proposals, and recognizing the value to the nation of the research and development potential that they represent, the Department has proceeded with extraordinary care to come to the most equitable decisions possible.

When all was said and done, hard choices had to be made. We had to eliminate many excellent proposals simply because there were others which were even better. The abundance of submissions compelled particularly close attention to the appropriateness and responsiveness of each proposal to the program requirements and the scope of the technical topics, as defined and described in the Program Solicitation. Confronted with the choice between a high quality proposal that was more closely responsive to a stated topic and one that was less so, we had to opt for the former.

We plan to send out our next SBIR solicitation in December 1983, and you will receive a copy automatically. Should you find in it technical topics under which your firm could submit a proposal, I hope you will consider another submittal.

Sincerely,

Ryszard Gajewski

Ryszard Gajewski
SBIR Program Manager

PART XXXIII:

SUPPRESSION OF THE TESTS OF THE ROTATIONAL SYMMETRY

SECTION A:

DIFFICULTIES AT THE ILL–LABORATORY OF GRENOBLE, FRANCE

INSTITUT MAX VON LAUE - PAUL LANGEVIN

Prof. Dr. R.M. SARTILLI
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

ETATS UNIS

Grenoble, le 22nd October 1981 V/lettre du Notre référence à rappeler : HRF/ep/6824

Re: Discussion of proposals in subcommittee III

Dear Colleague,

Please find enclosed a copy of a letter to Prof. H. Rauch containing the decisions of the subcommittee 'Fundamental and Nuclear Physics'.

Best regards

H.R. FAUST
College Secretary College III

Encl.

INSTITUT MAX VON LAUE - PAUL LANGEVIN

Prof. H. RAUCH
Atominstitut der Österr. Universitäten
Schüttelstr. 115
A-1020 Wien

AUTRICHE

Grenoble, le 21th October 1981 V/lettre du Notre référence à rappeler : HRF/ep/6818

Re: Discussion of proposals in subcommittee III

Dear Colleague,

In the subcommittee meeting 'Fundamental and Nuclear Physics' held on October 14, 1981 your proposal 'Test of SU(2) symmetry breaking due to strong interaction by neutron interferometry' proposal no. 03-13-034, was extensively discussed. Before allocating measuring time, the members of the subcommittee ask for more detailed information concerning the theoretical background of the proposal. Furthermore the following questions have been raised :

- Is there any information on SU(2) breaking from presently known data from other experiments and what are the current limits ?
- Are there any theoretical predictions for the outcome of the proposed experiment ?

The subcommittee decided to postpone the proposal and asks to resubmit it with a new formulation of the problem including the additional information.

With best regards,

H. Faust

H.R. FAUST
College Secretary College III

INSTITUT MAX VON LAUE - PAUL LANGEVIN
B.P. 156 X - 38042 - GRENOBLE CEDEX, FRANCE

DECISIONS OF THE SCIENTIFIC COUNCIL OF OCTOBER 19

(NB. Please inform your co-proposers of this decision).

If you have been allocated beam time you are requested to communicate immediately with Mr. G.A. Briggs, stating any preferential dates other than those given on the proposal form. The provision of neutron beams is extremely expensive and a perturbation of the instrument schedules and the consequent inconvenience to other users caused by non-compliance with this request is no longer acceptable.

RAUCH H
ATOMINSTITUT
SCHUETTELSTRASSE 115
A-1020 WIEN
AUTRICHE

Exp. Number : 3-13- 34 (to be quoted in all replies)

Title : TEST OF SU(2)-SYMMETRY BREAKING DUE TO STRONG INTERACTION BY NEUTRON INTERFEROMETRY

The above proposal has been accepted : NO (SEE BELOW)

Instrument : Beam allocation : days

Instrument : Beam allocation : days

The local contact for this experiment is :

The above proposal has been refused for the following reason :

ADDITIONAL COMMENTS :

SEE LETTER REF. HRF/EP/6818

Atominstitut der Österreichischen Universitäten

Schüttelstraße 115
A-1020 Wien
Tel. (0222) 72 51 36
AUSTRIA

Prof.H.Rauch

for information
I appreciate your
accordant reaction
Yours sincerely

Directors
Institut Laue-Langevin
BP 156X Centre de Tri
F-38042 Grenoble Cedex
France

Wien, 4/11/1981

Dear Directors,

With some astonishment we got the information that our proposals mentioned below are either rejected, or postponed or shortened in the beam allocation by the Scientific Council of the ILL. The proposed experiments represent a mixture of routine investigations where the scientific outcome is rather well assured and some more speculative experiments which can give highlights to basic physical investigations.

Exp. No. 5-16-145 (rejected) "Observation of Triple Laue-Reflection Curves" (Bonse/Rauch). A standard experiment for D18 which should demonstrate a central reflection peak with a width of 0.002 sec of arc with a better background ratio than obtained by an earlier experiment. Information about the lateral spread of the wave packet can be obtained and applications of this central peak effect for special adjustment problems can be envisaged.

Exp. 5-16-144 (rejected) "Investigation of Metal-Hydrogen Systems near the α-β-Phase Boundary" (Rauch/Bonse). Again a typical experiment for D18 where the capacity of the interferometric hydrogen and deuterium determination is demonstrated in an earlier experiment. New samples and a proper thermostat are now available to perform such investigations with an accuracy higher than a factor of 10 compared to conventional methods. Not only the content of hydrogen (deuterium) can be determined but also the amount of free and precipitated hydrogen can be extracted. Three weeks would be enough for this experiment.

Exp. 3-13-34 (postponed) "Test of SU(2)-Symmetry Breaking Due to Strong Interaction by Neutron Interferometry" (Rauch/Santilli). Within a Cambridge/USA - Grenoble - Wien cooperation the 4π-spinor-symmetry experiment of 1975 and 1978 should be repeated in order to observe any nonlinear effect on the symmetry factor caused by the additional strong interaction introduced into the coherent beams of the interferometer. For fundamental physics any observation of a deviation from 4π-symmetry would be a sensation but also a more accurate value for the symmetry factor (at present 715.87 ± 3.8 deg) would be very useful.

Exp. 3-13-36 (shortened from 10 to 2 weeks) "Development of Single Crystal Interferometers for Thermal Neutrons" (Bonse/Rauch). Here I want to mention that at D18 more than 90% of the experiments are still performed using our first interferometer crystal tested successfully 1974 at our TRIGA-reactor in Wien. The ILL should be extremely interested to get new interferometer crystals for D18 not only to replace the old crystal but also to get interferometers with alternative beam paths.

Exp. 3-13-036 (shortened from 16 weeks to 18 days) "Precision Measurements of Coherent Scattering Lengths b_c" (Bonse/Rauch). This proposal represents the routine application of D18 and any element or isotopic value should be remeasured if suitable samples are available.

Our group in Wien is especially concerned about these decision of the Scientific Council because some of the proposals are part of thesis works where most of the preparatory work has been done at our home institute and substantial financial support has been given to these experiments. The students are trained with a comparable interferometer set-up at our small reactor and therefore an effective use of D18 is guaranteed. No additional financial support from ILL is requested. Is there a possibility to present the details and the background information of the proposed experiments to the Subcommittees or to the Scientific Council? In principle we can perform some interferometer measurements at our small research reactors and

we have been invited by some reactor institutes in Europe to install interferometer set-ups with them but we are still of the opinion to continue the Grenoble - Dortmund - Wien cooperation in the field of neutron interferometry, because an effective use of D18 is last but not least also in our interest. We welcome other applicants for D18 but we suggest an effective use joint experiments during the first period as a User Instrument. We are still involved in neutron interferometer projects and on the average we would like to ask for an access to D18 (via proposals etc.) of about 5 weeks per year. Please inform us if this seems to be nonrealistic at all in order to push some alternatives.
I will visit ILL during the week from 23 to 27 November 1981 and would be very grateful to get the opportunity to discuss all related problems with you in detail and hope for a satisfactory solution.

Yours sincerely,

Copy: Prof.Bonse, Dortmund
U.Kischko, ILL

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

October 29, 1981

Office of the President

ProfessorH.R.FAUST
Institut Max von Laue - Paul Langevin
F-34042 Grenoble, France

CERTIFIED AIR MAIL LETTER

Dear Professor Faust,

Please accept the sentiments of my sincere appreciation for the courtesy of mailing to me a copy of the report HRF/ep/6818 addressed to Professor H. RAUCH, Director of the Atominstitut of Wien, Austria, regarding the decision by the subcommittee on 'Fundamental and Nuclear Physics' to postpone the proposal entitled "Test of the SU(2)-symmetry breaking due to strong interactions by neutron interferometry" (number 03-14-034), and to request additional theoretical information. I am taking here the liberty of providing some of the information requested. I would like to stress that I did not have time to consult with Prof. Rauch in Wien (your letter arrived this afternoon). Therefore, I am solely responsible for the contents of this letter.

The theoretical background of the proposal has been discussed extensively at the four yearly WORKSHOPS ON THE LIE-ADMISSIBLE FORMULATIONS held here, first at Harvard (1978, and 1979) and then at our Institute (1980 and 1981). A detailed theoretical study of the theoretical background would call for the reading of the five volumes of the PROCEEDINGS of these Workshops, plus a predictable number of papers and monographs in mathematics and physics.

To semplify the task of your committee, I have separately mailed to you:

[1] A collection of seven selected papers specifically devoted to the experiment (five theoretical papers and two experimental ones) under the title
"Primary bibliography on the problem of the exact or approximate validity of the SU(2)-spin symmetry under strong interactions".
The understanding is that the reading of the seven papers is grossly insufficient for a true understanding of the theoretical studies. In particular, the study of the quoted papers in Lie-admissibility by the mathematicians of our group is an understandable prerequisite.

[2] A copy of my second volume
"Foundations of Theoretical Mechanics, II: Birkhoffian Generalization of the Hamiltonian Mechanics", now in press at Springer-Verlag in the series "Textbooks and Monographs in Physics."
This monograph provides, in my view, a necessary classical background.

[3] A very limited number of additional, recent papers not yet available in print, such as the paper by Mignani (Rome) on the preliminary construction of a nonpotential scattering theory. The parallel experimental paper on the apparent T-violation in nuclear physics is also included.

In addition to the inspection of this literature, I remain at your disposal for any additional element you may need. Simply let me know the information and/or paper desired, and you can count on my best possible assistance (most of the material has been published in the HADRONIC JOURNAL which, I understand, is not widely available in France).

In addition, as a member of the Organization Committee, I am happy to invite you as well as any interested member of your Subcommittee to attend the
FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT
which will be held at the Universite d'Orléans, France, from January 5 to 9, 1981. Copy of the announcement as well as of the registration is enclosed for your convenience.

For your information, the problem of the spin under strong interactions will be an important part of the Conference at its various levels (mathematics, theoretical and experimental physics). Participation will therefore give to the members of the committee the opportunity to talk directly to the originators of the studies.

Permit me here the liberty of touching on some of the interesting issues raised in your letter, in the hope that the ideas presented below may be of some value. I reserve myself to enter into more details, as soon as I have more informations on the reasons which resulted in postponement of an experiment of clearly fundamental physical relevance.

CLASSICAL PROFILE. The first profile which should be taken into account is that, thanks to the contributions by a large number of mathematicians and physicists beginning from the past century, we can today state with confinence that the conventional Hamiltonian/Lie symplectic mechanics has been generalized into a covering form, which, for certain historical reasons, has been called Birkhoffian Mechanics [2]. This mechanics preserves the derivability from an action principle, the Lie algebra character of the time evolution, and th symplectic geometric structure but, the action functional has the most general possible (Pfaffian) integrand; the product of the Lie algebra has the most general possible ((regula realization; and the symplectic structure is the most general possible (exact) two-form.

What your Subcommittee should take into account is that the generalization has been constr cted at all levels of Hamiltonian formulations, ranging from variational principles, to the transformation theory, to symmetries anf first integrals, to the canonical perturbatio theory, etc. Most importantly for this letter, it has been proved in the literature (Sarlet and Cantrijn) that the conventional Hamilton-Jacobi equations admits a consistent generalization of Birkhoffian type. The confrontation of the possible existence of a generalized formulation of quantum mechanics is then inevitable (see below).

The physical applications of the Birkhoffian mechanics are rather forceful. In fact, the insistence of the preservation of the Hamiltonian Mechanics often implies perpetual-motio type of approximation (often tacit), unless properly treated. The Birkhoffian mechanics is directly universal for all Newtonian systems satisfying certain smoothness and regularity conditions. The spinning top, as an example, rather than being studied under the perpetual motion approximation of an exact SO(3) symmetry and conserved angular momentum, can be treated in its more physical realization, that under the presence of a drag torque with consequential nonconservation of the angular momentum, and breaking of the SO(3) symmetry.

In essence, the transition from the trivial Hamiltonian Mechanics to its covering Birkhoff form permits the transition from the description of a system of point-like particles under long range, action-at-a-distance interactions to extended objects under potential forces as well as the most general possible nonpotential (but still local) ones due to internal collisions and any conceivable contact interaction (universality). Most intriquing is the capability of preserving total coservation laws without resulting into perpetual-motio internal approximation. This is the notion of closed nonselfadjoint interactions which is the starting point of the Orléans Conference of 1982. A rather forceful image is given l our Earth when seen by an outside observer as isolated from the rest of the universe. The system verifies all the ten Galilean conservation laws. But, internally, the Galilean symmetry is grossly violated (spinning top with drag torques, Space Shuttle during re-entr in atmosphere; damped oscillators; etc.).

In summary, we can confidently state that a new concept of interactions has been establish at the classical level. Besides existing in Nature, the available theoretical formulations have reached a rather remarkable maturity and sofistication, which I could only grossly touch in my review [2]. Regrettably, these generalized techniques are still known only to a restricted circle of researchers. We hope with the Orléans Conference to propagate a bit the information, of course, to colleagues not solely interested in preserving Hamiltonian Mechanics as the final form of our classical description.

QUANTUM PROFILE. Permit me to stress from the outset that, unlike the classical profile, all the studies at the quantum mechanical level are still of tentative and conjectural character, mathematically and physically. There are, however, a number of points which have transpared rather clearly.

First, the existence of the Birkhoffian generalization of the Hamiltonian Mechanics has reversed the situation. The old criticism was that no generalization of quantum mechanics was possible because of the lack of a consistent classical image. Now the criticism is the opposite one. Until a quantum formulation admitting the Birkhoffian (rather than the Hamiltonian) mechanics as the classical image has not been built, ALL our microscopic descriptions, whether of Heisenberg or other conventional type, remain tentative and conjectural.

To state it differently, the complexity of the Newtonian world was usually bypassed via simplistic point-like assumptions of the elementary constituents, and the regaining of the potential-Hamiltonian-unitary description at the microscopic level. Today the situation is different. Physicists respect the reduction, but jointly ask for explicit studies of theoretical compatibility, that is, the proof that the quantum description of potential type of a large number of point-like particles is truly compatible with an experimentally established nonpotential macroscopic form. Any orthodox physicist who actually sits down and initiates mathematical studies of compatibility will soon discover a host of problems.

In summary, a considerable number of contributions have established that the customary reduction of Newtonian contact nonpotential interactions to potential quantum mechanical ones, even though conceivable, is, first of all, a mere personal conjecture by individual researchers at this time; and, second, that the conjecture is plagued by numerous problems of internal consistency (one can readily see a gross violation of the correspondence principle to begin with).

This situation has stimulated a truly intriguing effort by a growing number of scientists from virtually all over the World to attempt the unthinkable: generalize quantum mechanics into a covering discipline for particle wave packets under long range potential forces, as well as conventional contact nonpotential forces due to mutual wave penetration. Permit me to confess that, without any doubt, to eyewittness the birth of so many and so disparete ideas by so many people, has been for me the most stimulating experience of my research life. But, again, the efforts, in my view, are at the very beginning, and so much remains to be done.

However, thanks to the participation by distinguished mathematians the structure of a conceivable covering mechanics can be identified with a considerable degree of confidence. I am referring to the mathematical structure (a layered generalization of Lie's theory at all its levels/enveloping algebras-Lie algebras-Lie groups/ of Lie-isotopic and of Lie-admissible type, one for the exterior treatment and one for the interior one). The actual construction of the mechanics is still ahead in the future, in my view, even though several specific generalizations exists (of Heisenberg's equation, of Heisenberg's uncertainty principle, of Galilei's relativity, etc.). Needless to say, the most important part will remain the confrontation of the prediction of the theory with experiments.

THEORETICAL PREDICTIONS. In your letter you correctly asked for theoretical predictions. The generalized theory predicts that hadrons, when interpreted as they are in nature (extended charge distributions with a wave packet), exhibit an alteration of their magnetic moment and of their spin (as well as of other Poincaré quantities) when in conditions of mutual penetration with other particles or under very strong fields. No alteration is predicted by the theory when (a) hadrons are treated via point-like abstractions; (b) the mutual distances are large compared to the electromagnetic radius of the particles (atomic structure), or the fields are sufficiently weak; and (c) when the magnetic moment is not altered in the transition from elm interactions to the strong. This is, in essence, my prediction of 1978 [1] via the proposal of breaking the SU(2)-spin symmetry at its enveloping level (replacement of the associative envelope A(SU(2)) with a nonassociative Lie-admissible form).

Since that time, the proposal has been studied by a number of mathematicians and physicists. More recently, Professor G. EDER of the Atominstitut in Wien made fundamental advances in the problem [1]. Most important for this letter is Professor Eder's discovery that, even when the magnitude and third component of the spin are the conventional ones, the SU(2)-spin symmetry can still be grossly broken because of alterations of the space structure of hadrons (under the conditions considered) wich result in non-SU(2) values of the other components as well as of the higher Casimirs.

Permit me to be candid on this important point. I do not know whether the SU(2)-spin symmetry is exact or broken under strong interactions. However, the technical profile is such that I would fear statements of extreme confidence in the exact value. In fact, the studies available, even though grossly incomplete and inconclusive, are sufficient to restrict confidence statements of this type to the level of scientific politics, rather than that of the true pursuit of human knowledge.

OTHER EXPERIMENTAL INFORMATION. Your Subcommittee has also asked the important question whether there are additional experimental informations, independent from Professor Rauch's proposal, which might indicate similar results. The answer is YES, with a number of understandings. First, one must realize that the SU(2)-spin was conceived for the atomic structure and experimentally established under elm interactions. The same notion was then assumed to persist under strong nuclear interactions, but without a direct experimental information until recently. As a result, the SU(2)-spin is simply assumed as valid and used in the data elaboration.

You can rest assured that a considerable effort is under way here at our Institute for the purpose of re-inspecting a number of experiments directly dependent on the value of the spin under strong interactions. Regrettably, a number of papers are written in a way too criptical for a theoretician to reconstruct the data elaboration without the presence of the experimenters. However, a number of them show clear sign of beling along the lines you indicated. Here, I want to indicate at this time only preliminary indications of compatibility of the covering theory (called Hadronic Mechanics) with the experiment by Forte et al (Phys. Rev. lett. 26, 2088 (1980)). You can easily arrive at the same conclusion after a minimal study of the Lie-isotopic and Lie-admissible techniques. In fact, the asymmetry of the experimental results is a typical effect of our Lie-admissible mutation of the charge distribution of the neutron while within the inside of the atoms. At the same time, the orthodox plane wave description, while being unable to reach truly effective quantitative interpretations, is substantially nonconvincing to any physicist who rejects the use of too many constants.

But there are more data to be reinspected. I shall here and in the future abstein myself from presenting the case, again, until I am aware of the reasons resulting in the delay of Professor Rauch's experiments.

PROFESSOR RAUCH'S EXPERIMENT. I assume your Subcommittee is aware of the fact that Professor Rauch experiment of 1978 [1] is the SOLE experiment available until now which measures directly quantities related to spin under strong interactions. I assume you are aware of the fact that the initial reading was 716.8 ± 3.8 deg, and that this reading has been recently revised because of updated constants and other reasons, resulting in the value 715.8 ± 3.8 deg. I assume, finally, that your Subcommittee is aware of the fact that THIS CURRENT EXPERIMENTAL VALUE DOES NOT REPRODUCE THE 720 deg NEEDED TO EXTABLISH THE VALIDITY OF THE SU(2)-SPIN SYMMETRY UNDER STRONG INTERACTIONS ON TRUE SCIENTIFIC GROUNDS (that is, outside scientific politics).

As soon as these points are fully known, the need to repeat the experiment is, in my view, self-evident. After all, which experiment could resist a comparative analysis of value with the fundamental relevance of Professor Rauch experiment?

Permit me to express my position in this respect as clearly as possible.

(1) I support unconditionally Professor Rauch for the repetition of the experiment NO MATTER WHAT THE RESULT IS, that is,whether in favor of against orthodox views. The same support is shared by all the TRUSTEES of our Institute as well as by all members (some of whom are orthodox physicists, and other no).

(2) I believe that the experiment must be repeated first by Professor Rauch and his associatices, and the possibility that other groumps in other parts of the World re-do the experiment before Professor Rauch does should be prevented as much as possible. This is clearly due to our scientific ethics. In fact, the idea, as well as the scientific courage,belong to Professor Rauch. It is very regrettable that your delay in acting on the proposal has substantially increased in my view the possibility that other groups arrive before Professor Rauch. I assume you are fully aware of this, and ready to assume the related scientific responsibility.

(3) Third, and perhaps most importantly, I am in favor of the repetition of the experiment by Professor Rauch in exactly the same way he did it the first time, and with the same data elaboration. To be explicit, I shall object against any alteration of the data elaboration as desired by Professor Rauch. This is clearly necessary to prevent the repetition of the experiment with different data elaboration which might have been conceived to reach compatibility with orthodox views.

But, apart from any scientific reason, permit me to confess as condidly as possible the ultimate reasons of my drive (which, as you eventually know, has resulted in a number of initiatives, ranging from the organization of a new Journal to that of a new Institute of research). I believe that we have an ethical duty to test and establish our beliefs via experiments. We are currently spending truly large public sums in strong interactions. A considerable portion of these sums is spent under the assumption of the validity of the SU(2)-spin symmetry (and other fundamental elm notions) under strong interactions. How long can we procastinate these fundamental tests without forcing a public investigation of scientific accountability? How long we can continue along the current lines without risking a severe judgment by future historians?

In closing, permit to beg you not to interpret this letter as intended to be offensive. If this has been the case, please accept my most sincere apologies. The primary intent of this letter is to be as clear as possible for an important page of physics, with considerable implications of nonscientific character. My intention is that the more clearly the problems are identified, the better for the true pursuit of physical knowledge.

In closing, I also would like to re-iterate that the request by the Subcommittee for additional theoretical information is fully sound, and meets my full approval. However, I fear that an excessively long theoretical study on the possible violation, for an experiment which is predictably expected to reproduce the preservation of the SU(2)-spin may be excessive, and may put the Submittee into dangerous areas such as that of Point (2) above. I mentioned this point in the hope that, perhaps, you will then see the intend of this letter of been useful to you, that is, of providing sufficient knowledge of the various aspects to reach a mature decision.

Hoping to have the pleasure of meeting you at Orléans this coming January, I remain

Yours Very Truly

Ruggero Maria Santilli
Professor of Theoretical Physics

RMS-pm
encls.
cc. Professors H. RAUCH and E. EDER, ATominstitut, Austria
Professors J. FRONTEAU and A. TELLEZ-ARENAS, Univ. of Orleans, France

MAILGRAM SERVICE CENTER
MIDDLETOWN, VA. 22645

— 1028

western union Mailgram

4-0307203318002 11/14/81 ICS IPMHTZZ CSP BSNB
1 6179641684 MGM TDMT NEWTON MA 11-14 0959P EST

RUGGERO MARIA SNANTILLI
28 CROSS ST
WEST NEWTON MA 02165

THIS MAILGRAM IS A CONFIRMATION COPY OF THE FOLLOWING MESSAGE:

TDMT NEWTON MA 11-14 0959P EST
INT DIRECTORS INSTITUTE LAUE-LANGEVIN
CENTRE DE TRI
38042GRENOBLE (FRANCE)
OUR INVESTIGATIONS GRENOBLE AFFAIR REVEALED ELEMENTS POTENTIAL SCIENTIFIC SCANDAL OF INTERNATIONAL PROPORTIONS. SUGGEST URGENT REINSTATEMENT PROFESSOR RAUCH EXPERIMENTS WITH PARTICULAR REFERENCE TO INITIATION EXPERIMENT 3-13-34 ON SPIN SYMMETRY WAS ORIGINALLY SCHEDULED IN JANUARY 1981. FOR BEST INTERESTS OF ALL PARTIES I RECCOMMEND TELEGRAPHIC COMMUNICATION TO ME OF FINAL DECISION IN ORDER TO CONTAIN THE EPISODE AS MUCH AS POSSIBLE.
RUGGERO MARIA SNANTILLI

COL 38042GRENOBLE 3-13-34 1981.

22:03 EST

MGMCOMP

TO REPLY BY MAILGRAM, SEE REVERSE SIDE FOR WESTERN UNION'S TOLL - FREE PHONE NUMBERS

INSTITUT MAX VON LAUE - PAUL LANGEVIN

PROF. DR. T. SPRINGER

Prof. R. M. Santilli
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge
Massachusetts 02138
U.S.A.

Grenoble, le 17 November 1981 V/lettre du Notre référence à rappeler : TS.JMR

Dear Professor Santilli,

I was sorry to learn of your very strong reaction concerning the postponement of the experiment 3.13.34 on spin symmetry. As you are probably aware, decisions to accept or refuse proposals at the ILL are taken in the sub-committees concerned. The difficulty concerning this proposal arose because it is not sufficiently self-explanatory. Sub-committee members are unable to look into original papers quoted in proposals, the number of proposals to be treated in one session of the sub-committee being of the order of one hundred. The information they asked for in particular was :

1) Is there any information on SU(2) breaking from presently known data from other experiments and what are the current limits?

2) Are there any theoretical predictions for the outcome of the proposed experiment?

Consequently the proposal has not been refused but postponed and a re-submission asked for with a new formulation. I will do my best to prevent any delay and have already spoken with the co-author, Prof. Rauch, on the telephone. I will be meeting with him on 24 November to discuss the matter further.

Yours sincerely,

T. SPRINGER
Director

Telex sent out 17/11/81

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

November 30, 1981

Professor Ruggero Maria Santilli, President

Professor T. SPRINGER, Director
Institute Laue-Langevin
F-38042 GRENOBLE, France

Dear Professor Springer,

I would like to confirm our phone conversation of today with particular reference to the following aspects.

(1) The solution most scientifically expeditious would be to leave the original proposal 03-13-034 in full standing and still formally under consideration by your Institute. The additional theoretical information requested by the sub-committee would then be nothing but part of the ordinary process of consideration.

(2) Some of the difficulties in the case have been created by the formal rejection of the proposal by the sub-committee in its current form, with indication of possible resubmission at some eventual future time. Since each of the investigators is an Officer at an Institute of Research, the fulfillment of the possibility calls for the repetition of the administrative iterim to receive new authorization to sign. This, in turn, has a number of evident and predictable implications. The advantage of solution (1) over the current status is clear, in my view. In fact, solution (1) would not need any new authorization of signature, and the consideration of the proposal could be restricted solely to due scientific process.

(3) The requests for theoretical information were answered by me in my letter to Mr. Faust of October 29, 1981 (copy of which is at your disposal), as well as in the collection of seven reprints of articles in the field of the proposal, and in the copy of my Volume II of "Foundations of Theoretical Mechanics" with Springer-Verlag, which were separately air mailed to Mr. Faust (again, additional copies are available on request). In case this information is insufficient, I would appreciate a request of specific technical elements which may be additionally needed.

(4) In case the members of the committees (or one of their representatives) are interested in a serious study of the mathematical, physical, experimental, as well as historical setting underlying the proposal, I seggest participation to the First International Conference on Nonpotential Interactions to be held at the Universite d'Orleans, France, from January 5 to 10, 1982. Copies of the Conference Announcement as well as of the registration form were mailed to Mr. Faust on October 29, and additional copies are available on requests.

(5) You will kindly provide the names of the individual members of the sub-committee as well as of the chairman, so that, in the future, scientific material can be duplicated and mailed directly to them. This would also avoid possible errors in our current information on the matter.

I can be reached at this office until December 21, 1982. The courtesy of a communication prior to that time would be appreciated and, I believe, would be in the best interest of all. From December 22 until January 10 I will be in Europe.

Very Truly Yours

Ruggero Maria Santilli
Co-investigator of former proposal 03-13-034
RMS-vf
cc. Mr. Faust, ILL.

date: Aug. 3, 1981

RESEARCH PROPOSAL

for nuclear physics experiments at the ILL-HFR

dead lines for submission: february 15 and august 31

N° of proposal (to be filled in by ILL)	title:	proposer(s):
3-13-034	Test of SU(2)-symmetry breaking due to strong interaction by neutron interferometry	H.Rauch 1/ R.M.Santilli 2/

exact address of applicant(s):

1/ Prof.H.Rauch
Atominstitut der Österr. Universitäten
Schüttelstrasse 115
A-1020 Wien, Austria

2/ Prof.R.M.Santilli
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138, U.S.A.

telex: ——

tel: 72 51 36 (H.R.)
19-43 222

participants and measuring team:
G.Badurek, H.Rauch, J.Summhammer, S.Hammerschmied

suggested local contact:
U.Kischko
A.K.Freund

IMPORTANT
... to facilitate the mailing of ... concerning this proposal, ... requested to type below ... and full postal address ... person to whom all letters ... sent.

H.Rauch
Atominstitut
Schüttelstrasse 115
A-1020 Wien, Austria

additional information on the proposal:
(to be filled in by the applicant)

keyword n° :

new proposal: ☒ continuation: * ☐ resubmission: ☐

date of report on previous measurements:

n°(s) of previous proposal(s) about same subject:

n°(s) of previous proposal(s) about related subjects:

03 - 05 - 013

* An application for further beam time must be supported by a report on the previous measurements

instruments to be used

☐ Lohengrin PN1 ☐ GAMS PN3 + Pair Spectrometer PN4

☐ BILL PN2 ☐ OSTIS PN6 ☒ Special Neutron Beam (please specify*) D18

* PN5 (UCN), PN7 (Pol. n. beam), H17, H18, IH1, H22D, H22E, H22F

estimated measuring time: 3 weeks

target material: Bi

details of special material or equipment supplied by the user, proposed location and lay-out

A Bi-phase shifter will be introduced within the coherent beams of the neutron interferometer at positions with and without magnetic fields (0.5 kG).

definite starting date:

NOTE
FOR PROPOSALS SUBMITTED IN OCTOBER
1-3 APPLY
FOR PROPOSALS SUBMITTED IN MARCH
4-6 APPLY

safety aspects:

is there any danger associated with the proposed sample or experimental condition?
yes ☐
uncertain ☐
no ☒

if yes what are the risks ?

auxiliary equipment to be supplied by ILL (see guide-lines)

electronics :

mechanical equipment :

others none

data evaluation :

data format required: standard D18

data reduction by whom ? where ? J.Summhammer ILL and Wien

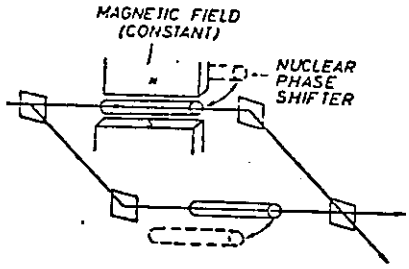
expenses the ILL is expected to cover (see guide-lines)

no separate expenses (experiment financed from Austrian Fonds zur Förderung der wissenschaftlichen Forschung and from

signature of proposer:

scientific background and detailed description of the proposed experiment

Last year's theories have been published which violate the Pauli-principle and the validity of standard quantum mechanics in the region of strong interaction /1,2/. These theories violate also the 4π-symmetry if the spinor rotation occurs at a magnetic field region where strong interaction is present too. Since the first verification of the 4π-symmetry factor has been achieved at SI12 (now D18) in 1975 /3/ and a precision measurement using well defined magnetic fields within Mu-metal sheets has been performed 1978 and yield a periodicity factor of $\alpha_0 = 716.8 \pm 3.8$ deg /4/. The whole magnetic phase shift is given as $\alpha = \gamma m \lambda \int B ds$ (γ ... gyromagnetic ratio, m ... neutron mass, λ ... neutron wave length, B ... magnetic induction, $\int ds$ ... path integral). Using D18 and a measuring procedure as shown in the figure it is possible to reach an accuracy of $\Delta\alpha/\alpha_0 \sim 10^{-4}$ concerning the influence of additional strong interaction introduced by a Bi-phase shifter. The usual nuclear phase shift $X = k(1-n)D$ (k ... wave number, n ... index of refraction, D ... thickness is compensated and a hypothetical dependence $\alpha = \alpha(\lambda)$ can be extracted.



Justification for the performance of the experiment at the ILL and list of references

D18 is optimally suited for this kind of experiment because magnetic and nuclear phase shifts can be applied to the coherent neutron beams. High intensity is needed to reach the required accuracy.

References

/1/ C.N.Ktorides, H.C.Myung, R.M.Santilli; Phys.Rev. D22 (1980) 892

/2/ R.M.Santilli; Hadronic Journal 4 (1981) 1166

/3/ H.Rauch, A.Zeilinger, G.Badurek, A.Wilfing, W.Bauspiess, U.Bonse; Phys.Lett. 54A (1975) 425

/4/ H.Rauch, W.Wilfing, W.Bauspiess, U.Bonse; Z.Physik B29 (1978) 281

INSTITUT MAX VON LAUE - PAUL LANGEVIN

Prof. R.M. Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138
Etats Unis

Grenoble, le 8th December 1981 V/lettre du Notre référence à rappeler : GAB/AT 231

Dear Professor Santilli,

Thank you for your letter of the 30th November 1981.

May I reassure you, that from the point of view of the Institute there is absolutely no problem in re-submitting your proposal 3-13-34 to the Scientific Council. I will of course, in order to conform with the request of this committee, require additional theoretical justification from you and this can be sent to me directly to be attached to the original proposal as an addendum. It would be preferable if your scientific justification was in a condensed form in order to facilitate photocopying for onward transmission to the committee members, but all this can be simply added to the original proposal, as an appendix.

Regarding the names of the nuclear physics sub-committee, these are as follows: Drs. SPECHT, LEROUX, VINH MAU, LYNN, SANDARS and SHOTTER. The chairman is Prof. Dr. SCHULT, Institut für Festkörperforschung der KFA Jülich, Postfach 1913, 5170 JÜLICH, W. Germany and it would be preferable for reasons of protocol if you addressed yourself to Prof. Schult in the first instance.

Any additional scientific material relevant to the proposal should be sent directly to the Scientific Secretariat at the Institute who will ensure distribution.

Yours sincerely,

PROF. T. SPRINGER
Director

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

December 16, 1981

Professor Dr. SCHULT
Institut für Festkörperforschung der KFA Jülich
Postfach 1913, 5170 JÜLICH, West Germany

Dear Professor Schult,

I am contacting you in your quality of Chairman of the Sub-Committee that considered the former application by Professor RAUCH on the repetition at ILL of his important experiment on the spinor symmetry done there in 1978. The primary motivations of the application were clearly indicated in the application it-self, and stem from the fact that an up-dating of the old measures of 1978 DOES NOT produce an angle of precession which is inclusive of the 720 deg needed for the exact SU(2)-spin symmetry. This, in turn, is more in agreement with modern Lie-isotopic and Lie-admissible theories which break and generalize the SU(2)-spin symmetry under strong interactions. The fundamental character of the SU(2)-spin symmetry for the totality of contemporary physics, then renders the repetition of the experiment truly important.

In the hope of preventing misrepresentations, permit me to review the situation to the best of my understanding and knowledge.

I. At your Sub-Committee meeting "Fundamental and Nuclear Physics" held on October 14, 1981, Professor RAUCH's proposal number 03-13-034 was rejected. An informal suggestion to submit a new proposal sometime in the future was conveied. However, as a result of the action taken at the meeting indicated, no formal consideration of Professor RAUCH's proposal is currently active or otherwise pending at ILL.

II, The proposal made by our Institute has been either rejected, or it has not been acted upon. I am referring to the proposal I made by phone to Professor SPRINGER, Director of the ILL, and then confirmed via my letter to him of November 30, 1981, according to which your Sub-Committee should
(a) reconsider the vote of October 14 as soon as possible (see below);
(b) keep proposal 03-13-034 under active, formal consideration by ILL; and
(c) ask for any additional information which may be needed to reach a decision at the appropriate future time.

Permit me to stress that the lack of immediate approval of the application DID NOT constitute a problem. The request of additional theoretical information also DID NOT constitute a problem because it is a routine for most applications. Our difficulties, communicated verbally to Professor SPRINGER, originated from the rejection of the application, as confirmed by the need that we have to prepare a new one (Case I) rather than keeping the application under formal consideration and asking for additional material (Case II).

Permit me to give you an indication of our difficulties, as well as, and perhaps most importantly, of the delicate profile related to our disclosure of your decision to the necessary colleagues.

For me to submit a new application, there is the evident authorization by our Board of Governors who, in turn, acts following the advice of an international body of distinguished scientists scattered throughout the World (our Institute has mainly an international character with minimal in house presence at this time). In order for me to apply for such authorization, I have to report the existing rejection, and be prepared to asnwer predictable requests of explanations.

On the morning of January 5, 1982, I have to deliver my invited opening talk of the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, which, as you know, will be held at the Universite d'Orleans, France. The existing deviations from the 720 deg in Prof. Rauch's experiment may be an indication of the possible existence of a nonpotential component in the nucleons-nuclei interactions. His experiment is therefore important for the Conference, jointly with a number of additional experiments.

Owing to these reasons, it was natural that most of the Organizers and/or the Members of the Advisory Committee of the Conference, had been informed of the application by Professor RAUCH at ILL to repeat the experiment. I am referring here, for instance, to Professor BOGOLUBOV, Director of the Joint Institute for Nuclear Research in Dubna, U.S.S.R., Professor PICOZZA, Deputy-Director of the Italian National Laboratories in Frascati, Italy, Professor BIEDENHARN, now at the Stanford Linear Accelerator Center, U.S.A., Nobel Laureate Professor Prigogine, Director of the Center for Statistical Mechanics of the University of Texas at Austin, U.S.A., and numerous additional distinguished scolars.

It is equally natural to expect that the participants to our First International Conference will ask the status of Professor RAUCH's application to you and to your Sub-Committee at ILL.

In order to prevent completely un-necessary aggravations and impressions, I provided by best effort to submit Proposal II above, but, as I can see from a recent letter from Professor SPRINGER, the proposal has not need acted upon or it has been tacitly rejected. Permit me to ask you directly that Proposal II be considered by you and by your Sub-Committee as soon as possible, and that the answer be communicated to me PRIOR to the inauguration of the Conference on January 5, 1982. In the consideration of the case you should keep in mind that Proposal II would

(1) imply no acceptance whatsoever of the experiment at this time;
(2) prevent my asking a new authorization to sign a new application; and
(3) permit us to communicate at the Orlean International Conference that "Professor Rauch's application for the repetition of his fundamental experiment of 1978 is currently under formal consideration at ILL, and that a decision is expected in the near future" (this communication is precluded at this time).

In case you reject also Proposal II, and insist in the preservation of the original decision (re-submit a new application), it would be recommendable that you give me guidelines for the presentation of the situation at my opening talk.

I shall soon leave for Europe. I have therefore asked my secretary to enclose my schedule until January 5, so that you can reach me in case needed. In case of letters, please use my address at the Conference in Orleans (and exclude other mailing addresses).

You should not expect additional communications on the matter from my part.

Very Truly Yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President
RMS-vf
cc.: Professors SPRINGER, SPECHT, LEROUX, VINH MAU, LYNN, SANDARS, and SHOTTER. FAUST
~~and ILL Scientific Secretariat~~

January 14, 1982

TEST OF SU(2)-SYMMETRY BREAKING DUE TO STRONG INTERACTIONS
Proposal number 03-13-034

Additional information prepared by R.M.Santilli

Basic information

1. The original measure of the spinor symmetry of neutrons at the ILL experiment of 1978 was 716.8 ± 3.8 deg for two spin flips. The measure was therefore inclusive of the 720 deg predicted by the exact SU(2)-spin symmetry.
   Ref.: Rauch et al. Z. Physik B29, 281 (1978)
2. Up-dated physical constants and other reasons have suggested a re-inspection of the 1978 measure. The new value is 715.8 ± 3.8 deg which, as such, it does not include the theoretically expected 720 deg.
   Ref.: IBR publication, October 1981.
3. In addition to the problem of periodicity, there is the problem of the clusters of points clearly outside the sinusoidal behaviour of the intensity modulation predicted by the exact SU(2)-spin symmetry, as well as additional aspects not sufficiently clear at this time (such as the phase of the intensity and polarization modulation).
   Ref.: R.M.Santilli, Hadronic J. 4, 1166 (1981)

It is submitted that this information alone is sufficient to warrant the repetitic of the experiment. The improvement on uncertainty can be identified by Professor Rauch on request.

Theoretical information.

4. The nonconservation of the angular momentum is experimentally established in all open classical, Newtonian or statistical systems (e.g., the spinning top with drag torques, continuous variation of angular momenta in plasma, etc.) This experimental occurrence is an indication of the breaking in nature of the rotational symmetry.
   Ref.: Santilli, "Foundations of Theoretical Mechanics", II (in press), Springe
5. In 1978 the hypothesis of the breaking of the SU(2)-spin symmetry was formulated for the case of open strong systems, e.g., for one neutron in interactio with a nucleus which is considered as external. The hypothesis was based on th idea that the charge distribution of the nuetron is deformed under wave overlapping due to strong fields. The recovering of the exact symmetry for the clo implementation of the system is understood.
   Ref.: Santilli, Hadronic J. 1, 574 (1978) and subsequent Proceedings of Worksh
6. Upon refinements of the original hypothesis, it was predicted that the mutatio (or fluctuation) of the spin of the neutron in the external field of a nucleus is of the order of 1%.
   Ref.: G. Eder, Hadronic J. 4, 2018 (1981)

It is submitted that this provides additional motivation for the repetition of th experiment. In fact, the current number as per paragraph 2 implies a deviation from the exact SU(2)-spin symmetry which is exactly equal to the order of magnitude predicted by the theories of paragraphs 4-6 (called, on mathematical grounds, of Lie-isotopic or Lie-admissible type).

It is stressed that the basic motivation for the repetition of the experiment remains that of paragraphs 1,2, and 3, inasmuch the theoretical studies of paragraphs 4,5, and 6 are tentative at this time.

Speculative information.

7. The violation of the P-symmetry in nuclear physics is established, and so is its origin in the spin component (which is responsible, say, for the optical activity of neutrons in matter).
   Ref.: Forte, Ramsey, et al, Phys. Rev. Letters 26, 2088 (1980)

8. Experimental evidence on the additional violation of the T-symmetry in nuclear physics has been recently achieved by a collaboration Berkeley - Quebec-Bonn. In particular, it has emerged that the most likely origin of the T-breaking is the spin component of the nuclear force, by therefore indicating the possible origin of this additional breaking in the spin.
   Ref.: Slobodrain et al, Phys. Rev. Letters 47, 1803 (1981)

9. Recent unpublished and tentative calculations have indicated that the deviation of the current available measures of paragraph 2 from the exact SU(2)-spin symmetry (1%) are sufficient to interpret the breaking of the T- and P-symmetry, by therefore indicating the possibility that the ultimate origin of the P- and T-violation lies in the SU(2)-spin.

It is submitted that this speculative information provides additional elements of judgments favoring the repetition of the ILL experiment of 1978 by Professor Rauch. The rationale is that the identification of the breaking of the discrete symmetries, even though a fundamental step in physics, is per se insufficient, and calls for the experimental identification of the origin of the breaking themselves. The argument of paragraphs 7,8, and 9 is established in classical, Newtonian and statistical mechanics. For instance, a large collection of rotationally invariant orbits constitutes a reversible system. Thus, the most direct way to reach compatibility with the experimentally established violation of the T-symmetry in statistics is by making sure that at least some of the orbits of the constituents of the system are not rotationally invariant. This latter aspect is requested any how by the experimental evidence of varying angular momenta of parts of the statistical system.

A corresponding knowledge in particle physics is not available at this time. The experiment by Professor Slobodrian has established the origin of the irreversibility at the particle level. The corresponding experiment by Professor Rauch on the spinor symmetry is inconclusive in its available form. Even though theoretically the most probable origin of the T-breaking can be identified with that of the breaking of the rotational symmetry, the corresponding experimental test is lacking.

These aspects were discussed at the recent FIRST INTERNATIONAL CONFERENCE ON NON-POTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT Univ. d'Orléans, France, January 5 to 9, 1982. A qualitative and classical, but majestic illustration was provided by slides of the NASA missions on Saturn. Each and every chunk of ice of Saturn's rings has the rotational symmetry. The rings then also have globally the rotational symmetry. But they constitute a reversible system (all continuous and discrete symmetries are verified in this case). Saturn itself is a more complex system. Globally, it verifies the rotational and discrete symmetry, e.g., for its orbit in the solar system or its intrinsic rotation. However, in the interior of the system, all conventional, continuous and discrete symmetries are broken, as clearly illustrated by the NASA slides. This confirmed that the classical origin of the breaking of the discrete symmetries lies in the continuous ones. Several talks were then presented at the Conference to indicate that the same situation is expected at the particle level following the fundamental results by Professor Slobodrian, not only to achieve compatibility with the discrete breaking, but also to achieve compatibility with the classical, experimentally established situation.
Ref. Proceedings of the First International Conference on Nonpotential Interactions and their Lie-admissible treatment, to be published in 1982.

INSTITUT FÜR KERNPHYSIK
DER KERNFORSCHUNGSANLAGE JÜLICH GmbH
EXPERIMENTELLE KERNPHYSIK II
DIREKTOR: PROF. DR. OTTO SCHULT

D-5170 Jülich, den
Postfach 1913
Telefon (02461) 614408
Telex-Nr.: 833556 kfa d

January 6, 1982

Prof. Dr. R.M. Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138
U S A

Dear Professor Santilli,

I have received your letter dated December 16 only today, and I therefore want to answer immediately.

All members of the Sub Committee 'Fundamental and Nuclear Physics' including myself as chairman follow in their work the basic principle to give the physics that is planned at the ILL the maximum possible support. I can assure you that the attitude of all of us is a very constructive one indeed.

Our work is based essentially on the information given in the proposals. The proposal 'Test of SU(2)-symmetry breaking due to strong interaction by neutron interferometry' by Prof. H. Rauch and Prof. R.M. Santilli (see enclosure) has been discussed on the basis of the brief information that has been given by the authors. It was the feeling of all members of the Sub Committee that more information is required for us to recommend the experiment to be carried out at the ILL. The Sub Committee has therefore postponed its final decision and asked for additional information (see the letter of October 21 to Prof. Rauch).

From your various letters to Prof. Springer and Dr. Faust and your letter of December 16 I conclude that you have great interest in this experiment to be carried out as soon as possible. In this context I am very interested to give you any support and I therefore ask you to please proceed according to the letter of October 21 by Dr. Faust. It will help if you are brief, as I have been in my letter of October 16 (copy is enclosed).

A few remarks on statements that you have made in your last letter are in order:

1. If 'an up-dating of the old measures of 1978 DOES NOT produce an angle of precession which is inclusive of the 720 deg needed for the exact SU(2)-spin symmetry', why has this information then not been given in the proposal dated August 3, 1981 where the figures are 716.8 ± 3.8 which clearly is consistent with 720^{o}?
2. What is the up-dated value and its error?
3. What is the Lie-isotopic and Lie-admissible prediction for SU(2)-spin symmetry breaking? Please give me a number and its uncertainty in case your theory allows the estimation of uncertainties of the theoretical prediction.
4. Your statement that the Sub-Committee has rejected Prof. Rauch's proposal does simply not correspond with the facts. The proposal has been postponed which is explicitly stated in the letter by Dr. Faust. I would like to urge you to read this letter carefully because it is expressis verbis stated 're-submit it (the proposal) with a new formulation of the problem including the additional information'. The Sub-Committee and the college secretary have carefully chosen this way which in fact avoids all the problems that you are dwelling on in the rest of your letter.
5. I vaguely understand that you have formal problems with proposals. Nobody in the Sub Committee is interested in formalities and everybody will definitively give you any support; but please give us if possible briefly and clearly the information we have asked for. Let me stress that this is our basis for a fast decision.
6. I can assure you that neither Prof. Springer nor the Sub Committee will tacilty reject a proposal. I must inform you though that I cannot accept your letter of November 30 as an answer to the questions risen by the Sub Committee. Nor can your discussion with Prof. Springer be considered by the Sub Committee as a new proposal.
7. I am convinced that outstanding scientists will ask at a conference what the new numbers are, rather than about the status of an application.

By the way: I do not care about rumors but about facts. Therefore, I am very much looking forward to your short and clear answer of our questions and I would rather not take your statement 'you should not expect additional communications on the matter from my part' as your final answer.

Yours sincerely,

Otto Schult

O. Schult

Copies to:
Prof. Springer, Director at the ILL
Dr. Faust → Members of the Sub Committee
Prof. Rauch

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

January 14, 1982

Professor Dr. OTTO SCHULT
Institut Für Kerphysik
der Kernforschungsanlage Jülich
Postfach 1913
D-15170 JULICH, West Germany

Dear Professor Schult,

I would like to express my sincere appreciation for your cooperative letter of January 6, 1982. By reading it, I have the impression that the two of us would have resolved all difficulties in a brief and friendly phone back in October 1981. Regrettably, it took me almost three months to know your name [perhaps in the future ILL should indicate the name of the subcommittee members in all communications regarding their consideration of applications-- this would save time and help all].

I enclose the data you recommended that I submit, in their shortest possible presentation, with references in case additional information is needed. In case it is insufficient, please let me know.

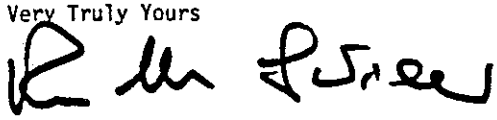
In Orléans, Professor Rauch and I discussed the case, and decided that it is best that, from now on, the ILL application be carried forward by Professor Rauch alone. After all, I am not an experimentalist.

Our Orléans conference was indeed a success (I shall try to let you have a complimentary copy of the Proceedings which are expected to be some three volumes). The climax was reached at the third day of talks, when Professor Slobodrian reported about the clear and firmly stated results of the violation of the T-symmetry in nuclear physics in his experiment with Conzett et al (Phys. Rev. Letters 47, 1803 (1981), Dec. issue). The origin of the breaking in the spin component of the nuclear force was also identified as quite probable. It was a pity that Professor Rauch did not have more recent data on the rotational symmetry (which is at the basis also of the P-violation experiment by Ramsey et al, as you know). This would have permitted a "grand unification of symmetry breakings"... But we hope that this will be possible in the near future.

During my opening talk I avoided, of course, any reference to the ILL case. In private conversations I tried to be as elusive as possible. However, the question when the experiment by Professor Rauch will be repeated was voiced by several participants from numerous Countries. The reasons are clear: we are currently spending truly large amounts of public funds on strong interactions, most of which are crucially dependent on the rotational symmetry. I am sure you will agree that

scientific accountability calls for the experimental resolution one way or another.

Very Truly Yours



Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-vf

cc. Professor Rauch

Professor Springer and Dr. Faust, ILL

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 30, 1982

Professor Dr. Schult
Institut fur Festkorperforschung
Der KFA Julich
Postfach 1913, 5170 JULICH, West Germany

Dear Professor Schult,

I would appreciate the courtesy of an indication concerning the study by your subcommittee on the proposal by Professor Rauch on the test of the spin–symmetry for the strong interactions. Since your letter of January 6, 1982, and my answer of January 14, 1982, I have not heard from you or from the ILL Laboratory in Grenoble.

I would like to take the liberty to enclose a brief paper of mine illustrating the fundamental character of Professor Rauch's proposal, this time in regard to the experiment by Professors Slobodrian and Conzett on the violation of the time–reversal symmetry.

If I can be of any further assistance, please do not hesitate to contact me.

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

Enclosure

RMS/mlw

cc: Professors SPRINGER, SPECHT, LEROUX, VINH MAU, LYNN, SANDARS, SHOTTER, and FAUST

INSTITUT FÜR KERNPHYSIK
DER KERNFORSCHUNGSANLAGE JÜLICH GmbH
EXPERIMENTELLE KERNPHYSIK II
DIREKTOR: PROF. DR. OTTO SCHULT

D-5170 Jülich, den May 12, 1982
Postfach 1913
Telefon (02461) 61 44 08
Telex-Nr.: 833556 kfa d

Prof. Dr. R.M. Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138
U S A

Dear Professor Santilli,

as you can see from the enclosed copy it takes only one week between the recommendation of our Sub-Committee and the letter by Dr. Maier to Prof. Rauch. May I recommend that you speed up your cooperation with Prof. Rauch accordingly? As I am very busy I do everything to get things in order and we really have. It would be nice if you could also follow this example by more intense interaction with Prof. Rauch.

Yours sincerely,

O. Schult

O. Schult

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 22, 1982

Professor O. SCHULT
Jülich, West Germany

Dear Professor Schult,

Your note of May 12 concerning the resolution of your subcommittee on Professor Rauch's spin experiment arrived jointly with a similar one from Professor Rauch.

It was our expectation that your subcommittee would provide our Institute with the courtesy of sending us a copy of this resolution, to save time to all parties. After all, I was a co-investigator and, as you know, we are providing the Nobel Committee with all information relevant for the Slobodrian-Conzett experiment on time-asymmetry, and this includes Professor Rauch's experiment.

Evidence indicate that this expectation of courtesy was erroneous. I guess it is an indication of contemporary academic costume which escapes my comprehension.

Very Truly Yours

Ruggero M. Santilli
President

cc.: Professor Springer ILL Grenoble

INSTITUT MAX VON LAUE – PAUL LANGEVIN

Prof. H. RAUCH
Atominstitut der Österr. Universitäten
Schüttelstr. 115
A-1020 Wien

AUTRICHE

Grenoble, le 8th April 1982 — V/lettre du — Notre référence à rappeler : BM/ep/6978

Re: Discussion of proposals in Subcommittee III

Dear Colleague,

In view of the additional information received concerning your proposal no. 03-13-034, the subcommittee 'Fundamental and Nuclear Physics' recommends to perform the proposed experiment.

With best regards

B. Maier

B. MAIER

PART XXXIII

SUPPRESSION OF THE TESTS OF THE ROTATIONAL SYMMETRY,

SECTION B:

DIFFICULTIES AT THE U.S. NATIONAL SCIENCE FOUNDATION

Research Grant Application

Submitted to the

NATIONAL SCIENCE FOUNDATION

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street

Cambridge, Massachusetts 02138

tel. (617) 864-9859

entitled

EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION

Proposed Starting Date:	Proposed Duration	Amount Requested:
June 1, 1982	12 Months	$ 94,900

ENDORSEMENTS

H. Rauch
Principal Investigator
Atominstitut
Wien, Austria
Tel. (0222) 75 51 36

R. M. Santilli
Co-Investigator
The Institute for Basic Research
Cambridge, Massachusetts USA
Tel. (617) 864-9859

J. Summhammer
Co-Investigator
Atominstitut
Wien, Austria
Tel. (0222) 75 51 36

R.M. Santilli
President
The Institute for Basic Research
Soc. Sec. No. 032 46 3855
Tel. (617) 864-9859

Accounting Firm of the Institute
Vaccaro and Alkon CP, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
Att.: Mr. R. Alkon, President
Tel. (617) 969 6650

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
Att.: Mr. J. Grassia, Senior Partner
Tel. (617) 955 1700

MATH SCI DIVISION
HPR 13 12 34 PM '82

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5. AUSTRIA—FRANCE—USA cost sharing 8

Appendices

A. Information on The Institute for Basic Research

B. Addresses of investigators

C. Experimental Papers

1. H. Rauch, Hadronic J. , 5, (1982), 729

2. H. Rauch, A. Zeilinger, G. Badurek, A. Wilfing, W. Bauspiess, and U. Bonse, Phys. Lett., 54A, (1975), 425

3. G. Badurek, H. Rauch, A. Zeilinger, W. Bauspiess, and U. Bonse, Phys. Rev. D, 14, (1976) 1177

4. H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse, Z. Physik, B29, (1978) 281

5. S. Hammerschmied, H. Rauch, H. Clerc, and U. Kischko, Z. Phys. A, (1981) 302

D. Theoretical Papers

6. C. N. Ktorides, H. C. Myung, and R. M. Santilli, Phys. Rev. D22, (1980), 892

7. G. Eder, Hadronic J., 4, (1981), 2018

8. R. M. Santilli, "Use Of The Hadronic Mechanics For The Best Fit Of The Time—Asymmetry Recently Measured By Slobodrian, Conzett, Et Al", IBR, Preprint April 1982

ABSTRACT

As it has been known for some time, the magnetic moment of neutrons can change within and perhaps even near the region of the strong interactions. The possibility of a corresponding change of the spin of neutrons under strong interactions was pointed out by R.M. Santilli (Hadronic J. 1 (1978), 574), and subsequently studied by several authors. More recently, G. Eder (Hadronic J. 4 (1981), in press) has pointed out possible fluctuations of the spin of the neutrons due to the magnetic field in the neighborhood of the nuclei, which are of the measurable order of one percent. All these effects can be tested most accurately via neutron interferometers, where widely separated coherent neutron beams are available. The most direct and precise test of the SU(2)–spin symmetry for neutrons has been done by H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse (Z. Physik B29 (1978), 281) via the test of the 4π periodicity of the spinorial wave function, yielding the value $\alpha_o = 716.8 \pm 3.8$ deg. Recent corrections due to up–dated physical constants yield the value $\alpha_o = 715.87 \pm 3.8$ deg which does not include the 720 deg expected for the exact SU(2)–spin symmetry. This proposal recommends a joint AUSTRIA–FRANCE–USA collaboration for the repetition of the experiment in such a way to render it most sensitive to the addition of the strong interactions, as well as to the electromagnetic fields in the vicinity of atomic nuclei. This can be achieved via an additional (Bi or Pb) phase shift placed alternatively into the coherent beams of the interferometer at a position with and without magnetic precession fields, as suggested by H. Rauch and A. Zeilinger (Hadronic J. 4 (1981), 1280) and R.M. Santilli (Hadronic J. 4 (1981), 1166). It can be estimated that a relative accuracy of $\Delta\alpha/\alpha_o$ in the range of 10^{-4} can be achieved by this advanced technique. It should be noted that the measure of any deviation from the SU(2)–spin symmetry due to strong interactions and/or other interactions at short range would require a suitable generalization of quantum mechanics, perhaps of the type studied at the yearly *Workshops on Lie–Admissible Formulations* and at the recent *First International Conference on Nonpotential Interactions and their Lie–admissible Treatment* held at the Université d'Orléans, France, from January 5 to 9, 1982, or the inclusion of additional new physical effects.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 27, 1982

Dr. PETER S. ROSEN,
Program Associate for Theoretical Physics
NATIONAL SCIENCE FOUNDATION
Washington, D.C. 20550

Dear Dr. Rosen,

I hereby respectfully submit one original and seven copies of the research grant proposal entitled

EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-U.S.A. COLLABORATION

under administration by our Institute, and with Principal Investigator Prof. H. RAUCH, Director, Atominstitut der Österreischischen Universitaeten, Wien, Austria, who is an undisputed experimental leader in the field of the proposal.

As you can see, the proposal has been made as brief as possible, thanks also to its experimental character. However, we would appreciate your consideration of the advisability whether we should prepare a collection of experimental and theoretical papers in the problem of the proposal, for referee's assistance. Please let us know at your convenience whether such collection of papers should be prepared or not. Also, please keep in mind that the experiment could be initiated this summer, and a solicit resolution would be appreciated, of course, within the time schedule of NSF. I understand that you are in the theoretical division of NSF. In case you pass the proposal to an officer in the experimental part, kindly let me know his name.

Finally, permit me the liberty of recommending, if at all needed, that NSF exercises extreme care with respect to the proved ethical standards of the desired referees. The proposal is for an open problem that is clearly at the foundation of current physical knowledge. Individual referees might therefore be tempted to discourage the conduction of the experiment to protect personal academic-financial interests, to the detriment of the true pursuit of novel human knowledge. At any rate, the proposal reaches your desk after years of documented opposition by a number of physicists who have been trying, whether openly or criptically, to prevent the conduction of this experiment. I believe that it is in the best interest of NSF as well as the international physics community that you are informed of the existence of this opposition so that you can take appropriate precautionary measures. Also, I believe that the proposal and the contemporary scientific climate in the USA warrant a serious consideration of the ethical profile from the outset. Needless to say, we are fully confident that NSF will indeed meet our best expectations.

Sincerely

Ruggero Maria Santilli
President
RMS-mlw
encls.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 8, 1982

Dr. R.M.SINCLAIR
Division of Physics
NATIONAL SCIENCE FOUNDATION
Washington, D.C. 20550

Dear Dr. Sinclair,

We appreciated the courtesy of your recent note of June 29. As you can see from the enclosed copy, we have communicated the situation to Prof. Rauch in Wien in a form which is the best possible for NSF.

I would like to take the opportunity also to enclose copy of a few lines prepared by our members here concerning the possibilities of rather intriguing advances in QCD and all that permitted by the hadronic mechanics. These possibilities are virtually unknown in the community at large. They have not been developed until now pending the availability of a more detailed formulation of the new mechanics. It appears that we are now approaching that point. Within a year or so, some of the possibilities considered here could therefore reach maturity for papers.

You will be amused to know that all these possibilities are rather crucially dependent on the deformations of the charge distribution of hadrons measured by Rauch, as well as by other data, such as the time-asymmetry measured by Slobodrian, Conzett et al, the variation in the space-asymmetry from one nucleus to another measured by Ramsey et al; etc.

The irony is that these preliminary measures of symmetry breakings are generally opposed by physicists in quark theories, thus resulting in a potentially self-damaging posture, as it has been the case a number of times in the past history of physics.

Sincerely,

Ruggero M. Santilli

RMS-mlw

cc.: Drs. M. Bardon and P.S.Rosen, NSF.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

MAY 28 1982

Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I have been asked to consider the proposal you sent to Dr. Peter Rosen of this office. I return it herewith because I feel it is inappropriate for consideration by the Foundation for the following reasons:

1. The main thrust of the proposal seems to be for work done by foreign scientists in a foreign laboratory. The involvement of members of the U.S. scientific community is nowhere made clear. Please refer to the enclosed publication NSF 81-79 ("Grants for Scientific and Engineering Research"), p. 4, which gives our policy on funding work in foreign institutions.

2. The proposal is excessively brief in experimental details and fails to describe what would be done and by whom, and would probably be impossible to have reviewed.

3. Our Division of International Programs advises me that to consider the proposal as an international collaboration under the U.S.-France Program a number of extra steps have to be taken. These are described in the enclosed draft of the guidelines for the U.S.-France Cooperative Science Program. Since there is no corresponding Program with Austria, they cannot consider that aspect of your proposal.

Sincerely yours,

Rolf M. Sinclair

Rolf M. Sinclair
Program Director for Atomic,
Molecular, & Plasma Physics

Enclosures

cc: M. Bardon

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

June 3, 1982

Dr. ROLF M. SINCLAIR
Program Director
Atomic, Molecular, & Plasma Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Sinclair,

I acknowledge receipt of your letter of May 28, communicating to us the decision by NSF not to consider the proposal by Professor Rauch on the fundamental test of the rotational symmetry under strong interactions.

Please rest assured that our Institute accepts this decision with grace and respect. Also, please rest assured that the same acceptance will be provided to all future NSF decisions on research grant applications submitted by our Institute to your office or any other NSF office.

As president of the IBR, one of my functions is to communicate NSF decisions to the principal investigators in a way as smooth as possible. Another function expected from me is to provide NSF with sufficient information to reach all the necessary maturity of judgment.

The comments below are respectfully submitted to you in the hope of achieving these objectives toward both Professor Rauch's team, as well as NSF.

COMMENTS ON PROFESSOR RAUCH'S APPLICATION. Permit me to provide additional information on the application. Regrettably, your office decided to reject the proposal without any prior consultation with us, while a courtesy phone call to us would have rendered this letter unnecessary.

The primary use of the proceeds of the proposal are anticipated for U. S. physicists by specific desire of Professor Rauch as well as of the IBR. Actually, a reason why our Institute supports the proposal is to have U. S. experimentalists and theoreticians trained in the field under the supervision of Professor Rauch, who is an undisputed leader in the field on a world wide basis [see below for the advisability of this training].

The names of the U. S. recipients of possible funds were not indicated because we anticipated consultations with the interested Federal Agency on the specific guidelines for their selection [advertising under the Equal Opportunity Right, etc.].

The budget was not prepared in the NSF forms, and was submitted as received from Europe, precisely to stress its preliminary character, as well as the need of specific guidelines from NSF for its finalization.

Also, Professor Rauch is Director of the Atominstitut der Oesterreichen Universitaeten of Wien, and, as such, he needs no salary from U. S. grants.

In short, we acknowledge that the proposal was incomplete on administrative grounds, and we assume all the responsibility for possible misleading impressions that may have resulted.

On scientific grounds, however, permit me to disagree with your view expressed in point 2 of your letter, to the effect that the proposal is excessively brief and of potentially impossible review. The proposal deals with a fully established exterimental setting, that of neutron interferometry, that is now well known by experts in the field; it identifies the proposed experiments in all necessary technical details; and it includes copies of five experimental papers in the field. The references of these papers consists of the virtual totality of the literature in the field. For these reasons, the proposal is fully sufficient for a technical review by experts.

Needless to say, as it is the case for scientific papers, no research grant application is perfect. Professor Rauch's application can be improved in a number of ways, e.g., by listing additional experiments, or by adding copies of additional papers in the experimental techniques in case a reviewing by non—experts in the field is desired.

COMMENTS ON POSSIBLE IMPLICATIONS FOR NSF. The submission of Professor Rauch's application is clearly the best opportunity to identify the relevance of the underlying physical problem for the general NSF programs in strong interactions. Permit me the liberty of presenting a few comments in this respect, in the hope that they can be of some value for NSF either now or in the future.

Theoretically, the situation is so simple to appear paradoxical. One of the conditions for the exact character of the rotational symmetry for hadrons is that their charge distribution is perfectly spherical. This property is evidently verified under long range electromagnetic interactions, say, for the proton of an hydrogen atom. However, the preservation of a perfectly spherical charge distribution under sufficient impacts due to strong interactions cannot be sustained on true scientific grounds. In fact, the possibility of small deformations for sufficiently intense collisions is rather natural.

The use of the conventional quantum mechanics leads to a perfectly rigid and spherical charge distribution. The use of a covering mechanics currently under

study for the strong interactions [called hadronic mechanics] permits the representation of possible deformations [they are technically achieved via the replacement of the enveloping, associative algebra of operators with more general Lie—admissible forms]. Studies by several theoreticians along this alternative line, particularly those by Professor Eder, predict about 1% deviation from a perfectly spherical charge symmetry for neutrons within the intense fields in the vicinity of nuclei.

Experimentally, the situation is at a considerably advanced stage, although so much remains to be done. In fact, Professor Rauch initiated the experimental test of the rotational symmetry via neutron interferometry back in 1975. Since then, his team has continuously improved the experimental techniques, by repeating the experiments several times through the years [for this reason he is the initiator and undisputed experimental leader in the test]. The point you should be fully aware of is that **the recent measures yields exactly the 1% deviation predicted by Professor Eder et al.**

It should be stressed that these latest measures are still tentative at this time, and in need of verifications, first, by Professor Rauch's team itself, and, second, by independent U. S. experimentalists. Actually, the unsettled character of the available measures is precisely the reason for the application.

Administratively, the situation is quite delicate, and deserving the best consideration by NSF. In fact, NSF is spending large amounts of public funds in strong interactions. A considerable portion of these funds is spent under the belief [by grant recipients and NSF officers] that the rotational symmetry is exact under strong interactions. However, physics is based on experiments. Scientific accountability and lack of discrimination among equally probable scientific views, demand the **experimental resolution** of the exact or only approximate validity of the rotational symmetry. After all, this symmetry is not a minute detail. It is at the foundations of virtually ALL contemporary physical knowledge with self—evident implications at the level of National interests.

The interplay between academic and governmental circles should also deserve the best possible NSF attention. It is public knowledge that the possible experimental detection of a deformation of the charge distribution of hadrons under strong interactions would cause considerable damage to several academicians in various institutions. For this reason, experiments such as that proposed by Professor Rauch, are opposed by organized academic interests, as documented in a variety of cases.

For the orderly condition of our community, it is essential that Federal Agencies such as the NSF, continue to provide evidence of their independence from conceivable academic lobbying toward scientifically discriminatory and administratively unbalanced uses of public funds.

Needless to say, NSF has brilliantly accomplished this duty in the past. I have achieved my objectives if this letter provides you with valuable information for the continuation of this difficult duty in a rapidly changing scientific scene.

After all, the number of physicists now accepting the plausibility of a possible breaking of the rotational symmetry due to deformations of the charge distribution, is increasing considerably in time. Also, the literature directly or indirectly relevant to the problem has reached rather substantial proportions [I am referring to some 10 volumes of proceedings of Workshops and Conferences, plus several research monographs, plus a large number of papers]. Even though this literature continues to be ignored by academicians financially committed to the exact rotational symmetry, it should not be ignored by NSF. In fact, its ignorance could one day prove to be excessively and unnecessarily risky.

THE IBR RECOMMENDATIONS. Upon due consultation with our Board of Governors, as well as colleagues and advisors, permit me to submit the following alternatives for consideration by NSF.

Alternayive A: Confirm the rejection of Professor Rauch's application.
In this case, permit me to ask the courtesy of sending me a new letter of rejection mentioning only the lack of agreement between Austria and the U.S., and absteining from additional remarks, particularly those in point 2 of your current letter [that might be interpreted as being offensive, or, at extreme, even as a manifestation of the desire by NSF not to consider the experiment]. The IBR shall then communicate the rejection to the Atominstitut in Wien via a copy of this modified letter. For reasons communicated verbally to Professor P.S.Rosen of your office, we believe that, in this instance, it is recommendable to proceed in the smoothest possible way. As a gesture of courtesy on our part, we are therefore returning your original letter hereby enclosed.

Alternative B: Accept the consideration of Professor Rauch's application in a modified form.
In this case, we would be most grateful to receive the following guidelines.
(a) Criteria for the identification of the U.S.recipients;
(b) Administrative guidelines for the itemization of funds that can be allocated for the ILL—Laboratory in France under current NSF rules;and
(c) Suggestions for the technical improvements of the presentation vis-a-vis the refereeing process, as well as any other pertinent data.

Alternative C: Recommend the submission of a new applicatjon ON THE SAME EXPERIMENT to be done entirely in the U.S. by U.S. physicists.
In this case, we can provide our best efforts to put together a team comprising U.S. experimentalists in neutron interferometry, as well as theoreticians in favor and against the exact rotational symmetry, in such a way to achieve the best possible diversification of data elaboration and scientific maturity. The administration of this possible new proposal needs not necessarily to be conducted by the IBR, and can be conducted by other Institutions. In this latter case, however, the cost is expected to be higher because of overheads generally higher than those practiced by the IBR. Also, to achieve credibility, alternative Institutions should not have a record of opposition to the experiments and underlying theoretical studies. The emphasis on the same experiment is referred here to the test of the rotational symmetry, but its realization can be different than that suggested by professor Rauch.

Whatever the final decision will be, NSF can count on the best possible collaboration, understanding, and backing from the IBR.

Very Truly Yours

Ruggero Maria Santilli
President and
Chairman of the Board of Governors

RMS-mlw

cc.: Drs. M. Bardon and P.S.Rosen, NSF

encl . Original letter by Dr. Sinclair of May 28, 1982

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

June 23, 1982

Drs. R.M.SINCLAIR, M. BARDON, and P.S.ROSEN
Division of Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Drs. Sinclair, Bardon, and Rosen,

I would like to confirm my recent informal conversation with Peter Rosen, indicating the consideration of Rauch's application by the Division of Nuclear Physics of the DOE.

I would like to confirm also our tentative plans to inform Wien(and Grenoble) that the proposal is under consideration by DOE, and that it is not being considered by NSF

> " as a result of a sound judgment to avoid un-necessary duplications of Governmental efforts."

We believe that this is the smoothest possible way of communicating the NSF decision. Nevertheless, as verbally indicated to Peter Rosen, we would appreciate the courtesy of a final communication from you on the matter [an informal "go ahead" by phone would be o.k. for us].

As a gesture of courtesy on our part, we are enclosing copy of a report prepared for Dr. Ritter at DOE providing a scientific elaboration of the proposal. The open physical issues addressed in this report have an evident administrative relevance. We therefore hope that the information may be valuable to you, and we shall attempt to keep you informed in the future of major developments.

Very Truly Yours

Ruggero Maria Santilli
President

RMS-mlw

encl.

cc. Dr. E.T.RITTER, Div. Nucl. Phys., DOE

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 23 1982

Dr. Ruggero Maria Santilli
President and
Chairman of the Board of Governors
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of June 3, 1982. Please note that the proposal you submitted was not rejected. It was just not possible for us to consider it.

I enclose again our letter of May 28, 1982. The first two reasons given therein indicate why we felt the proposal referred to therein could not be considered by the Foundation. The third states in addition why it did not fit the further requirements of our international programs.

As regards the second of these points - the brevity of the proposal - I refer you to pp. 6-21 of the enclosed booklet NSF 81-79 ("Grants for Scientific and Engineering Research"), which describes the information that should be included in a research proposal to the Foundation. Your inclusion of background material is of course of help to us and to the reviewers. It would still be necessary for us to have a description of the proposed new experiment in appropriate detail, with quantitative assessments where necessary, together with the other material called for, in order to arrange for proper review.

We are not in a position to give advice on which scientists should be involved, nor can we suggest specific technical improvements in a proposal.

I hope these additional remarks are of help to you and your colleagues.

Sincerely yours,

Rolf M. Sinclair

Rolf M. Sinclair
Program Director for Atomic,
Molecular, & Plasma Physics

Enclosures: our letter of May 28, 1982
NSF 81-79

cc: M. Bardon

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 29 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of June 23, 1982 (which crossed with our letter to you of that date).

We are informed by your letter that you have submitted a proposal to DOE that is basically the same as one you had sent earlier to NSF, and thus you do not wish the Foundation to consider your proposal further.

Sincerely yours,

Rolf M. Sinclair
Program Director for Atomic,
Molecular, & Plasma Physics

cc: M. Bardon
S. P. Rosen

PART XXXIII

SUPPRESSION OF THE TESTS OF THE ROTATIONAL SYMMETRY, SECTION C: REJECTION OF AN I.B.R. APPLICATION BY THE U. S. DEPARTMENT OF ENERGY FOR A JOINT AUSTRIA–FRANCE–U.S.A. COLLABORATION

Research Grant Application

Submitted to the

~~NATIONAL SCIENCE FOUNDATION~~
DEPARTMENT OF ENERGY
by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street
Cambridge, Massachusetts 02138
tel. (617) 864-9859

entitled

EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION

Proposed Starting Date:	Proposed Duration	Amount Requested:
June 1, 1982	12 Months	$ 94,900

ENDORSEMENTS

H. Rauch
Principal Investigator
Atominstitut
Wien, Austria
Tel. (0222) 75 51 36

R. M. Santilli
Co-Investigator
The Institute for Basic Research
Cambridge, Massachusetts USA
Tel. (617) 864-9859

J. Summhammer
Co-Investigator
Atominstitut
Wien, Austria
Tel. (0222) 75 51 36

R.M. Santilli
President
The Institute for Basic Research
Soc. Sec. No. 032 46 3855
Tel. (617) 864-9859

Accounting Firm of the Institute
Vaccaro and Alkon CP, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
Att.: Mr. R. Alkon, President
Tel. (617) 969-3600

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
Att.: Mr. J. Grassia, Senior Partner
Tel. (617) 956-1700

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2.	Proposed experiment for the observation of the influence of strong interactions on the validity of the SU(2)—spin symmetry	5
3.	References	6
4.	Budget	7
5.	AUSTRIA—FRANCE—USA cost sharing	8

Appendices

A. Information on The Institute for Basic Research

B. Addresses of investigators

C. Experimental Papers

1. H. Rauch, Hadronic J. , 5, (1982), 729
2. H. Rauch, A. Zeilinger, G. Badurek, A. Wilfing, W. Bauspiess, and U. Bonse, Phys. Lett., 54A, (1975), 425
3. G. Badurek, H. Rauch, A. Zeilinger, W. Bauspiess, and U. Bonse, Phys. Rev. D, **14**, (1976) 1177
4. H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse, Z. Physik, B29, (1978) 281
5. S. Hammerschmied, H. Rauch, H. Clerc, and U. Kischko, Z. Phys. A, (1981) 302

D. Theoretical Papers

6. C. N. Ktorides, H. C. Myung, and R. M. Santilli, Phys. Rev. D22, (1980), 892
7. G. Eder, Hadronic J., **4**, (1981), 2018
8. R. M. Santilli, "Use Of The Hadronic Mechanics For The Best Fit Of The Time—Asymmetry Recently Measured By Slobodrian, Conzett, Et Al", IBR, Preprint April 1982

Some of the primary experimental papers by Professor Rauch and his team, in the topic of the proposal, during the period 1975—1982, are reproduced in the application to DOE. Their listing will not be repeated in this additional material.

The scientific value of these experiments can be better focused by the fact that they constitute the ONLY experiments currently available on the direct measure of spin. In fact, ALL the remaining experiments in strong interactions, both in nuclear physics and high energy physics, ASSUME the exact validity of the spin symmetry in the data elaboration.

BACKGROUND OF THE ILL—LABORATORY. All experiments considered here were conducted by Professor Rauch at the Laue—Langevin Laboratory in Grenoble, France. The proposal recommends the conduction of the proposed experiments also at the ILL—Laboratory.

This is due to a number of scientific and logistic reasons, including the availability at the ILL—reactor of a high flux D18 user set up particularly suited for the proposed experiment.

The ILL—Laboratory is well known and actually used by several U. S. physicists. No additional information is therefore needed here.

THE IDEA OF THE EXPERIMENT. The objective is to achieve a direct experimental test of the SU(2)—spin symmetry under joint strong and electromagnetic interactions. This objective is made feasible by a branch of experimental physics known under the name of neutron interferometry.

In simple terms, a perfect cristal neutron interferometer (see Figure 1) is constituted by a neutron beam subjected to coherent splitting into two branches, and then to a coherent recombination via the use of a perfect cristal. The neutron beam is generally monochromatic, unpolarized, of low energy and of high flux. The perfect cristal is generally a Si cristal with extremely low impurities shaped with three vertical slabs, as indicated in the figure.

An important feature of the set up is the possibility of having a wide angle of separation of the two branches. This feature permits the application of an electromagnet to one (or both) of the branches of the beam for precession (spin flip). Since neutrons are Fermions, a minimum of two complete spin flips (720 deg) are needed to permit the same coherent recombination as that without spin flips. The value of the magnetic field needed to produce two spin flips of the neutrons is 7,496 G. The gap of the electromagnet is generally 1 cm. A typical beam cross section is 2×1.5 mm^2. The cristal wavelength is 1.83 Å.

If both branches of the beam are subjected to spin precession, the intensity of the exiting beam is modulated, as per Figure 2. The periodicity of the modulation is then a measure of the angle α of spin flip. Note that the measure is as direct as experimentally possible, in the sense that the periodicity of the modulation does not require theoretical models in the data elaboration.

The test was first conducted in 1975 under electromagnetic interactions only, and yielded the

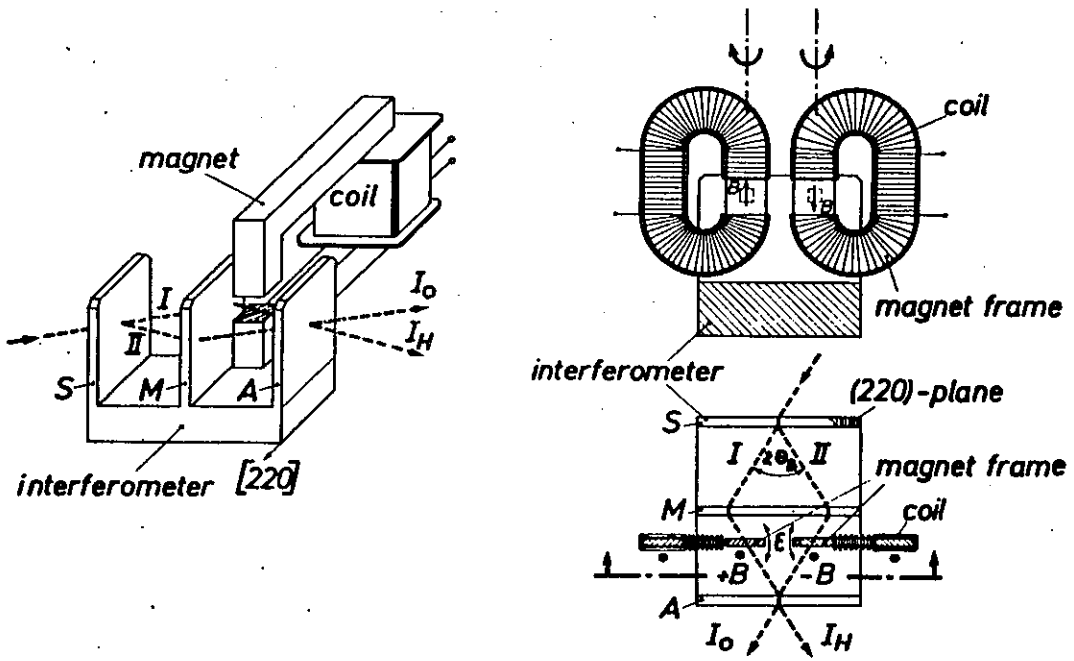

magnet
coil
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[220]
coil
magnet frame
interferometer
(220)-plane
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I
II
magnet frame
M
coil
+B
-B
A
I_o
I_H

FIGURE 1

ABSTRACT

As it has been known for some time, the magnetic moment of neutrons can change within and perhaps even near the region of the strong interactions. The possibility of a corresponding change of the spin of neutrons under strong interactions was pointed out by R.M. Santilli (Hadronic J. 1 (1978), 574), and subsequently studied by several authors. More recently, G. Eder (Hadronic J. 4 (1981), in press) has pointed out possible fluctuations of the spin of the neutrons due to the magnetic field in the neighborhood of the nuclei, which are of the measurable order of one percent. All these effects can be tested most accurately via neutron interferometers, where widely separated coherent neutron beams are available. The most direct and precise test of the SU(2)–spin symmetry for neutrons has been done by H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse (Z. Physik B29 (1978), 281) via the test of the 4π periodicity of the spinorial wave function, yielding the value α_o = 716.8 ± 3.8 deg. Recent corrections due to up–dated physical constants yield the value α_o = 715.87 ± 3.8 deg which does not include the 720 deg expected for the exact SU(2)–spin symmetry. This proposal recommends a joint AUSTRIA–FRANCE–USA collaboration for the repetition of the experiment in such a way to render it most sensitive to the addition of the strong interactions, as well as to the electromagnetic fields in the vicinity of atomic nuclei. This can be achieved via an additional (Bi or Pb) phase shift placed alternatively into the coherent beams of the interferometer at a position with and without magnetic precession fields, as suggested by H. Rauch and A. Zeilinger (Hadronic J. 4 (1981), 1280) and R.M. Santilli (Hadronic J. 4 (1981), 1166). It can be estimated that a relative accuracy of $\Delta\alpha/\alpha_o$ in the range of 10^{-4} can be achieved by this advanced technique. It should be noted that the measure of any deviation from the SU(2)–spin symmetry due to strong interactions and/or other interactions at short range would require a suitable generalization of quantum mechanics, perhaps of the type studied at the yearly *Workshops on Lie–Admissible Formulations* and at the recent *First International Conference on Nonpotential Interactions and their Lie–admissible Treatment* held at the Université d'Orléans, France, from January 5 to 9, 1982, or the inclusion of additional new physical effects.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

June 16, 1982

RE: ADDITIONAL INFORMATION RELATED TO THE RESEARCH GRANT PROPOSAL ENTITLED

"Experimental Verification of the SU(2)–spin Symmetry under Strong and Electromagnetic Interactions via a joint AUSTRIA–FRANCE–U.S.A. Collaboration"

Principal Investigator: Professor H. Rauch
DOE REF. NO. P82206041

Dr. ENLOE T. RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545
Mail Stop ER–23 GTN

Dear Dr. Ritter,

We would like to express our appreciation for your consideration of the proposal by Professor Rauch, as well as for the courtesy of your time and cooperation during our recent phone conversation.

It appears that additional information may be useful for the review of the proposal. I shall therefore attempt to outline in this letter a number of aspects, beginning with *prima facie* motivations, and then passing to conceptual, theoretical, and speculative arguments. Some of the conceivable implications are also discussed for completeness. Finally, a few comments on the budget are appropriate because they are not included in the proposal.

On our part, we do not contemplate the release of additional scientific material for review, unless unexpected novel developments occur. Nevertheless, we shall remain at your disposal to provide any additional assistance you might need, such as copies of the Proceedings of the various Workshops and Conferences relevant for the proposal, copies of individual papers, delivery of informal presentations on the topic at your office or other locations, etc.

BACKGROUND OF PRINCIPAL INVESTIGATOR. Professor H. RAUCH is the Director of the Atominstitut der Osterreischen Universitaeten of Wien, Austra. He is an undisputed leading experimentalist in the field of the proposal. In fact, he initiated the experiment proposed in the DOE application back in 1975. Since that time, his team has repeated the experiment several times, by improving apparatus, operation, and approximation.

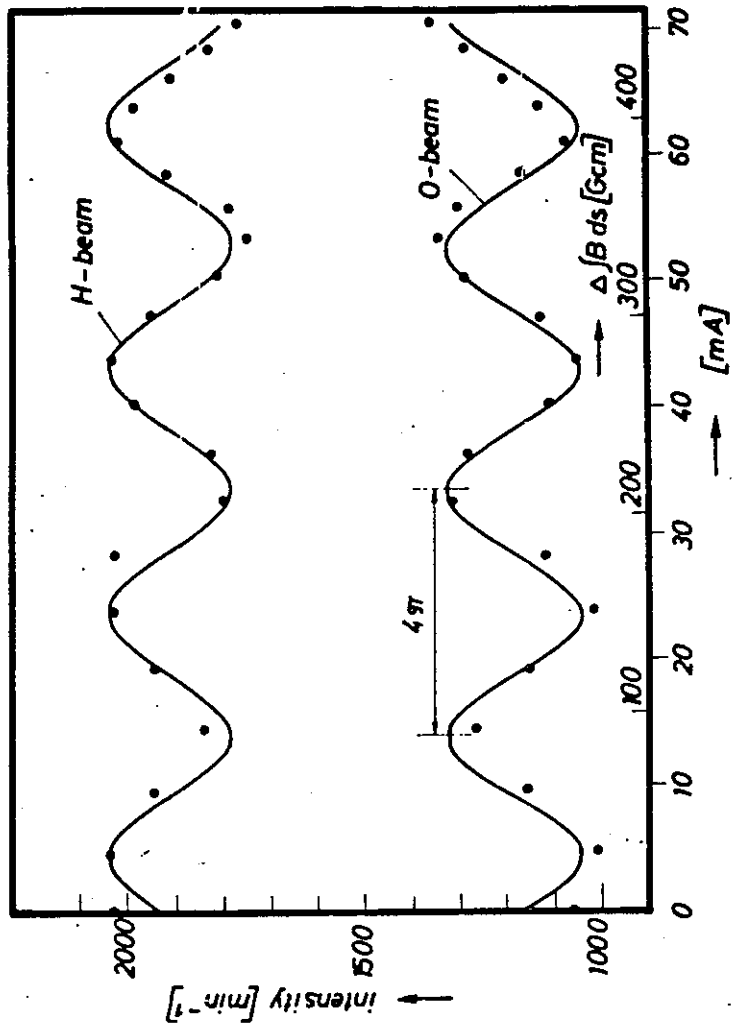

H-beam
O-beam
4 π
intensity [min⁻¹]
2000
1500
1000
Δ ∫B ds [Gcm]
[mA]
0
10
20
30
40
50
60
70
100
200
300
400

FIGURE 2

angle

$$\alpha = 704 \pm 38 \text{ deg} \begin{cases} \text{Max } 742 \text{ deg} \\ \text{Min } 666 \text{ deg} \end{cases} \quad (1)$$

which is inclusive of the 720 deg needed for the exact SU(2)–spin symmetry, as expected [See paper 2 enclosed in the DOE application].

The objective of the proposed experiment is to repeat the test under joint electromagnetic and strong nuclear interactions. This can be achieved in a number of ways. The most direct one is by filling up the electromagnet gap with matter suitably selected to enhance the neutron–nuclei interactions. In fact, the spin flips of the neutrons beam now occur under joint electromagnetic interactions and nuclear interactions due to the matter within the gap.

The strong component can be enhanced in a number of ways [which are contemplated for consideration but not mentioned in the DOE application]. One possible way is to repeat the experiment with a progressive increase of the width of the matter to be penetrated by the neutron beam (say, 3cm, 4cm, and 5cm). Another possibility is to repeat the experiment with an increasing number of spin flips (say, 2, 4, 6, etc.). Other possibilities are offered by different strong interactions in the two branches of the beam. Note that these possibilities could also permit the test of the SU(2)–spin symmetry under strong interactions characterized by linearly varying data of width, spin flips, etc. For a study of these alternatives, see Vol. C of ref. 2 of this letter and refs.[40,41] in particular.

The test can be conducted with high accuracy, typical of neutron interferometers, which is of the order of

$$\frac{\Delta\alpha}{\alpha} = 10^{-4} \quad (2)$$

and which is fully sufficient for the desired objectives (see below). It should be noted that the achievement of the accuracy demands a variety of experimental considerations for: corrections for diamagnetisms, spin–orbit interactions, nuclear polarization; avoiding temporal instability; reducing stray fields; etc.

PRIMA FACIE MOTIVATION FOR THE EXPERIMENT. During the last repetition of the experiment by Rauch's team in 1978, the magnet gap was filled up with Mu metal sheets. This was done only to reduce stray fields. In fact, the experimenters did not have in mind the test of spin under joint strong and nuclear interactions. Their measures resulted in the modulation of Figure 3 with periodicity [See paper 4 enclosed in the DOE application].

$$\alpha = 716.8 \pm 3.8 \text{ deg} \begin{cases} \text{Max } 720.6 \\ \text{Min } 713.0 \end{cases} \quad (3)$$

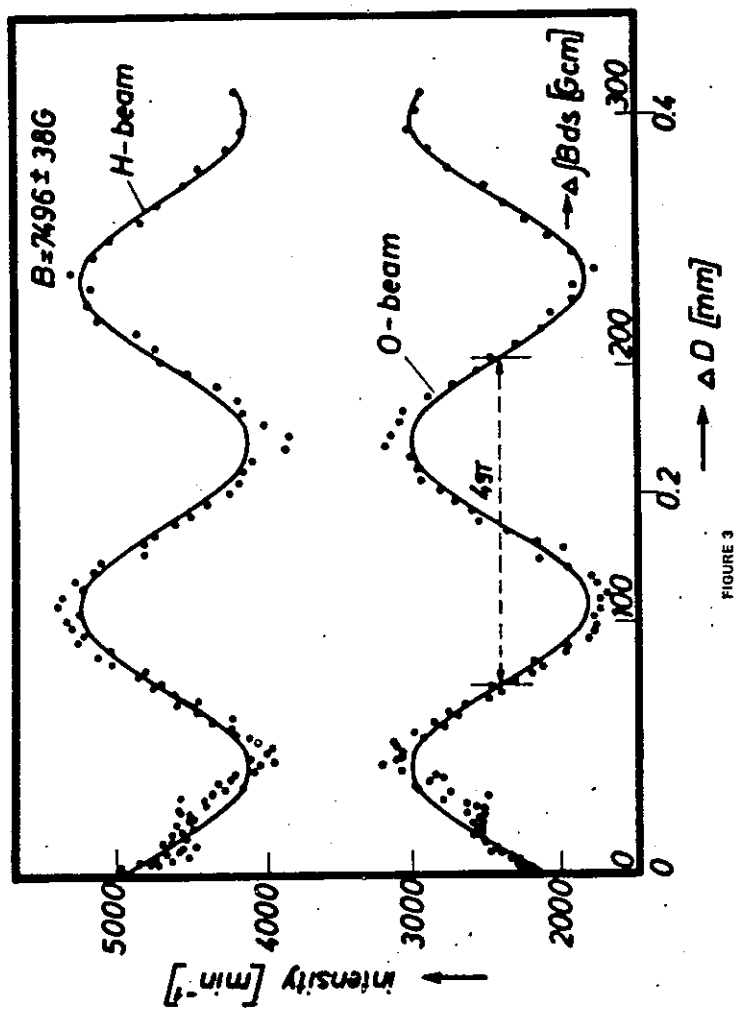

B=7496 ± 38G
H-beam
O-beam
4π
∫Bds [Gcm]
Intensity [min⁻¹]
ΔD [mm]
5000
4000
3000
2000
0
100
200
300
0.2
0.4

FIGURE 3

More recently, the team has reinspected these measures for a number of reasons, including the availability of improved value of nuclear constants. The best measure available at this time is given by [See paper 1 enclosed in the DOE application].

$$\alpha = 715.87 \pm 3.8 \text{ deg} \begin{cases} \text{MAX } 719.67 \\ \text{MIN } 712.07 \end{cases} \qquad (4)$$

This value does not contain the 720 deg needed for the exact character of the SU(2)—spin symmetry under strong interactions. The need to resolve the issue via new experiments is then consequential.

It should be stressed that value (4) is only one of the *prima facie* reasons. At a deeper inspection, a number of additional data are not in agreement with the predictions of the exact rotational symmetry in a scientifically convincing way. For instance, we have clusters of points outside the minimal and maxima predicted by the exact symmetry (see fig. 3); the phase between the intensity and the polarization modulation does not appear to include the predicted 90 deg in a truly clear way; the average values of the modulations of all experiments conducted until now is BELOW 720 deg contrary to statistical expectations [this has been called "angle slow—down effect"[40]]; etc.

Note that these *prima facie* motivations are only those originating from a direct inspection of the experimental data available at this moment. Several additional motivations of theoretical, as well as historical character, exist and will be indicated below.

CONCEPTUAL MOTIVATIONS. As we know well, neutrons and protons are not points. Instead, they are extended objects with a charge radius of the order of 10^{-13} cm, as it is the case for all hadrons.

The extended character of hadrons creates a fundamentally new situation vis—a—vis to SU(2)—spin symmetry. In fact, **a necessary condition for the rotational symmetry to be exact for an (extended) hadron under strong interactions, is that its charge distribution remains perfectly spherical, irrespective of the intensity of the external fields, and of the impact with other hadrons.**

This rigid view is clearly untenable on objective scientific grounds. In fact, the existence in nature of perfectly rigid objects can be advocated by religious arguments similar to those used by the Catholic Church at Galileo's trial, but not by scientific arguments.

It is natural to expect that the charge distribution of hadrons experiences deformations under strong and electromagnetif interactions, depending on the local physical conditions, that is, the intensity of the external field, the energy of the collision with other hadrons, etc. Needless

to say, the deformation is expected to be very small [see below for theoretical predictions].

Stated in words as simple as possible, *the proposed experiment has been conceived to measure possible deformations of the charge distribution of a hadron under sufficient external fields.*

This is the first concept needed for the understanding of the proposal. Second, recall that, by comparison, leptons can be very well approximated as being point–like. The possibility of a deformation of the charge distribution is evidently absent in this case.

Another important concept is that no test whatever is recommended for the rotational symmetry of leptons, as well as, more generally, for all particles that are truly "elementary".

Third, let us note that the possible deformation of the charge distribution of hadrons is expected to exist for *all interactions*, including most importantly the electromagnetic ones. More specifically, consider the physical conditions relevant for the proposed experiments, that is, *neutrons within the intense fields in the vicinity of nuclei*. The deformation of their charge distribution can be expected not only from strong interactions, but also from the electromagnetic ones. It should be stressed that we are referring to the electromagnetic interactions neutron–nuclei and NOT neutron–electromagnet (the latter being too long range and weak to produce measurable deformations).

The first theoretical prediction of a breaking of the rotational symmetry for extended hadrons under strong interactions was done in ref.[11] and thereafter studies in papers.[17,18] The first prediction of the same deformation under short range electromagnetic interactions is that of ref.[19] [reproduced in the DOE application] The cases of the weak and gravitational interactions are under study.

APPARENT INSUFFICIENCIES OF THE "ATOMIC MECHANICS". To my best knowledge, the "Atomic Mechanics" (i.e., the conventional quantum mechanics) does not appear to be capable of representating hadrons experiencing a deformation of their charge distribution.

Conceptually, this can be understood by recalling that the mechanics was conceived for long range electromagnetic interactions of the point–like electrons (hence, the emphasis on its "atomic" character), while the physical situation we are interested in is basically different.

The deficiency can be seen in more detail at the technical level.

The structure of the Atomic Mechanics is entirely set up for the representation of elementary, point–like, structurless, particles. This is the case beginning at the basic axioms of the Hilbert space $\mathcal{H}$ over the field $\mathbb{C}$ of complex number, with: inner product

$$\langle a | a' \rangle = \delta_{aa'} \qquad (5)$$

enveloping associative algebra $\mathcal{OL}$ of local–differential Hermitean operators A, B, C, . . . with conventional product AB

$$\mathcal{OL}: \quad AB = \text{ASSOCIATIVE PRODUCT} \tag{6}$$

attached Lie algebra

$$L: \quad [A,B]_{\mathcal{OL}} = AB - BA \tag{7}$$

Lie group

$$G: \quad e^{\theta X} = 1 + \frac{\theta}{1!} X + \frac{\theta^2}{2!} XX + \cdots \tag{8}$$

etc. This theory can only represent interactions at a collection of isolated points, as evident from the local–differential character of the operators, and several other features.

For the case of neutron interferometers, the predictions of the Atomic Mechanics are based on Pauli's realization of SU(2) spin

$$\vec{S} = \frac{1}{2}\vec{\sigma}, \quad \hbar = 1$$

$$SU(2): \quad (\vec{S})^2 |> = \frac{3}{4} |>, \quad S_3 |> = \pm \frac{1}{2} |> \tag{9}$$

$$[S_i, S_j]_{\mathcal{OL}} = i \varepsilon_{ijk} S_k$$

The spinorial wave function of the neutrons transforms according to the law

$$\psi' = e^{i\chi} e^{-i\sigma_3 \frac{\alpha}{2}} \psi \tag{10}$$

where χ and α are the magnetic and nuclear phase shifts, respectively.

The intensity and polarization modulations for the out-going beam can then be written, after

simple algebra,

$$I' = \psi'^{\dagger}\psi' = \frac{I}{2}\left(1 + \cos\chi \cos\frac{1}{2}\alpha\right)$$

$$\vec{P}'\left(\vec{P} = \vec{0}\right) = \frac{\sin\chi \sin\frac{\alpha}{2}}{1 + \cos\chi\cos\frac{\alpha}{2}} \cdot \frac{\vec{\alpha}}{\alpha} \qquad (11)$$

Note that the angle of modulation α is precisely the angle measured by the experimenters.

The divergencies indicated earlier are apparent deviations from the predictions of formulae (11) for the case of the exact symmetry.

The point–like characterization of the neutron and of its charge distribution in formulae (9) through (11) is evident. Equally evident is then the need to attempt a representation of the neutron closer to physical reality.

THE INTRIGUING PREDICTIONS OF THE "HADRONIC MECHANICS". A comprehensive study of the insufficiencies of the Atomic Mechanics for strong interactions was initiated in 1978 by a coordinated group of scholars comprising mathematicians, theoreticians, and experimentalists. The studies included the organization of four WORKSHOPS ON LIE–ADMISSIBLE FORMULATIONS held in Cambridge–USA from 1978 until 1981, and of the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE–ADMISSIBLE TREATMENT held at the University of Orléans, France, in January 1982.

These studies have resulted in some ten volumes of Proceedings,[1-3] five research monographs,[4-8] and a predictably large number of papers [see the bibliography[9]].

These efforts are devoted to a generalization of the Atomic Mechanics into a covering form called "Hadronic Mechanics", which is capable of representing the *EXTENDED* character of hadrons. The understanding is that the interactions are then given by a combination of the conventional action–at–a–distance/potential/Hamiltonian terms plus new contact/nonpotential/non–Hamiltonian terms [note that points can only interact at a distance, while extended particles have additional contact interactions for which the notion of potential energy has no physical basis].

The realization of the generalized mechanics is permitted by mathematical studies on the existence of two progressive generalizations of Lie's theory called of Lie–isotopic and of Lie–admissible type.[25-30]

The need for such a mathematical generalization is self–evident. The notion of particle used in Atomic Mechanics is technically realized via a Lie group. As a result, no genuine advancement

in the notion of particle is possible without a generalization of the very structure of Lie's theory (Lie group, Lie algebras, and enveloping associative algebras).

The simplest generalization is given by the Lie–isotopy theory. It is characterized by the generalization of the envelope $\mathcal{O}$ of operators A, B, C, . . . into the form

$$\mathcal{O}^*: A * B = ATB \tag{12}$$

where T is a suitable operator (Hermitean, bounded, and positive) fixed for all products. The attached Lie algebra L^* is then characterized by the generalized product

$$L^*: [A, B]_{\mathcal{O}^*} = A * B - B * A = ATB - BTA \tag{13}$$

The underlying generalized group is now given by the expansion in $\mathcal{O}^*$, i.e.,

$$G^*: e^{\theta XT} T^{-1} = 1 + \frac{\theta}{1!}X + \frac{\theta^2}{2!}X*X + \cdots \tag{14}$$

Structures $\mathcal{O}^*$, L^* and G^* are called associative–isotopic envelopes, Lie–isotopic algebras, and Lie–isotopic groups, respectively, because they preserve the original enveloping, algebraic and group character, respectively.

Heisenberg's time evolution of the Atomic Mechanics

$$i\dot{A} = [A, H] = ATH - HTA, \quad T = \hbar^{-1} = 1 \tag{15}$$

is then generalized into the isotopic form proposed in ref.[11]

$$i\dot{A} = [A, H]^* = ATH - HTA, \quad T = T(\vec{r}, \vec{p}, \ldots) \tag{16}$$

As well known, law (15) describes the time evolution of a point–like particle under external action–at–a–distance/potential/Hamiltonian fields (or a collection of such particles). The capability

of the covering law (16) to represent the time evolution of an extended particle can be seen in a number of ways, e.g., from possible integrodifferential realizations of the isotopy operator T, from the fact that the forces CANNOT be reduced all to a Hamiltonian forms (hence, they are also of contact type); etc.

The theory is technically made possible by an underlying isotopic generalization $\mathcal{H}^*$ of the Hilbert space $\mathcal{H}$ [14-16] with inner product

$$\langle a|*|a'\rangle = \langle a|T|a'\rangle = \delta^*_{aa'} = T^{-1}\delta_{aa'} \qquad (17)$$

and with corresponding generalizations of: operators (Hermitean, unitary, antiunitary, etc.); quantum postulates (observability, states, time evolutions, etc.); and numerous other aspects of the conventional Atomic Mechanics. In particular, Planck's unit

$$I = \hbar = 1\,, \qquad IA = AI = A \qquad (18)$$

is generalized into the integrodifferential (left and right) unit operator

$$I^* = \hbar^* = T^{-1}(\vec{r},\vec{p},\dots) \qquad I^* * A = A * I^* = A \qquad (19)$$

which is expected to represent the increased complexity of the energy exchanges for short range interactions among extended particles.

Also, the atomic eigenvalue equation is generalized into the isotopic form

$$H*|\rangle = HT|\rangle = \lambda^* * |\rangle = \lambda|\rangle\,, \quad \lambda^* = I\lambda \qquad (20)$$

which is, structurally, the most general possible one under an *associative* enveloping algebra.

These advances have lead to a generalization of Schrödinger's equations of the type[15,20]

$$i\frac{\partial}{\partial t}\psi(t,\vec{r},\vec{p}) = H(\vec{r},\vec{p}) * \psi(t,\vec{r},\vec{p}) \qquad (21)$$

Under certain realizations of the $\vec{r}$ and $\vec{p}$ operators, eq. (21) has been proved to be equivalent to equations (16), and to admit as a classical limit the Birkhoffian generalization of the Hamiltonian Mechanics.[5] This latter aspect is important to confirm that the underlying theory is not one

for massive points in perpetual–motion conditions.

A review of the theory is impossible in a letter. Therefore, we can indicate here only the essential ideas of the "hadronic spin" used in the data elaboration of the proposed experiment.

Consider a neutron beam under the condition that possible deformations of the charge distribution are ignorable. Under these circumstances, the "atomic spin" (9) is applicable. Suppose now that the same beam enters a region of fields of high intensity, such as when in the vicinity of nuclei. We then assume the hadronic spin characterized by

$$\vec{S}^* = I^* \vec{S} = T^{-1}(\hat{r}, \hat{p}, \dots)\, \vec{S} \qquad (22)$$

Suppose now, in first approximation, that the T–operator can be averaged to a constant

$$\frac{1}{VT}\int dV \int dt\, T = c \approx 1 \left(= \hbar^{-1}\right) \qquad (22)$$

as conceivable for certain nuclear conditions. Then, we have the eigenvalues equations in $\mathcal{H}^*$

$$\left(\vec{S}^*\right)^{2^*} * |> = \tfrac{3}{4}|> \,, \quad S_3 * |> = \pm \tfrac{1}{2}|> \qquad (23)$$

namely, the magnitude and third component of spin are the conventional ones. Nevertheless, a number of new features emerge. First, a study reveals that the remaining two components of spin transform according to a mixture of conventional rotations and deformations[19]

$$U_3 * \vec{S}^* * U_3 = M_3 N_3 \vec{S}^*$$

$$M_3 = \begin{pmatrix} \cos\alpha & -\sin\alpha & 1 \\ \sin\alpha & \cos\alpha & 1 \\ 0 & 0 & 0 \end{pmatrix}, \quad N_3 = \begin{pmatrix} \beta & 0 & 0 \\ 0 & \beta & 0 \\ 0 & 0 & 1 \end{pmatrix} \qquad (24)$$

by therefore confirming the desired objective.

Next, one sees the possibility of representing ANOMALOUS magnetic moments via CONVENTIONAL values of spin (and charge). This is a typical situation of NUCLEAR (AND NOT

ATOMIC) physics, which has essentially escaped true understanding until now.

For applications to a number of other aspects of nuclear physics that have remained obscure via the use of the Atomic Mechanics, we refer the interested reader to the technical literature.[2,3]

What is important for the DOE application is that the isotopic generalization of the Hilbert space, of the quantum postulates, and of the atomic time evolutions, permit a direct interpretation of a number of nuclear phenomena that are apparently outside the capability of the Atomic Mechanics.

The main idea of the data elaboration of the proposed experiment is now predictable, and consists in the generalization of basic law (10) into the corresponding form in $\mathcal{OZ}^*$, i.e.,

$$\Psi' = e^{i\chi} e^{-i G_3 T \frac{\alpha}{2}} I^* \qquad (26)$$

This yields intensity and polarization modulations DIFFERENT than the atomic ones (11). Most importantly, the isotopic term enters directly in the argument of the periodicity of the modulation. Thus, deformations of the charge distribution result into different values of the periodicity, as expected.

A number of explicit forms of the generalized intensity and polarization modulation have been studied, some of them via the still more general Lie–admissible (non–associative) extension of $\mathcal{OZ}$, and others are comtemplated to be investigated if the proposal is funded.

As an example, we quote the generalized laws computed in ref.[40] via a Lie–admissible mutation of $\mathcal{OZ}$

$$I' \cong \frac{I}{2}\left[1 + \varepsilon \cos\chi + (1+\varepsilon)\cos\chi \cos\left((1+\varepsilon)\frac{\alpha}{2}\right)\right] \qquad (27)$$

$$\vec{P}'(\hat{P}=0) \cong \sin\chi \sin\left((1+\varepsilon)\frac{\alpha}{2}\right) \Big/ \left[1 + \varepsilon\cos\chi + (1+\varepsilon)\cos\chi\cos\left((1+\varepsilon)\frac{\alpha}{2}\right)\right]$$

That computed in ref.[19] yields the angle

$$\alpha' = \alpha\left[1 + (1-\varepsilon^2)\alpha^2/12 + \ldots\right] \qquad (28)$$

as one can see in the papers reproduced in the DOE application.

Most importantly, ref.[19] predicts 1% deviation from the perfectly rigid charge distribution for neutrons within the intense fields of nuclei. This prediction is CONFIRMED by best values (4) currently available.

In addition, law (28) predicts an angle α' which is ALWAYS smaller than that of the Atomic Mechanics. This prediction too is confirmed by the average values of all measures conducted until now, as indicated earlier.

A number of additional predictions of the Hadronic Mechanics that are confirmed by available measures, calls for an in depth technical knowledge of the field, and cannot be indicated here in a meaningful way.

To summarize, the application submitted to DOE with Professor Rauch as Principal Investigator recommends the conduction of an experiment for the future resolution of the following different predictions in the spin behaviour of a neutron beams under certain physical conditions identified in the proposal:

1. *The Atomic Mechanics predicts two complete spin flips for a total of 720 deg; while*

2. *The Hadronic Mechanics predicts a smaller rotation of the order of 710 deg.*

The available best measures do not include 720 deg and favor the prediction of 710 deg. The resolution of the difference (of about 1%) is well within current experimental capabilities in neutron interferometry, with the understanding that experimental results under this proposal must be subjected to additional independent verifications [see last part of this letter].

The differences in prediction can be conceptually reduced to the fact that:

1'. *The Atomic Mechanics represents neutrons as massive, structurless points. Under these assumptions, the rotational symmetry CANNOT be broken; while*

2'. *The Hadronic Mechanics represents neutrons as extended charge distributions. Under these conditions, the spherical charge distribution can experience small deformations under sufficiently intense external fields, with consequential small rotational—asymmetry.*

The quantitative treatment of the different predictions is made possible by the underlying mathematical structures of the theories, that is:

1''. *The Atomic Mechanics is based on the conventional Lie theory realized via operators on a conventional Hilbert space; while*

2''. *The Hadronic Mechanics is based on a generalization of Lie's theory realized via operators on a generalized formulation of Hilbert spaces.*

It should be kept in mind that:

1" '. The classical image of the Atomic Mechanics is given by the Hamiltonian Mechanics for massive points under perpetual–motion conditions; while

2" '. The classical image of the Hadronic Mechanics is given by the Birkhoffian Mechanics for extended systems under superpositions of action–at–a–distance/potential and contact/nonpotential forces.

Also, it may have some value to know that the DOE application under consideration has been submitted following studies conducted over the period 1978–1982 under DOE support by a coordinated group of mathematicians, theoreticians, and experimentalists.

Finally, the DOE application under consideration is the EXPERIMENTAL PART of a comprehensive research program submitted to DOE and including

- *A THEORETICAL PROPOSAL by a group of physicists for a coordinated study of the Hadronic generalization of the Atomic Mechanics; as well as*
- *A MATHEMATICAL PROPOSAL by a group of mathematicians for a coordinated study of the mathematical structure underlying the physical theories.*

Copies of these additional proposals are available on request.

POSSIBLE IMPLICATIONS OF THE PROPOSED EXPERIMENT. The rotational symmetry is not an aspect of secondary physical significance. In fact, it is at the foundation of virtually ALL contemporary knowledge in particle dynamics. A study of the proposal by Professor Rauch without the consideration of its possible implications, would therefore be grossly deficient. By the same token, this letter too would be deficient without at least touching some of the possible implications.

The first predictable implications are those related to Galilei's relativity and Einstein's special relativity. The rudimentary review made in this letter is sufficient to indicate the inapplicability of these relativities to the Hadronic Mechanics, and the need for their suitable generalization.

In fact, these conventional relativities are realized via (unitary) Lie groups acting in the Hilbert space $\mathcal{H}$ and, as such, they cannot act in $\mathcal{H}^*$. Also, the time component of these relativities is Hamiltonian, while the Hadronic Mechanics demands the incorporation of contact non–Hamiltonian interactions. Third, all unitary and antiunitary transformations ALTER the Lie–isotopic product and cannot be symmetries of eqs. (16).

The list of insufficiencies of Galilei's and Einstein's special relativities for extended particles treated via the Hadronic Mechanics could continue, but it is not needed for the scholar familiar with the writings of the originators (rather than their followers). In fact, both Galilei (and Einstein) stated quite clearly that their studies were conceived for "massive point" (and "point–like particles")

moving in vacuum under action—at—a—distance forces. The physical conditions we are referring to here are fundamentally different. The generalization of the relativities at a suitable future time is therefore unavoidable, despite a predictable academic resistance.

At any rate, a feverish effort is now well under way to generalize the relativities via covering forms permitted by the Lie—isotopic structure (14).

It should be kept in mind that, at the classical level, rather comprehensive studies have been conducted (some of them dating back to the past century) for the non—Hamiltonian generalization of conventional Galilean formulations. A review of these studies is reported in monograph[5], including a Lie—isotopic generalization of Galilei's relativity.

The studies for a parallel operator generalization of the relativity are well under way. After all, *the studies reviewed in this letter are based on such generalization.* In fact, the itegrated form of the isotopic time evolution (16), i.e.,

$$T^*(t): A' = e^{iHt} * A * e^{-iTH} \qquad (29)$$

IS the time component of the desired generalized relativity, while the isotopic covering of the rotational symmetry used in the data elaboration of Professor Rauch's experiment

$$SU^*(2): A' = e^{i\theta_k J_k T} * A * e^{-i\theta_k T J_k} \qquad (30)$$

$$J_k \in SU(2)$$

IS the rotational component of the desired covering relativity.

Needless to say, these studies are at the very beginning and so much remains to be done. At any rate, I hope that this letter has communicated at least in a small way the contageous scientific enthusiasm underlying these studies [we had planned one volume for the Proceedings of the recent Orléans International Conference, bur we have been forced to increase them to FOUR VOLUMES — see ref.[3]].

Other predictable implications are related to the basic laws and principles of the Atomic Mechanics and, inevitably, Pauli's exclusion principle. It is understood that Professor Rauch's experiment DOES NOT refer to the validity of Pauli's principle in the arena for which it was conceived, the atomic structure. It is also understood that the proposed experiment DOES NOT refer to the approximate validity of Pauli's principle in nuclear physics, which is well established by now.

Nevertheless, in the opinion of an increasing number of scholars, *it is time to submit the exact validity of the exclusion principle in nuclear physics to direct, specific, and detailed tests.*

When neutrons experience a conceivable small deformation of their charge distribution, they are no longer exact Fermions, and comparatively small departures from Pauli's exclusion principle follow.

The view expressed by an increasing number of scholars[1-3] is that it is time to abandon personal views in this fundamental problem, and initiate quantitative experimental studies. The preliminary information already exists, and FAVORS possible small deviations from Pauli's principle theoretically predicts in ref.[11]. In fact, recent data on neutron—tritium scattering experiments apparently permit a small overlapping of the wavepackets of the incident s—neutron with those of the two s—neutron of the tritium core, contrary to the exclusion principle [ref.[31] reproduced in the DOE application].

Needless to say, and as stressed repeatedly by Professor Rauch in his articles and invited talks, the experimental information currently available is highly tentative. But this is precisely the reason for suggesting additional measures. After all, the exclusion principle is merely assumed in current data elaborations of nuclear physics without the beautiful, historical and direct verifications that occurred in atomic physics.

For a full assessement of the implications under consideration, the additional study of the discrete symmetries is necessary.

Consider the time—reversal symmetry. It is represented by an antiunitary operator depending explicitly on spin. For the case of spin ½, the atomic symmetry is characterized by

$$\tau = e^{-i\pi J_2} \mathbb{C} \qquad (31)$$

It is evident that a possible small rotational—asymmetry in the charge distribution would necessarily imply a consequential time—asymmetry. In turn, a time—asymmetry in the evolution of each hadron would have rather profound implications for the virtual entirety of physics. For instance, it would imply a resolution of the historical open problem of the origin of the irreversibility of our macroscopic reality.

Intriquingly, available measures by a collaboration Berkeley/Quebec[33] have indicated a rather clear time—asymmetry. Subsequent measures at Los Alamos[35] have put in question the magnitude of the time—asymmetry of ref.[33]. The problem is now under intense experimental study in the USA, Canada, Europe and, apparently, the U.S.S.R. A resolution is therefore possible in the near future.

Nevertheless, *there is a rather general consensus that the time—reversal symmetry is violated in nuclear physics (and, expectedly, under all strong interactions) although in a small amount.*

Evidently, the proposed experiment is not directly concerned with the time–reflection symmetry. However, *the establishing of a rotational–asymmetry would be the most powerful indirect verification of the time–asymmetry of hadrons.*

A much similar situation occurs for the space–reflection symmetry, whose violation is established in nuclear physics, as well known. In fact, this symmetry too is explicitly dependent on the rotational symmetry via realizations of the type

$$P = \int dV \sum_{s_3} |\vec{r}, s_3\rangle \langle s_3, \vec{r}| \qquad (32)$$

Again, *the verification of a rotational–asymmetry via the proposed experiment would confirm the space–asymmetry,* e.g., that of experiment.[32]

As an amusing comment, permit me to note a rather odd academic situation. A frequent attitude is that of accepting the space–asymmetry, as experimentally established anyhow, but of rejecting the existence of a joint time–asymmetry. This is odd because against all teaching by Einstein (on the equivalence of space and time), as well by Dirac (who explicitly recommended the joint space–and time–asymmetries since 1949, that is, much before the discovery of the space–asymmetry.

A serious study of the experiment proposed by Professor Rauch calls for a technical evaluation of the problematic aspects underlying conventional attitudes of this type in order to separate the pursuit of knowledge from established scientific interests.

For instance, to achieve scientific credibility for the exact time–reflection symmetry joint with the broken space–reflection symmetry, one must solve a number of technical problems of consistency for the conventional formulation of Einstein's special relativity. Clearly, these consistency problems would be resolved by a confirmation of measures (4).

Along similar lines, to achieve credibility for an exact rotational symmetry, joint with the experimentally established space–asymmetry [say, that of the Tin isopotes] one must solve additional problems of consistency. We are referring here to a proof that the Atomic Mechanics can represent the difference in space–asymmetry in the transition from one isotope of Tin to the other. Again, these problematic aspects would be resolved by a confirmation of measures (4), trivially, because deformations of the charge distribution depend directly on the local physical conditions and, thus, they vary from nuclei to nuclei.

Similarly, to achieve credibility for the exact time–reflection symmetry joint with the established macroscopic irreversibility, one should solve a host of technical problems of consistency between the macroscopic and the microscopic descriptions. As an example, one should prove that the experimentally established NONCANONICAL character of the time evolution of Newtonian systems of the real world can be consistently reduced to a large collection of conjectured UNITARY time evolutions of the particle constituents.

Again, situations of this type would be resolved by a confirmation of measures (4). In actuality, the unity of though in physics would be rather beautifully expressed by the fact that the Newtonian, Statistical, the Particle Mechanics are nothing but different realizations of the same generalized formulation of Lie theory. As an example, the Birkhoffian Mechanics and the Hadronic Mechanics are nothing but different realizations of the same Lie–isotopic theory, one via functions, and the other via operators.

The regaining of the unity of scientific though in the various branches of science was one of the objectives of the recent Orléans Conference on Nonpotential Interactions, and an inspection of the Proceedings[3] is recommendable for the refereeing of the experiment proposed.

In essence, we are attempting to convey the idea that it is time to limit the compartmentalized conduction of physics via phenomenological models tailored for each individual aspect of particle dynamics. It is time to test the compatibility of the basic assumptions not only within the limited field considered, but also at the level of the general unity of physics.

An ultimate aspect focused by Professor Rauch's experiment is therefore the dichotomy currently existing between

- the ESTABLISHED NONHAMILTONIAN character of the physical reality of our environment, versus
- the CONJECTURED HAMILTONIAN character of particle dynamics,

and which would be resolved via a NONHAMILTONIAN generalization of particle dynamics.

In such a setting, the restriction of the scientific vision to the Hamiltonian particle mechanics without a general view, can likely prove to be a temporary expedient.

USE OF PROCEEDS. It may be valuable to provide information on the use of the proceeds of the proposal, if funded. It is clear that Professor Rauch, being Director of the Atominstitut of Wien, does not need a salary from U.S. grants. The same situation occurs for other participants in Austria and in France.

The proceeds of the proposal are primarily intended for U. S. physicists by specific desire of Professor Rauch as well as of the Board of Governors of the IBR.

The understanding of this point is important for the review of the proposal. The rationale is that

(a) Professor Rauch's team is currently the leading one in the test of the rotational symmetry;

(b) an intensification of experiments in the field can be predicted, particularly if the current deviations from the perfectly rigid charge distribution are confirmed; and

(c) the U.S.A. has all the technology for additional experiments in National and private laboratories.

In a situation of this type it is advisable that U. S. experimentalists are trained in the field of the proposal UNDER PROFESSOR RAUCH'S SUPERVISION at the ILL—Laboratory in Grenoble. Once this training is achieved, the experimentalists are ready for a continuation of the tests in the U.S.A.

The proposal submitted intends to achieve exactly this objective, with particular reference to the traning of young U. S. experimentalists. The understanding is that the budget cannot be restricted to this purpose only, and a participation in the logistic expenses sustained by Austria and France must be contemplated.

The submission of a detailed budget, including itemization of all expenses and personnel (or the criteria for their identification) is contemplated for submission to DOE at some future time.

Very truly yours,

Ruggero Maria Santilli
President, IBR
and Co—investigator

RMS/mlw

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4-030811S318002 11/14/81 ICS IPMHTZZ CSP BSNB
1 6179641684 MGM TDMT NEWTON MA 11-14 1007P EST

RUGGERO MARIA SANTILLI
28 CROSS ST
WEST NEWTON MA 02165

THIS MAILGRAM IS A CONFIRMATION COPY OF THE FOLLOWING MESSAGE:

TDMT NEWTON MA 11-14 1007P EST
INT PROFESSOR RAUCH
ATOMINSTITUT SCHUETTELSTRASSE 115
1020WIEN (AUSTRIA)
SUGGEST SENDING ME AUTHORIZATION BY TELEGRAM TO SUBMIT PROPOSAL
WITHOUT ZEILINGER SIGNATURE. POSSIBLE OFFICIAL SUPPORT BY US
GOVERNMENT MAY BE IMPORTANT TO RESOLVE GRENOBLE IMPASSE. BEST WISHES
FOR YOUR TRIP TO GRENOBLE
RUGGERO MARIA SANTILLI

COL 115 1020WIEN

22:11 EST

MGMCOMP

TO REPLY BY MAILGRAM, SEE REVERSE SIDE FOR WESTERN UNION'S TOLL - FREE PHONE NUMBERS

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

October 22, 1981

Office of the President

Dr. DAVID C. PEASLEE
Division of High Energy Physics
Physics Research Branch
DEPARTMENT OF ENERGY
Mail Statikn J-309
WASHINGTON, D.C. 20545

Dear Dr. Peaslee,

I enclose copy of the proposal
EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION
as per our phone conversation of October 19, 1981.

As you can see, the proposal has been signed by the Principal Investigator Professor H. RAUCH, as well as by the co-investigators Professor J. SUMMHAMMER and myself, but (regrettably), it has not been signed by an additional co-investigator recommended by Professor Rauch, namely, Dr. A. ZEILINGER (a member of the Atominstitut of Wien, currently spending the 1981-1982 academic yeat at M.I.T.).

As a result, please do not consider this letter a submission of the proposal.

While the M.I.T./Dr. Zeilinger case continues to be investigated, I would appreciate the courtesy of your recommendation on the following aspects.

(1) As you know, the proposal is of international nature and the Principal Investigator (Professor Rauch) is the Director of the Atominstitut of Wien. According to DOE regulations, does Professor Rauch needs an appointment at our Institute (the administrative conduit) to qualify as Principal Investigator?

(2) Is my Social Security Number in the Proposal sufficient according to DOE regulations, or additional investigators must have the S.C.N.?

(3) Is there any additional regulatory aspect we should be aware of in order to file the application in a proper way ?

Your assistance in the f nalization of the proposal would be sincerely appreciated. Thanking you in advance,

I remain, Yours Sincerely

Ruggero Maria Santilli
RMS-pm
cc.: Drs. Wallenmeyer and Hildebrand, DOE

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

November 10, 1981

Dr. ROBERT L. THEWS
Division of High Energy Physics
Office of High Energy and Nuclear Physics
DEPARTMENT OF ENERGY
Mail Station J-309
WASHINGTON, D.C. 20545

RE: Conversation of October 5 regarding the possible submission to DOE of a research grant proposal entitled "EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION", with Principal Investigator Professor H. Rauch.

Dear Dr. Thews,

I would like to confirm our phone conversation of October 5 regarding the administrative requirements for our possible filing of the proposal , with particular reference to:

(1) Prof. Rauch Position. There is no need that Prof. Rauch acquires a U.S.A. Social Security Number because no funds of the proposal would be used for his personal salary. However, it is recommendable (if not necessary) that Professor Rauch has a formal appointment at our Institute. The appointments of Full Professors we are currently issuing are sufficient (I am referring here to our joint appointment as members of our Institute in a way compatible with existing academic appointments).

(2) Use of proceeds. You suggested that, in case known, the names of the persons who would receive the funds should be indicated in a letter, and that those persons should have an U.S. Social Security Number, if possible. I do not have this information. I shall thorefore do my best that the information be relased to you in case the application is founded.

(3) MIT Stall. As you know, the proposal has been stalled by the MIT suggested member, Dr. Zeilinger, and his superiors (see my letter to Dr. Zeilinger of October 29). As also indicated to your Office, there is no need that Dr. Zeilinger signs the proposal; or, in case his superiors will authorize him to do so, he is welcome to sign. The important point is that a formal written resolution on whether to sign or not to sign be reached as soon as possible. In fact, additional un-necessary and un-explained delays may give the impression that MIT might be interested in stalling the proposal, which I presume is not the case.

Owing to these (and a number of other circumstances), permit me the liberty of asking your friendly intervention.

I am suggesting here the possibility that you contact Dr. Zeilinger or his superiors (Dr. Shull, Head of the Nuclear Physics Division at MIT) and Dr. Feshback (Chairman of the Department of Physics at MIT), to the effect of suggesting a solicit resolution of the issue (Yes, Dr. Zeilinger will sign; or No, Dr. Zeilinger will not sign). Equivalently, in case of a continuation of the current status of lack of any decision, we would appreciate an indication of the reasons.

However, in case you think that this direct contact between your office and MIT is inappropriate at this time, please ignore my request. You can count on our best understanding.

Very Truly Yours

Ruggero Maria Santilli
President

RMS-pm

cc. Professor H. RAUCH. Atominstitut
Schuettelstrasse 115, A-1020 WIEN, Austria

Department of Energy
Washington, D.C. 20545

January 3, 1982

Dr. Ruggero M. Santilli
President
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

This is in reply to your letter of December 11, 1982. As you probably surmise from the newspaper accounts, the Department of Energy's budgetary situation in Fiscal Year 1983 is somewhat confused. We are functioning under Continuing Resolution and expect to be under Continuing Resolution for the indefinite future. I very seriously doubt that any favorable action on your proposal will be possible. Certainly favorable action in early January does not appear to be at all likely.

Sincerely,

Enloe Ritter

Enloe T. Ritter
Director
Division of Nuclear Physics

Dr. Wallenmeyer, Director
High energy physics Divisio
Division of high energy Physics
Department of Energy
WASHINGTON, D.C.

Tel. (301) 353 3367

I am happy to report formal authorization from LL-Laboratory Grenoble to proceed test of spin symmetry under strong interactions via a collaboration Austria-France-USA. DOE application with Professor Rauch as principal investigator is forthcoming.

Best Regards

Ruggero Maria Santilli, President
The Institute for Basic Research

Telegram mailed March 3, 1982

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 27, 1982

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
DEPARTMENT OF ENERGY
Washington, D.C. 20545

Dear Dr. Wallenmeyer,

I hereby submit most respectfully the enclosed original and seven copies of the research grant proposal entitled

EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-U.S.A. COLLABORATION,

under administration by our Institute, and with Principal Investigator Professor H. RAUCH, Director, Atominstitut der Österreichischen Universitaeten, Wien, Austria, who is an undisputed experimental leader in the field of the proposal (neutron interferometry

As you can see, the proposal has been made as brief as possible, thanks also to its experimental character. However, I would appreciate your consideration of the advisability for us to prepare a collection of experimental and theoretical articles in the problem, for referee's convenience. Please let us know whether or not we should prepare this collection of articles. Also, please keep in mind that the experiment could be started this summer, in case funded. We would therefore appreciate a speedy consideration of the proposal, of course, within the time schedule of your Office. The connection with the other proposal currently pending at your Office under administration by our Institute should be kept in mind. In fact, the proposal by Professors BENKART, MYUNG, OEHMKE, OSBORN, and TOMBER deals with the development of the basic mathematical tools to treat the deformation of the charge distribution of hadrons under strong interactions, as preliminarily detected by the available measures of this proposal.

Finally, permit me the liberty of recommending, if at all needed, that extreme care be exercised in the desired referees, with particular reference to their proved ethical standards. The proposal is for an open problem that is clearly at the foundation of contemporary physical knowledge. As such, individual referees might be tempted to discourage the conduction of the experiment in order to protect personal academic-financial interests, to the detriment of the true pursuit of novel human knowledge. At any rate, this proposal reaches your desk after years of documented opposition by a number of physicists who have been trying, whether openly or criptically, to prevent the conduction of this fundamental experiment. This opposition apparently originates within circles of researchers financially and academically committed to the conjecture that quarks are the constituents of hadrons. In fact, the finalization of the current experimental measure by Professor RAUCH of a small (1%) deformation of the charge distribution of hadrons under strong interactions, could have rather profound, negative implications for the quark conjecture (even though the physical value of the unitary models of Mendeev-type classification of hadrons would be essentially untouched). I believe tha it is in the best interest of DOE as well as of the international physics community that you are informed of this opposition, so that you can take the appropriate precautionary measures. Also, it appears appropriate, owing to the nature of the application and its international character, that the ethical profile be focused from the outset. Needless to say, we are confident that your Office will indeed meet all pur expectations.

Very Truly Yours,

Ruggero Maria Santilli
President
RMS-mlw, encls

Department of Energy
Washington, D.C. 20545
Mail Stop ER-23 GTN

JUN 1 1 1982

Professor R. M. Santilli
President
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

In Dr. Wallenmeyer's letter of June 7, 1982, he informed you that he was forwarding your proposal entitled "Experimental Verification of the SU(2)-Spin Symmetry Under Strong and Electromagnetic Interactions by a Joint Austria-France-USA Collaboration" to the Division of Nuclear Physics. The proposal is now under review in this Division, and you will be advised as soon as a decision has been reached.

We would appreciate knowing whether this proposal is being submitted to any other Federal agency or whether there are any other sources of Federal support.

If you should wish to inquire about the status of this proposal, please feel free to get in touch with me.

Sincerely,

Enloe Ritter

Enloe T. Ritter, Director
Division of Nuclear Physics

cc:
H. Rauch, Atominstitut
J. Summhammer, Atominstitut
H. Willard, NSF

June 21, 1982

STRICTLY CONFIDENTIAL

Dear Dr. Ritter,

Permit me the liberty of recommending, most respectfully, that no referee for Prof. Rauch's proposal is selected from MIT, Harvard and other local institutions of the Boston area. The conflict of interest between the proposal and the research currently conducted at these institutions would then invalidate a fair referee process.

The recommendation is the result of a number of years of interference in the conduction of the studies underlying the test of the spin-symmetry (and, thus, of Einstein's special relativity) under strong interactions. Some of the episodes indicate such gross academic greed to be hardly believable. Yet, the existing documenation speaks for itself. At any rate, members of the research teams underlying the project have been forced in more than one occasion to hire attorneys. Therefore, we have been very close more than once to an open confrontation in court houses and news media. The fact that you did not read about these episodes in the Washington Post, or Paris Soir, or other newspapers, is the best evidence of my personal committment to an orderly condition of our community.

Lately, I recommended the Division of High Energy Physics of DOE to abstein from contacting these institutions in regards to a primary research grant application for our new institute of research. Apparently, my recommendation was not followed and backfired considerably both internally in the local institutions (which, after all, do house ethically sound scholars), as well as externally (via new unbelievable extremes of open interferences). This created predictable, un-necessary aggravations. I have absteined from reporting these episodes to Drs. Wallenmeyer and Hildebrand as a form of respect for them, and for the difficulties of their work.

Therefore, I believe that DOE can be only damaged by contacting MIT, Harvard, and other similar institutions on fundamental experiments such as the test of spin/Einstein's relativity under strong interactions. After all, the existence at these institutions of vested, organized academic interests favoring the preservation as much as possible of old knowledge, is well known.

The pursuit of knowledge as well as of national interests for novel advancements is in the hands of governmental officers such as you. I am fully aware of the difficulties of this task. For this reason you should count on my best possible assistance and backing. As indicated by phone, permit me to beg you to contact me confidentially in case delicate situations arise during the consideration process. In fact, we can join forces to identify the smoothest possible way.

But, most of all, permit me to stress that:(a) the application is for experiments; (b) it is of truly fundamental physical nature; and (c) it can eventually result to be either in favor or against old knowledge. Physicists opposing the test on scientific grounds are therefore of clearly questionable ethical standards. In fact, why should they oppose tests that may eventually result to be in favor of their own views?

This latter question is at the foundation of the existing problems here in the Cambridge college community. The decision to organize our new institute of research was taken by a number of concerned scholars, businessmen, and observers precisely in favor of an orderly condition of our research. However, the consultation of the local institutions for IBR grant applications, such as the latest one to the Division of High Ene-gy Physics might have done, undermine exactly these efforts, by therefore multiplying the possibilities of an open confrontation.

Very Truly Yours

Ruggero Maria Santilli

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 1, 1982

Dr. Enloe T. Ritter, Director
Division of Nuclear Physics
Department of Energy
Washington, D.C. 20545
Mail Stop ER–23 GTN

Dear Dr. Ritter,

As a gesture of courtesy I would like to pass to you information that recently became available to our Institute concerning the experiment by Slobodrian, Conzett, et al. on the violation on the time–reflectior. symmetry under strong interactions.

1. Assuming that they are correct, the four measures conducted by Hardekopf, et al at Los Alamos are not sufficient to establish the identity of the polarization of the forward reaction with the analyzing power of the backward reaction. This is according to a theoretical study conducted here at IBR. Copy of a diagram is enclosed for your information.

2. Professors Slobodrian and Conzett have found serious experimental reasons to doubt the validity of the four measures at Los Alamos. Copy of letters from Slobodrian to Veeser are enclosed on a *confidential basis*. Experimentalists contacted by us have indicated that the apparent inconsistencies of the Los Alamos measures are truly sound.

3. The Québec–Berkeley experimental group has repeated again their measures and found values very close to the original ones. It appears that a communication by the experimentalists on these additional measures will be made publicly available in the near future.

In addition to the direct information, you should also keep in. mind the considerable amount of indirect information supporting the violation of the time–reflection symmetry under strong interactions.

I am referring here, for instance, to:

a. The available measure by Rauch's experimental team on the apparent deformation of the charge distribution of neutrons in the field of nuclei. As you know, the underlying rotational–asymmetry, if confirmed, will imply a necessary violation of the time symmetry. Copy of a [illegible] by Rauch is enclosed

b. An increasing number of theoretical studies indicate the existence of new, rather substantial, problematic aspects in the relationship between the experimentally established macroscopic irreversibility and the conjectural particle reversibility. These problems were studied at our recent International Conference at Orléans [see for instance a paper by Tellez–Arenas]. It is clear that the best resolution of this historical problem is that along the experiment by Slobodrian, Conzett, et al.

c. An additional array of problematic aspects is currently surfacing for a joint time–reversal symmetry combined with the established, broken space–reversal symmetry. I am referring to inconsistencies in the structure of the Special Theory of Relativity. After all, Einstein taught us the equivalence of space and time and Dirac has stressed since 1949 his expectation of a joint space–asymmetry and time–asymmetry.

I hope that this information is of some value to you. I shall continue to keep you informed of the most salient events in this important physical problem.

Wishing you a happy summer.

I remain,

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

Enclosures

cc: Drs. W. A. Wallenmeyer, B. Hilderbrand, R. Thews, DOE.
Drs. R. M. Sinclair, M. Bardon, and P. S. Rosen, NSF

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 7, 1982

Professor H. RAUCH, Director
Atominstitut der Osterreischen Universitaeten
Schuettelstrasse 115
A-1020 WIEN, Austria

Dear Professor Rauch,

It is a pleasure to inform you that your application entitled

"Experimental verification of the SU(2)-spin symmetry under strong and electromagnetic interactions via a joint Austria-France-U.S.A.collaboration"

is under active consideration by the U.S. DEPARTMENT OF ENERGY, thanks to the interest of Dr. ENLOE T. RITTER, Director of the Division of Nuclear Physics, and following a kind referral to such division by Drs. W. WALLENMEYER and B. HILDEBRAND of the Division of High Energy Physics.

I would like to take this opportunity to inform you that your application will not be jointly considered by the U.S. NATIONAL SCIENCE FOUNDATION as a result of a sound judgment to avoid un-necessary duplication of Governmental efforts.

I have separately mailed to you more detailed information concerning the consideration of your proposal at DOE. Wishing you the best success in the funding of your important experiment, and in its actuation, I remain

Yours Very Truly

Ruggero Maria Santilli
President

RMS-mlw

cc.: Drs. E.T.RITTER, W.WALLENMEYER, and B, HILDEBRAND, DOE, and
Drs. R.M.SINCLAIR, M. BARCON, and P.S. ROSEN, NSF
Board of Governors, IBR

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 8, 1982

Dr. E.T.RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545

RE: Research proposal by Prof. H. RAUCH
"Experimental verification of the
SU(2)-spin symmetry ...."

Dear Dr. Ritter,

I enclose a self-explanatory letter to Professor Rauch in Wien indicating the status of his applications to DOE and NSF. As you can see, your consideration of the proposal is the only active one. The same proposal is not under consideration by any other Governmental, Corporate, or Private Institution. No additional submission of the proposal is contemplated in the future.

I am taking this opportunity to enclose a few lines prepared by members of our Institute regarding the possible applications of the hadronic mechanics to quark theories, QCD, and all that. As you can see, these possibilities are rather intriguing. Particularly relevant is the possibility of a genuine advancement in the vexing problem of confinement of quarks, which would become apparently possible if one differentiates the physical laws of the center of mass of a hadron in an external elm field (exterior problem) from possible generalized laws for the constituents (interior problem). As you know, the lack of achievement of quark confinement in a form truly credible, is one of the most delicate aspects of contemporary physics, scientifically and administratively.

Intriguingly, all the possibilities for quark theories under consideration are dependent rather crucially on the confirmation of the deformation of the charge distribution of hadrons under strong interactions, according to the proposal by Professor Rauch under consideration at your office. In fact, the measures would likely force the abandonment of conventional physical laws for the interior problem only, and set the way for more general laws. In turn, this would permit the attempt of genuinely novel advances in strong interactions, including conventional quark theories.

Best Personal Regards

Ruggero Maria Santilli
President

RMS-mlw

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

September 22, 1982

Professor H. RAUCH
Atominstitut
Schuttelstrasse 115
A—1020 WIEN, Austria

Dear Professor Rauch,

I would like to inform you that I have recently visited Dr. Ritter of the Division of Nuclear Physics of NSF, which is currently considering your proposal. I am pleased to report that Dr. Ritter is genuinely interested in the project, and he is doing his very best. Apparently, half of the referee's reports have already been received, and the remaining half is expected in the near future. After that, a decision will be based on budgetary considerations in view of the following occurrences.

As you know, Reagan's policies have implied substantial limitations in the current budget for research. However, Congress is expected to act this coming fall on the research budget for next year. This latter decision includes the budget not only for DOE at large, but also the budget specifically for Dr. Ritter's division.

As a result of this situation, in case there are no funds available in the budget for the current year, it may be advantageous to allow Dr. Ritter to delay the decision until the budget for next year is known.

In the meantime, I am happy to inform you that we have received an extension of our own contract for theoretical studies. Please keep in mind that I can make available to you funds up to a maximum of $1,500. My only restrictions are of administrative nature, in the sense that the funds should be dispersed in the same way as we did for you and Professor Eder. I am referring more specifically to: (a) consulting agreement with any person you recommend; (b) the release to us of subsequent publications in any journal [not necessarily the Hadronic Journal]; and (c) the nature of the research should be experimental or theoretical in strong interactions.

The contract has been legally executed and, therefore, the availability of the funds is guaranteed. However, please keep in mind that the actual release of funds is done one month after an individual request. Please feel free to phone me at any time at your convenience at home [(671) 964 1684] or at the office for the use of these funds.

Best regards,

Ruggero M. Santilli
President

RMS/mlw

I. B. 1109 R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 22, 1982

RE: Application: EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION

Principal Investigator: Professor H. Rauch
Submitted to the Nuclear Physics Division of DOE on June 21, 1982

Dr. ENLOE T. RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545
Mail Stop ER–23 GTN

Dear Dr. Ritter,

I have attempted to call you twice this week because I have good scientific news. In fact, the Berkeley–Quebec–Bonn experimental group has repeated the measures on time–asymmetry in nuclear physics and confirmed the original findings, contrary to the disclaim by the Los Alamos group. A copy of their recent paper is enclosed.

This result renders much urgent, if at all needed, the repetition of the tests on spin symmetry by Professor Rauch according to the pending proposal. In fact, the measures indicate quite clearly that the origin of the time–asymmetry is in the spin component, that is, in the spin part of the time–reversal operator

$$\tau = e^{i\pi J_2} C$$

Again, the theoretical interpretation is so natural, to be trivial. It is given by the possible deformation of the charge distribution of nucleons in the conditions of the reactions considered, with consequential loss of rotational symmetry.

Regrettably, I have also to report an escalation of academic opposition against the experimental–theoretical study of this fundamental physical problem. This opposition has taken the form of a blatant misuse of academic authority, with manifest misconduits in refereeing processes. Therefore, it has been necessary to force the issue of ethics in academia, by taking several actions, including formal requests of resignation of academicians holding editorial posts at the Journals of the American Physical Society.

I can personally testify to the DOE efforts and successes in avoiding academic pressures of doubtful ethical motivations. In regards to the current consideration of the proposal on the test of the rotational symmetry, we fear the possibility that your office might be exposed to outside pressures by corrupt academicians interested in preventing the acquisition of experimental knowledge on the possible deformation of the charge distribution of hadrons for the perpetration of personal gains, in disrespect of the most elemental National and human values. In order to prevent even the vague feelings of possibilities of this type, as well as to permit the smoothest possible conduction of the examination, it is evident that our direct communications are of utmost importance.

But, most of all, we should always keep in mind the true values here. In fact, there is absolutely no doubt whatsoever that the proposal to test the rotational symmetry is, by far, the most important and fundamental project on your desk at this time.

Very Truly Yours

Ruggero M. Santilli
President

RMS—mlw

encls.

I. B. R. – 1111 –

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

December 11, 1982

Dr. ENLOE T. RITTER
Division of Nuclear Physics
Department of Energy
Washington, D.C.

RE: I.B.R. application entitled "Experimental verification of the SU(2)-spin symmetry ...."

Dear Dr. Ritter,

I would appreciate the courtesy of your consideration whether a decision on the application can be reached in early January 1983.

Such a consideration appears recommendable on account of a number of aspects, some of which reported to Drs. Wallenmeyer, Hildebrand, and Thews at the DOE Division of High Energy Physics, and others that are specific for your case.

In particular, in case you foresee possibilities for funding, kindly consider the suggestion of my recent letter to you of November 18, 1982, concerning the possible signature of a contract with initiation of support at some subsequent, postdated, specified time.

However, if, for any reason, you are not in a position to reach a decision in early January 1983, you can count on my full understanding. In this case, I would be grateful for the mere indication of the time projection for a possible decision.

I would like to take this opportunity to express to you and to your family our best and sincere wishes for a happy holiday.

Yours, Sincerely

Ruggero

Ruggero M. Santilli

Department of Energy
Washington, D.C. 20545
Mail Stop ER-23 GTN

November 12, 1982

Dr. Ruggero M. Santilli
President
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

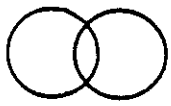
Dear Dr. Santilli:

The Division of Nuclear Physics has now had your proposal "Experimental Verification of the SU(2)-Spin Symmetry Under Strong and Electromagnetic Interactions by a Joint Austria-France-USA Collaboration" under review for six months. Unless you have an objection, we would like to retain this proposal under active consideration for another six months. Please contact me if you have questions or wish to discuss this action.

Sincerely,

Enloe Ritter

Enloe T. Ritter
Director
Division of Nuclear Physics



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 18, 1982

Dr. ENLOE T. RITTER
Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
Mail Stop ER–23 GTN
WASHINGTON, D.C.

RE: Research grant proposal No. P8206041 entitled
EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION
with Principal Investigator Professor H. RAUCH

Dear Dr. Ritter,

We would like to express our appreciation and approval for your decision to retain the proposal under active consideration for another six months, as expressed in your letter of November 12, 1982. We are fully aware of your current difficulties created by the lack of normal operations under a formal budget. You can, therefore, count on our full understanding, cooperation, and backing.

Permit me to take this opportunity to bring you updated on scientific developments which are relevant to the consideration of the proposal.

FORMAL APPROVAL AT THE ILL–LABORATORY. You will be pleased to know that the Laue–Langevin Laboratory has formally approved the repetition of the experiment, and authorized Professor Rauch to proceed whenever the availability of funds will allow him to do so. Copy of the formal communication is enclosed. Note that the approval implies also the confirmation of the cost sharing of the experiment as far as the French part is concerned. The Austrian cost sharing is ensured by the Principal Investigator, as Director of the Atominstitut in Wien.

The ILL approval is the result of a rather extensive investigation by a subcommittee in nuclear physics under the personal supervision of the Director, Professor Springer. The subcommittee was headed by Professor Schultz of Julich, West Germany, and included Professors Specht, Leroux, Vin Mau, Sandars, Shotter, Faust, and others. The extensive nature of the investigation, and the personal supervision by Professor Springer, were suggested by rather unfortunate external interferences that apparently caused a halt of the process.

Quite regrettably, this occurred just prior to our First International Conference on Nonpotential Interactions, held under support from the French Government (and DOE) at the Universite d'Orléans, France, in January, 1982. The problem of the possible deformation of the charge distribution was a central topic of the conference and, of course, Professor Rauch was one of the central invited speakers.

I am sure you will share our pleasure in seeing a smooth and orderly resolution of a case which had reached at times quite considerable tensions.

CONFERENCES AND WORKSHOPS. The *First International Conference on Nonpotential Interactions* resulted in four volumes of proceedings for over 2,000 pages, and was attended by numerous mathematicians, theoreticians, and experimentalists including formal convoys from the JINF of Dubna, U.S.S.R., and the University of Peking, China (see enclosures).

Two workshops have now been planned for summer, 1983. Professor Rauch's experiment is fundamental for both. In fact, we shall have the *FIRST WORKSHOP ON HADRONIC MECHANICS* in afternoon sessions on August 2–7, 1983 (see the enclosed preliminary announcement). Jointly, during morning sessions, we shall have the *FIFTH WORKSHOP ON LIE–ADMISSIBLE FORMULATIONS* of pure mathematical character. The former workshop deals with a generalization of quantum mechanics based on the possible deformation of charge distributions, while the second deals with the novel mathematical theories needed to represent the phenomenon (isotopies and genotopies of Lie algebras).

SCIENTIFIC IMPLICATIONS OF THE PROPOSED EXPERIMENTS. We have reported to you earlier some of the implications illustrating the truly fundamental character of the experiments. Here are a few additional comments.

(A) ORIGIN OF THE IRREVERSIBILITY OF NATURE. As you know, the measures by Conzett, Slobodrian et al. indicating a possible violation of the time–reversal symmetry in open (nonconservative) nuclear reactions have been recently confirmed, and a new paper has been submitted for publication. The experimenters insist that the violation occurs in the spin component of the nuclear forces.

Rauch's experiment on a possible deformation of the charge distribution of nucleons under external strong interactions is therefore a rather forceful indirect verification of the Conzett–Slobodrian experiment.

Regrettably, the case is afflicted by a number of prejudices. For instance, few physicists have done actual calculations to prove, as we do in the hadronic mechanics, that the time–asymmetry specifically applies to OPEN nonconservative nuclear reactions (e.g., when the target is fixed and external), and that it disappears in the reformulation of the setting into a closed form inclusive of the external target. In fact, the center–of–mass trajectory of any system isolated from the rest of the universe, is expected to be time–reversal–invariant. Also, few physicists have studied the compatibility of the time–asymmetry in open nuclear reactions with the apparent lack of time–asymmetry in spontaneous decays of hadrons via semileptonic processes.

These considerations are important because they also apply to the deformation of the charge symmetry of hadrons. In fact, the rotational–asymmetry measured by Rauch, and which should be confirmed or denied by the proposed experiments, is also for neutrons in the EXTERNAL field of the nuclei of the fixed target. Again, if one passes to the formulation of the interaction into a closed form inclusive of the target, we do not see interest in studying the problem of the charge symmetry, the system being isolated.

(B) CONSTRUCTION OF HADRONIC MECHANICS. As you know, the deformation of the charge distribution of hadrons does not appear to be describable via conventional quantum mechanics, owing to the intrinsically point—like representation of the particles and of their constituents.

Rauch's experiment is at the foundation of the current efforts to attempt a generalization of quantum mechanics into a covering discipline called Hadronic Mechanics.

The main idea is to generalize the structure of the Hilbert space via the so—called modular and bimodular isotopies (i.e., generalizations of the associative product, from the conventional form AB to the isotopic form $A*B = ATB$ where T is a suitable operator).

As shown by Professor Eder (also of the Atominstitut in Wien), the covering mechanics is capable of predicting the deformation of the charge distribution as measured by Professor Rauch, while preserving the conventional values of the magnitude and third component of spin. A separate research grant proposal, based on the experiments by Professor Rauch, Conzett—Slobodrian, et al, is currently under consideration by the High Energy Physics Division of DOE for the construction of the hadronic mechanics. I am confident that Drs. Wallenmeyer, Hildebrand, and Thews will cooperate with you for any additional information you may desire.

(C) APPLICATIONS TO NUCLEAR PHYSICS. You will be pleased to know that a host of new possibilities have been already stimulated by Professor Rauch's experiments. For instance, Professor Eder has shown how the anomalous character of the nuclear magnetic moments can be interpreted quite naturally by a conceivable deformation of the charge distribution in the interior conditions only.

The idea is not new, and, regrettably, its study was abandoned for apparent reasons of academic politics. For instance, Blatt and Weisskopf state quite clearly in their *Theoretical Nuclear Physics* (p.31) that "it is possible that the intrinsic magnetism of a nucleon is different when it is in close proximity to another nucleon." (see enclosure)

Numerous additional insights are coming out, all valuable for nuclear physics. I am referring here to the apparent possibility to suppress the atomic spectrum of energy, in order to resolve the vexing problem of lack of excited states in the deuteron, and several other developments.

(D) GENERALIZATION OF GALILEI'S RELATIVITY. As you know, conventional, classical and quantum mechanical relativities (Galilei's and Einstein's) apply to closed systems of point—like particles with only potential internal forces. However, inspection of nature soon reveals the existence of classical systems which are closed in the conventional sense (of verifying usual total conservation laws), yet the internal forces are of nonpotential/non—Hamiltonian type (think of our Earth as seen from an outside observer).

You will be pleased to know that I have recently completed my studies for a possible classical generalization of Galilei's relativity for closed non—Hamiltonian systems. The generalized relativity is presented in my Volume II of *Foundations of Theoretical Mechanics* entitled *Birkhoffian Generalization of Hamiltonian Mechanics,* which is just about to be released by Springer—Verlag. Incidentally, the monograph establishes the classical foundations of the rotational—asymmetry.

I am now feverishly working at the expected, corresponding generalization of the quantum mechanical Galilei's relativity via the structurally more general foundations of the hadronic mechanics.

Needless to say, Professor Rauch's experiment is absolutely fundamental for this task, evidently, because the rotational symmetry is at the foundations of all relativities. Note that a corresponding generalization of Einstein's special relativity is expected to be mandatory for the interior strong problem (only).

(E) CONSTRUCTION OF A NONPOTENTIAL SCATTERING THEORY. As you know, all contemporary elaboration of scattering experiments is done via the use of a theory which is strictly conceived for "potential scattering" that is, for point–like abstractions of hadrons or of their constituents.

A group of scientists headed by Professor Mignani (Univ. of Rome, Italy) is constructing a generalization of the theory which is called of nonpotential type mainly for its classical image while, technically, the theory is based on the isotopic lifting of the Hilbert space. In particular, the researchers have shown that THE REPRESENTATION OF HADRONS AS EXTENDED PARTICLES MAY IMPLY A CHARGE IN THE CROSS SECTION, that is, the possibility of reviewing the data elaboration of current experiments in high energy physics in which particles reach the conditions of mutual prenetration of their charge distributions.

The experimental information at the foundation of Mignani's nonpotential scattering theory is, again, Rauch's measure of deformation of the charge distribution, and the consequential time–asymmetry by Conzett and Slobodrian. The possible administrative implications alone are staggering.

I hope that this brief and nontechnical outline gives you an idea of the importance of Rauch's experiment, which is evidently such to dwarf any other study of minute incremental nature.

I also hope this may give you an idea why past academic interferences in this case have forced very moderate and peaceful scientists at the very edge of clamorous public gestures. In fact, academic interferences are, in this instance, too much beyond what a normal, ethically sound physicist can tolerate.

POSSIBLE POST–DATING OF CONTRACT. In summary,

- Professor Rauch is in a position to repeat the experiment at the ILL–Laboratory at any time now;
- Here, at the IBR, we are ready to hire a U.S.A. experimentalist to be trained by Professor Rauch at the ILL–Laboratory for his subsequent, independent continuation of studies in the States, as pointed out in the existing proposal, and our additional elaboration dated June 16, 1982; and
- Numerous scientific developments are under way, all crucially dependent on the funding of the proposed experiment.

We are aware that current regulations do not permit back–dating of DOE contracts. However, we understand that current regulations permit the POST–dating of contracts. We are referring to the possible signature of contracts for initiation of funding at some specified subsequent date.

Please consider this possibility. In fact, we have here the capability of financing Professor Rauch's experiments, of course, after signature of a contract. In particular, we would have no problem in financing the experiment even for delays of the order of six months (and even possibly more)

in the reimbursement by DOE. The necessary pre—requisite for such a financing is, of course, the signature of the contract.

I do not know whether this possibility makes administrative sense. I pass it to you as an expression of our sincere desire to collaborate.

Best Personal Regards,

Ruggero Maria Santilli
President

RMS/mlw

Enclosures

cc: Drs. W. WALLENMEYER, B. HILDEBRAND, and R. THEWS, DOE

ENCLS: 1 — Copy of ILL—authorization.
2 — Table of Contents of Proceedings of Orléan Conference.
3. — List of participants of the Orléans Conference.
4 — Front Page of new paper by Slobodrian et al.
5 — Quotation from Blatt and Weisskopf "Theoretical Nuclear Physics".
6 — Outline of "Birkhoffian Generalization of Hamiltonian Mechanics".
7 — Announcement of First Workshop on Hadronic Mechanics.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

December 11, 1982

Dr. ENLOE T. RITTER
Division of Nuclear Physics
Department of Energy
Washington, D.C.

RE: I.B.R. application entitled
"Experimental verification of the
SU(2)-spin symmetry ...."

Dear Dr. Ritter,

I would appreciate the courtesy of your consideration whether a decision on the application can be reached in early January 1983.

Such a consideration appears recommendable on account of a number of aspects, some of which reported to Drs. Wallenmeyer, Hildebrand, and Thews at the DOE Division of High Energy Physics, and others that are specific for your case.

In particular, in case you foresee possibilities for funding, kindly consider the suggestion of my recent letter to you of November 18, 1982, concerning the possible signature of a contract with initiation of support at some subsequent, postdated, specified time.

However, if, for any reason, you are not in a position to reach a decision in early January 1983, you can count on my full understanding. In this case, I would be grateful for the mere indication of the time projection for a possible decision.

I would like to take this opportunity to express to you and to your family our best and sincere wishes for a happy holiday.

Yours, Sincerely

Ruggero

Ruggero M. Santilli

I. B. R.

CERTIFIED MAIL

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

January 15, 1983

Dr. ENLOE T. RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY - GTN
Washinton, D.C. 20545

RE: Research grant proposal number P8206041 entitled "EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-U.S.A. COLLABORATION" Principal Investigator: Prof. H. Rauch (Austria)

Dear Dr. Ritter,

I feel obliged to convey my distress because of your letter of January 3, 1983 concerning your inability to fund the fundamental test of spin by H. Rauch et al. I feel also distressed by my inability to reach you by phone.

On my part, I would like to confirm what indicated earlier, that you can count on my best possible assistance to facilitate your task. For instance, I can provide my services to see whether the current budget of $94,900 can be slashed in half by eliminating deferrable items, such as the hiring of a U.S. experimentalist to be trained by Rauch for future continuation of the experiments in the States. The understanding is that, since the amount requested is already unusually small for an experiment, I cannot cooperate for reduction of costs below a level of decency vis-a-vis the expenditures supported by Austria and France.

On your part, you should consider dismissing any connection of this case with the Continuing Resolution owing to the extremely minute amount of funds involved, particularly when compared to the degree of scientific accountability vis-a-vis the taxpayers that is at stake here. In fact, as you know well, your office is spending large public funds in nuclear physics in projects based on the mere belief of the exact validity of the rotational symmetry. Statements to the contrary should be taken with extreme care because they might be motivated by the intension to manipulate basic physical knowledge in the benefit of vested academic-ethnic interests. In fact, the idea that the extended charge distributions of nucleons are perfectly rigid has no scientific basis. After all, Rauch's current measures, as you also know well, show deviations from the exact symmetry quite clearly, and this renders the repetition of the experiments simply unprocrastinable.

You should be fully aware that the experimental test of spin in nuclear physics is, as it must be, a breaking point, and, as such, it can provoce a host of undesired problems ranging from class actions to the solicitation of Governmental investigations. In fact, no ethically sound scholar can silently accept the scientific, economic and military implications caused by the indefinite deferral of the test.

The rotational symmetry is at the foundation of contemporary physical knowledge. The suppression of its direct verification which has been successfully achieved until now by vested, organized, academic-ethnic interests, has all the ingredients of a scientific crime against this beautiful Land, against our children who have to live on it, and against the pursuit of novel human knowledge.

Very Truly Yours

Ruggero Maria Santilli

TO BE MAILED TO: Dr. G.KEYWORTH II, Science Adviser, The White House
Drs.D.P.HODEL, Secretary, and S. BREWER, Assistance Secretary, DOE

March 31, 1983

CONFIDENTIAL

Dr. E.T.RITTER
DOE

Dear Dr. Ritter,

As you know, my distress letter to you of January 15, 1983 was the result of my inability to reach you by phone. It is perhaps in the DOE and your best interest that I disclose the reason of the distress.

It is due to rumors circulating by the end of 1982 that Drs. C.G.SHULL and A. ZEILINGER of the MIT nuclear physics division were running, under financial support by your division, the fundamental test of the spherical symmetry of the charge distribution of neutrons under intense external fields, via neutron interferometers.

As you know, this test is precisely the test that H. RAUCH has been conducting since 1975 and which is the subject of Rauch's experimental proposal under consideration by your office. Also, as you eventually know, Zeilinger is a former associate of Rauch.

The reason for the distress is that the conduction of the experiment had been recommended by our group to Dr. Shull since the time I was at Harvard, back in 1978. In fact, Dr. Shull laboratory has all the equipment for the conduction of the test in a few months. The test had also been recommended to the various salient physicists at MIT. Regrettably, despite our most respectful possible attitude, and despite our sincere desire to establish a scientific collaboration, our proposal met with an unprecedented disinterest, opposition, and interference from the part of MIT people, apparently, because we indicated the possibility of a deformation of the charge distribution (which is manifestly against the vested MIT interests).

The rumors that MIT was finally running Rauch's experiment, combined with the history of MIT opposition, then resulted in our distress. In fact, I personally do not believe that MIT can run such experiment without scientific manipulations. I am one of a few who admit it openly. However, the number of people who share this view is considerable, and growing.

Very regrettably, rather than decreasing, the rumors of MIT running Rauch's experiment under DOE support are increasing.

I beg you to clarify the situation in the best interest of all. A phone conversation on the topic in early January would have prevented our distress. Your reassurance now that you are unaware of the occurrence would be invaluable to prevent further deteriorations in a situation that MIT people have already brought to rather extremes of tension.

Sincerely,

Ruggero M. Santilli

cc. Dr. B. HILDEBRAND, DOE (only copy)

P.S. A number of theoretical articles dealing with the generalization of the theory of rotations for deformed charge distributions have appeared at the I.B.R. or are under finalization. In case you are interested in inspecting them because of possible usefulness for Rauch's proposal, please let me know. Thank you.

Department of Energy
Washington, D.C. 20545

JUL 25 1983

Professor R. M. Santilli
President
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

The Division of Nuclear Physics has completed its review of the research proposal entitled "Experimental Verification of the SU(2)-Spin Symmetry Under Strong and Electromagnetic Interactions by a Joint Austria-France-USA Collaboration."

We regret to advise you that we cannot support this research proposal.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

Enloe Ritter

Enloe T. Ritter
Director
Division of Nuclear Physics

cc:
H. Rauch
J. Summhammer
NSF, Harvey Willard

PART XXXIV:

LACK OF CONSIDERATION BY THE NATIONAL SCIENCE FOUNDATION OF AN I.B.R. COMPREHENSIVE EXPERIMENTAL–THEORETICAL–MATHEMATICAL PROPOSAL TO TEST EINSTEIN'S SPECIAL RELATIVITY UNDER STRONG INTERACTIONS

I. B. R. — 1123 —

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

Ruggero Maria Santilli, Professor of Theoretical Physics and President

May 11, 1983

Dr. E. F. INFANTE, Division Director
Mathematical and Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Infante,

Please accept the sentiments of our sincere gratitude for the courtesy of your phone call this afternoon. The possibility of an informal discussion with you has been simply invaluable for us.

After due consideration, we believe that the most appropriate form of consideration of the research conducted at our institute is a collective form incorporating our experimental, theoretical, and mathematical research.

For this reason, we have absteined from recommending to IBR principal investigators of NSF applications to apply for a reconsideration of their proposals. In fact, owing to the novelty of our program and other factors, the reviewers of mathematical proposals are not expected to reach full maturity of judgment without a joint inspection of the physical studies. Similarly, the reviewers of our physical applications may have major deficiencies in judgment without an inspection of the experimental status of the research, as well as of the underlying mathematical studies.

To our understanding, a consideration of this type goes beyond the scope of each individual NSF Division. We would therefore appreciate the courtesy of your bringing the case to the attention of the appropriate NSF Officer suitable for a joint consideration of our experimental, theoretical, and mathematical program. For this purpose, I enclose a preliminary documentation for advance consultation with tentative title:

EXPERIMENTAL, THEORETICAL, AND MATHEMATICAL STUDIES ON A CONCEIVABLE GENERALIZATION OF THE SPECIAL RELATIVITY FOR EXTENDED AND DEFORMABLE STRONGLY INTERACTING PARTICLES.

This documentation essentially consists of the combination of the following five deeply inter-related proposals previously submitted to the NSF Divisions of Physics and Mathematics in an independent way.

Experimental Part: Experimental Verification of the SU(2)–Spin Symmetry Under Strong and Electromagnetic Interactions via a joint Austria–France–U.S.A. collaboration.
Principal Investigator: H. Rauch, Professor of Physics and Director, Atominstitut, Wien, Austria

Theoretical Part: Studies on Hadronic Mechanics
Principal Investigator and Coordinator: R. M. Santilli, Professor of Physics and President, IBR

Studies of Nonpotential Scattering Theory
Principal Investigator: R. Mignani, Associate Professor of Physics, University of Rome, Italy, and Professor of Physics, IBR

Mathematical Part: Studies on Lie–admissible Algebras
Principal Investigators: H. C. Myung, Professor of Mathematics, University of Northern Iowa and IBR; R. H. Oehmke, Professor of Mathematics, University of Iowa and IBR; and M. L. Tomber, Professor of Mathematics, Michigan State University and IBR

Mathematical Studies on Reductive Lie–admissible Algebras and H–Spaces with Applications to the Geometry of Nonpotential Dynamical Systems
Principal Investigators: A. A. Sagle, Professor of Mathematics, University of Hawaii and IBR; and J. P. Holmes, Associate Professor of Mathematics, Auburn University

Upon due consultation, kindly let us know:

(a) the appropriate NSF office and officer where to submit the application;
(b) the form of application which is most appropriate to our case; and
(c) the necessary administrative guidelines to comply with, possible advice on the structure and size of the budget, and the minimal number of copies needed to apply.

The alternative forms of submissions appear to be the following.

I. SUBMIT A GROUP PROPOSAL. In this case, kindly let us know the restructuring of the enclosed advance presentation which is needed, besides the unification of the now separate budgets into one single form.

II. APPLY FOR INSTITUTIONAL SUPPORT. In case this latter form is preferable, kindly let us know the appropriate modifications of the enclosed presentation.

III. OTHER ALTERNATIVES RECOMMENDED BY NSF. In case the application for institutional support is recommended, NSF should be aware of a number of intriguing and promising possibilities.

Facilities. As you know, we have purchased a rather unique building, a charming Victorian within the compounds of Harvard University, known as The Prescott House. Our building has three floors suitable for the housing, in due time, of three divisions: one of pure mathematics, one of physics, and one of applied research.

Personnel. You should be aware that the position of Director of the IBR is vacant in the expectation of funding the Institute. According to our charter, the Director is in charge of all logistic operations, including the handling of IBR applications to Governmental Agencies, while the position of President is more similar to that of Chairman of the Board of a corporation. Since I am primarily interested in conducting research, I am rather eagerly waiting for the moment we can appoint the Director of our Institute. In case of an institutional support, we would be glad to fill up this position, as well as any needed additional one, in comformity with NSF regulations and recommendations.

Programs. In case of institutional support, you should keep in mind that the enclosed proposal would be a mere germ for future growth along lines set forth by National priorities and other NSF needs. To put is more explicitly, we would welcome the addition of any research program recom-

mended by NSF, even if completely independent from our current interests. Diversification of scientific inquiry is in fact a primary long term objective of the IBR.

Permit me to indicate that the current moment is rather unique in its gathering of qualified experimentalists, theoreticians, and mathematicians, or of novelty and unity of scientific thought. We are therefore firmly convinced that our group deserves a serious consideration by our peers. After all, our group can be easily dispersed via the prevention of financial support, but its possible future regathering would be difficult, assuming that it could be at all possible.

We do not believe that our research is the way the scientific community must necessarily follow, and we are not seeking passive acceptance by our peers. We are merely seeking constructively critical scientific interactions, mutual respect and consideration, particularly in those areas in which we share a common scientific accountability with our peers. The understanding is that the IBR has not been organized to clone the research conducted at other institutions, but rather to complement them with alternative avenues. In fact, we believe that America can be best served via a sufficiently diversified pursuit of basic advances, rather than research railed between preset narrow lines.

We believe that the past achievements of our group are sufficient to establish our credibility and qualification for further advances. In fact, during the first five years of activities, our group has produced:

- **experimentally**, rather forceful indications of the approximate character of the Lorentz symmetry in particle physics, in both its continuous and discrete parts, e.g., Rauch's measures of apparent deformation of neutrons in the intense fields of nuclei with consequential rotational–asymmetry; the measures by Slobodrian, Conzett et al on the apparent irreversibility of open nuclear reactions; and others;

- **theoretically**, we have accomplished a generalization of classical Hamiltonian mechanics into the so–called Birkhoffian mechanics; we have established the foundations of a generalization of Galilei's relativity for closed (isolated) systems of extended particles with contact/non–Hamiltonian internal forces; and we have identified the elements of a corresponding generalization of quantum mechanics (via isotopies and genotopies of the Hilbert space and enveloping algebras of operators) which appears capable of representing the experimental information indicated above; and, last but not least;

- **mathematically**, we have initiated two progressive and complementary generalizations of the very heart of contemporary mathematics, Lie's theory, called Lie–isotopic and Lie–admissible theories, which have solidly established themselves as the structural foundations of the generalized, classical and quantum mechanics indicated earlier.

As a gesture of consideration for your person, I have separately mailed to you complimentary copies of two recent monographs of our group published in 1983, that by Professor H. C. MYUNG on flexible Lie–admissible algebras (Hadronic Press), and my own monograph reviewing the construction of the Birkhoffian generalization of Hamiltonian mechanics (Springer–Verlag). I am sure you are aware of other monographs by members of our group and their advisors, such as that by Professor A. A. SAGLE on Lie algebras (Academic Press, 1973), or that by Professor G. EDER on nuclear forces (The MIT Press, 1968), or the two volumes on angular momentum by Professor L. C. BIEDENHARN (Addison–Wesley, 1981), and others.

During the first five years of operation, our group has also organized and conducted five international meetings emphasizing the interplay between experimentalists, theoreticians, and mathematicians, that resulted in the publication of some nine volumes of proceedings. Finally, our group has produced over 200 papers published in a considerable variety of mathematical and physical journals.

Stated in a nutshell, we believe that our group has established sufficient elements of credibility to attempt a generalization of quantum mechanics specifically conceived for extended and therefore deformable strongly interacting particles, with underlying generalization of conventional relativities and mathematical structures. We believe that a possibility of basic advance of this nature, with self-evident implications of scientific, economic, and military nature, deserves a chance to prove itself.

Best Personal Regards,

Ruggero Maria Santilli
President

cc: Dr. M. BARDON, Director, NSF Division of Physics
IBR Members and Advisors:
Mathematicians: Professors [redacted]

Theoreticians: Professors [redacted]

Experimentalists: Professors [redacted]

RMS/mlw

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

May 20, 1983

Dr. Ruggero M. Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Mr. Santilli:

I have received your letter of May 11, together with the "Preliminary Documentation for Advance Consultation".

As you requested, I am hereby forwarding a copy of your letter and the attached "Preliminary Documentation for Advance Consultation" to the Acting Assistant Director for Mathematical and Physical Sciences.

Please accept my gratitude in advance, for the two research monographs you have mailed to me, and which I look forward to receiving.

Sincerely yours,

E. F. Infante
Division Director
Mathematical and Computer Sciences

Preliminary Documentation for Advance Consultation

with the

NATIONAL SCIENCE FOUNDATION

on the submission of a collective proposal entitled

EXPERIMENTAL, THEORETICAL, AND MATHEMATICAL STUDIES ON A POSSIBLE GENERALIZATION OF EINSTEIN'S SPECIAL RELATIVITY FOR EXTENDED, DEFORMABLE STRONGLY INTERACTING PARTICLES

submitted by the Board of Governors

of

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street
Cambridge, Massachusetts 02138
Tel (617) 864 9859

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Paper 5: R. M. Santilli, A Possible Lie–admissible, time–asymmetric model for open nuclear reactions IBR preprint DE–82–9 (1982), submitted for publication, p. 38

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Principal Investigator and Coordinator:
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Principal Investigator and Coordinator:
R. Mignani, p. 219

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Principal Investigators and Coordinators:
H. C. Myung, R. H. Oehmke, and M. L. Tomber, p. 241

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SUMMARY

Recent theoretical and experimental studies conducted at a variety of institutions (N. Bohr, Denmark; FERMILAB, Batavia, Il; Atominstitut, Austria; I.B.R., Cambridge, MA; et al are providing increasing indications of apparent departures from the exact validity of the conventional Lorentz symmetry in a variety of sectors of physics, ranging from open nuclear reactions to unified gauge theories. The ultimate roots of the occurrence have been identified in the possibility that hadrons and their constituents, when represented as extended charge distributions, admit (generally small) deformations of their spherical symmetry under sufficiently intense external fields. This results, first, in a manifest rotational–asymmetry, and then in a consequential asymmetry of the Lorentz boosts, including a deformation of the light cone.

This application recommends the conduction of a comprehensive study of the issue with the following primary objectives: (1) Continuing the experimental measures via neutron interferometry of the apparent deformation of the charge distributions of hadrons under intense external fields; (2) Continuing the theoretical studies on a generalization of Einstein's special relativity specifically conceived for extended and, therefore, deformable particles, called Lorentz–Isotopic Relativity, of the related generalization of quantum mechanics called Hadronic Mechanics, as well as of a consequential generalization of the potential scattering theory called Nonpotential Scattering Theory; and, last but not least, (3) Continuing the studies on the generalized mathematical structures underlying the Lorentz–isotopic relativity and the hadronic mechanics, which have resulted to be generalizations of conventional formulations of Lie's theory called Lie–isotopic and Lie–admissible theories. The application also includes the study of several novel developments of quark theories, such as the construction of quarks as bound states of more elementary particles, the search for a strict confinement with identically null probability of tunnel effects of free quarks, possible generalization of spontaneous symmetry breakings via the isotopic liftings of the Hilbert space; etc.

The proposal is articulated into the following coordinated parts:

Part I: **Experiments.** Principal Investigators and coordinators: H. Rauch and J. Summhammer;

Part II: **Theoretical Studies:** Principal Investigators and coordinators: R. M. Santilli and R. Mignani; and

Part III: **Mathematical Studies:** Principal Investigators and coordinators: H. C. Myung, R. E. Oehmke, M. L. Tomber, A. A. Sagle, and J. P. Holmes

The experiments are scheduled for conduction at the ILL–Laboratory in Grenoble, France via a France–Austria–U.S.A. collaboration. The theoretical and mathematical studies are scheduled for conduction at the I.B.R. as well as at a number of other institutions in the U.S.A., and abroad.

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